2. "Note on the Embryo of *Ancistrocladus*;" by George Bentham, Esq., Pres. L.S., and J. D. Hooker, M.D., V.P.L.S. (See "Botanical Proceedings," vol. vii.)

May 25, 1863.

Anniversary Meeting.

George Bentham, Esq., President, in the Chair.

This day (the Anniversary of the birth of Linnæus having fallen on a Sunday), being the day appointed by the Charter for the Election of Council and Officers, the President opened the business of the Meeting with the following Address :—

Gentlemen,

It is with great satisfaction that I am again enabled, on meeting you at our annual gathering, to congratulate you upon the steady prosperity of our Society. Without departing from the rule laid down for the investment of a fair proportion of the compositions received for annual payments, about eighty guineas have been expended in the course of the year in the purchase and binding of books and cataloguing the library; the usual numbers of the Journal have been published; and two parts have been issued of our 4to Transactions, containing several papers which are generally admitted to be second to none in our collection in value and importance. But again I must urge you not to relax in your zealous efforts to maintain and extend the Society's pecuniary resources. Our increasing library, and the continued use of it made by the Fellows, may require additional assistance in its care; it would be desirable that the Catalogue, the transcription of which in its new form is now completed, should be printed for circulation amongst us; and there are several works which have been named to the Library Committee, and admitted by them to be desirable purchases, but which they have been obliged to defer for future consideration, as being beyond the means that could be at present allotted to the purpose. With a small but steady increase in the Society's income, with the final renunciation of the vain efforts to form a general museum, which I trust you will sanction at your next Meeting, I am confident that, under the liberal regulations established for the loan of books, or for their consultation at

Burlington House, the scientific benefit derived by our Fellows from the use of this library will be much increased from year to year.

In the wish to call your attention periodically to the present state of the science we cultivate, and to points which appear more especially to require investigation, I said a few words last year with reference chiefly to systematic and descriptive works. I would now, though with much greater diffidence of my powers of handling the subject, advert shortly to that important branch of our science which I alluded to last year under the name of Biology.

But, on the very threshold, a question arises as to whether this term can be retained in the limited sense in which I believe it was originally proposed—that of the *science of life*, *i. e.* of the phenomena of life, in contradistinction to the description and classification of living beings ; for it appears to have been recently extended to the general designation of everything relating to living beings, in contradistinction to Mineralogy and other physical sciences relating to inorganic matter, so as to include Zoology and Botany in all their branches. For the latter purpose, if a simple expression be really needed, the term Biontology, or the *science of living beings*, coined by Jeremy Bentham (although I do not find it in his published works on nomenclature), would, it appears to me, have been better, as being in direct opposition to Palæontology. I am aware, indeed, that language cannot be controlled by argument, but follows authority or fashion ; and if Biology continues to be used by Professor Huxley and other distinguished public lecturers in the most general sense, it will be the one which will be definitively attached to it. I would observe, however, that Dr. Whewell, in his classification of sciences (Novum Organum renovatum, 3rd ed. p. 140), separates Biology from the classificatory sciences ; and even Professor Huxley on several occasions appears to have the phenomena of life more especially in view when referring to Biology. Several Continental naturalists also use Biology in the limited sense above alluded to.

The science of life relates to the life of the species and to that of the individual. The life of the species includes its origin, increase, dispersion, migrations, diminution, and final extinction. This is touching on delicate ground, which I could have wished to have avoided ; but the subject has acquired that degree of importance, that no biological investigations can now be considered satisfactory which do not apply directly or indirectly to the great questions in agitation. And first I must enter a strong protest against all attempts to introduce personal feelings and moral prejudice into the discussion. It is quite unworthy of a searcher after truth, such as every naturalist professes to be, to endeavour to cast a slur on the observations of some of our most accurate botanists and zoologists, by representing them as "tinged with Darwinism;" and on the other hand, I scarcely think that due allowance is made for those who, like myself, through a long course of study of the phenomena of organic life, had been led more and more to believe in the immutability of species within certain limits, and have now felt their theories rudely shaken by the new light opened on the field by Mr. Darwin, but who cannot surrender at discretion so long as many important outworks remain contestable. Difference of opinion and eagerness of support of opposite theories act as a great incentive to the investigation of hidden facts, and thus promote the cause of science, but only so long as they are carried on without personalities and without dogmatism on the one hand or virulence on the other. Although, therefore, we cannot allow the time of our own meetings nor the pages of our publication to be devoted to the abstract discussion of any such theories, yet we should give every encouragement to the search after facts, on the one side or on the other, irrespective of what we may deem extravagant in the results which might be deduced from them.

Passing over the purely speculative part of the subject, as to how the first species or series of species originated, which appears to me to be utterly beyond human comprehension, and which, whatever theory we adopt, we must believe without material evidence, I take it to be generally admitted that, previous to the races of living beings, animal or vegetable, which now cover our globe, it was inhabited by animals and plants, as numerous, perhaps, and in many respects as diversified as those of the present day, but totally distinct from them as to species, and that the real question is how the one has been replaced by the other.

The extinction of ancient races is comparatively easy of comprehension. We all see how species gradually disappear from particular districts, and how many are becoming exceedingly rare in any country; and we have at least the Dodo amongst animals, and the St. Helena *Trochetias* amongst plants, which are now believed to be totally extinct, although we have specimens prepared from the life during the short time which has elapsed since natural-history collections were first formed. The discussion as to whether the majority of ancient species have disappeared by a similar gradual extinction, or by sudden convulsions, belongs more to geologists than to ourselves. But of the commencement of any one species we have no human record, and, as far as naturalists are concerned, we must rely entirely on circumstantial evidence.

Three different hypotheses are now more or less under discussion.

1. That the individuals now living have descended from as many common stocks as there are distinguishable varieties or geographical races or areas of dispersion, these common stocks being the result of special creations, either simultaneous or consecutive.

2. That the whole of the individuals belonging to each species (including often several more or less permanent races) are descended from one specially created common stock, from which they have gradually spread to the different parts of the world they now inhabit.

3. That the races now occupying the globe are lineally descended from those ancient and very different races which preceded them, by a gradual process of variation and extinction, according to pre-established laws still more or less actively in operation.

The first hypothesis, that of several centres of creation or origin for each species, was a favourite one among several Continental naturalists, especially botanists, a quarter or half a century since. It is, I believe, still maintained, either directly or disguised under the form of admitting distance of geographical area as a specific distinction, by Agassiz, by Carl Müller and others in Germany, as well as by some French botanists of the school of Lecoq, whose voluminous writings on geographical botany I do not find in any of our libraries, and am therefore unable to refer to. My recollection of them is, however, that the speculations they contain are founded chiefly on the geology and botany of a very limited district, and therefore of little weight with the general naturalist. And when Lyell, Forbes, and others broke down the limits previously assigned to the possibility of dispersion by the prevalent ideas of impassable geographical obstacles, the theory of separate creations became no longer necessary to account for observed phenomena : it was tacitly given up by many, and openly renounced by A. DeCandolle in his 'Géographie Botanique.' It has, however, been recently revived by some ethnologists, and especially by the President of the Ethnological Society, Mr. J. Crawfurd, in a series of papers read before that Society. His arguments however relate exclusively to Man, and they are therefore based not so much on difficulties of dispersion as upon the supposed immutability of races when not modified by hybridism, on the presumed distinctness of type in languages, and other questions of fact upon which ethnologists are by no means unanimous.

The second hypothesis, that of the independent creation of one common stock for each species now existing on the globe, has been at all times the one most generally received, acknowledged, and taught by authority. It has been supported by observations so numerous in every branch of natural history, that, when assailed, it has only been thought necessary summarily to refute any theories that may have been raised in opposition to it. This was an easy task so long as those theories were confined to mere speculations; and even now that the plausible application of undeniable facts to the establishment of serious objections, or at any rate of important exceptions, has caused a revolution in the views of many eminent naturalists, there are others who still more or less maintain the unity and original independence of existing species.

The third hypothesis, that the present species are lineally descended from preexisting ones by a process of gradual variation, was generally treated as an idle speculation, and, whenever brought forward, only broached to be almost unanimously rejected, until the publication of Mr. Darwin's 'Origin of Species.' This remarkable work has carried more or less of conviction into such minds as those of Lyell, Hooker, A. Gray, A. DeCandolle, and others whose previous arguments pointed towards a contrary direction; it has met with the most cordial adoption on the part of many eminent men who had not so committed themselves, and has excited in a large proportion of other naturalists doubts as to their previous firmest convictions. This has been effected rather by the extreme simplicity of the new principle applied to phenomena previously observed, but little attended to, and now first placed in juxtaposition, than by the discovery of any remarkable hitherto hidden phenomenon. By connecting the hereditary diversities in constitution as well as in form in the offspring of a species, with the premature destruction from external causes of the immense majority of the offspring produced, and by supposing that permanence would be given to those varieties alone whose hereditary constitution is the best suited to resist or survive these causes of destruction, Mr. Darwin has shown how specific changes *may* take place; and by the accumulation of a vast number of carefully observed or well-authenticated facts, aided by great lucidity of exposition and powers of argument, he has endeavoured to show how they do take place. His is not therefore a theory capable of proof, but "an unimpeachable example of a legitimate hypothesis" requiring verification, as defined by J. S. Mill in his excellent chapter on Hypothesis, commencing the second volume of his Logic. Mr. Darwin has proceeded with this verification so far as the present state of our knowledge permits; and we must, I think, cordially agree with the same distinguished logician, that he has "opened a path of inquiry full of promise, the results of which no one can foresee," and that it is "a wonderful feat of scientific knowledge and ingenuity to have rendered so bold a suggestion, which the first impulse of every one was to reject at once, admissible and discussible even as a conjecture" (Mill's Logic, 5th ed. vol. ii. p. 18)*.

Into the discussion itself of the points in which I should be disposed to agree or disagree with Mr. Darwin's conclusions it is not my intention, nor scarcely within the legitimate scope of our Society, to enter; but, as it is our special province to collect facts bearing upon that or any other biological hypothesis, it has been my wish to ascertain how far the discussion and verification of his views have proceeded since the publication of his work.

The reviews, analyses, and criticisms which have appeared have been numberless. The subject has been taken up in almost every periodical professing to treat occasionally or specially of scientific questions; it has been handled by most of the eminent naturalists of the day, at home and abroad, and I certainly can have no pretension to have read anything like the whole of these productions. I have, however, looked through all that have come in my way, and carefully studied those to which more weight was attached from the names of their authors, avowed or presumed, including several which Mr. Darwin kindly indicated to me as containing the best arguments opposed to his views, out of a collection of about ninety he had before him.

The majority of the reviews published on the first appearance of his work, intended mostly for the general reader, and more or less hostile to Mr. Darwin's views, dwelt more on the ultimate results he hinted at as derivable from his hypothesis, than on the observations and arguments on which he founded it. This enlisting of popular or religious feeling in the subject, successful as it had been in the case of the crude speculations of Lamarck or of the 'Vestiges of Creation,' so little supported by observation of facts, has been of little avail when opposed to the lucid juxtaposition and calm consideration of carefully observed phenomena, even though in several cases an unworthy attempt was made to depreciate the number and accuracy of these observations and to cast a general slur upon the line of argument adopted. Such criticisms are now, however, forgotten, and it is therefore useless to specialize them. Many

* Although I follow others in putting forward Mr. Darwin alone as the originator of this hypothesis, I am perfectly aware of the claims of Mr. A. R. Wallace to having independently, and at the same time, suggested the main ideas on which it rests (see Journ. Linn. Soc., Zoology, iii. p. 45); but it is Mr. Darwin alone who has methodized the subject *in all its bearings* into a tangible hypothesis.

others give an analysis more or less perfect and more or less conscientious of the book, but none have appeared to me so good or so clear in this respect as Mr. Darwin's own summaries of his several chapters.

Amongst the favourable reviews by distinguished men who have more or less adopted Mr. Darwin's views, the best I have seen are those of Dr. Edouard Claparède (who, in the Revue Germanique for August and September 1861, gives a good summary, dressed up in language very appropriate for a French reader) and, above all, of Dr. Asa Gray, reprinted from the Atlantic Monthly, under the title of 'Natural Selection not inconsistent with Natural Theology.' The chief object of this remarkable dissertation appears to have been the removal of the prejudices excited by religious views; and the question is no doubt exceedingly well put for the purpose. This is not, however, an aspect under which purely logical argument is likely to be of much avail. His opponents reply that his Natural Theology is not Religion. Our religious instructors have always interpreted or enforced great moral truths by illustrations taken from the physical world; and these illustrations, in order to have due effect, have applied rather to physical phenomena as generally understood by the community addressed, than as they might be found to be in a future and more enlightened age. Thus it is that many errors, like the astronomy of the middle ages, have at various times become incorporated with religious belief. It is therefore a dangerous thing for lay naturalists to endeavour to reconcile the facts they ascertain with religious traditions. It seems to me much wiser to leave it to theologians and churchmen gradually to make themselves so far acquainted with the progress of science as to modify accordingly that which is illustrative only in the lessons they teach, separating it in their minds from that which is essential in their doctrines.

The best objections which I have seen to Mr. Darwin's views on scientific grounds, independently of their ultimate tendency, are undoubtedly those of F. J. Pictet in the Bibliothèque Universelle de Genève for March 1860, and of Dr. H. G. Bronn in a final chapter of his German translation of the 'Origin of Species;' but especially the former, which afford a good example of the lucidity of exposition which has characterized many Genevese philosophers. Some of his objections have been taken up by Claparède, Asa Gray, and others, as well as by Darwin himself, who fully admits their force, although he believes them to be outweighed by the counter arguments by which he has tested his hypothesis.

In the consideration of the purely argumentative parts of most of these reviews there is a general logical confusion which it is often very difficult to get over; this arises from that aptness of illustration and figurative language which form one of the great charms of Mr. Darwin's works. It seems to carry his supporters, and sometimes perhaps the author himself, beyond strict logical bounds; and on the other hand, his opponents, feeling themselves led unconsciously into conclusions which they believe to be unsound, but of which they do not see the fallacy, are induced to mistrust the substantial arguments adduced. An apt illustration has a great persuasive influence, but it is no argument. The comparison of the origin of species with the origin of language, so well worked out by Lyell, is an excellent exposition of fallacies in some of the arguments opposed to Darwin's hypothesis, but it is no evidence in its favour; the two series of phenomena not being *ejusdem generis*, what is known in the one is no guide to what is unknown in the other. So it is also with the comparison of the divergence and development of varieties with that of the branches of a tree, and many others that explain his views to his audience but must not be considered as supporting them. Again, the figurative terms 'Laws of Nature,' 'Struggle for Life,' 'Natural Selection,' 'the good of the Species,' although well-devised and indispensable implements of reasoning, yet require the greatest caution in their use, from the great difficulty in keeping one's mind constantly alive to the difference between the real and figurative meaning of the words, or between their partial and general application. A social law is a command issued by, or the expressed or implied will of, an individual or a community: as this is the most familiar to us of all laws, it is exceedingly difficult, in speaking of the laws of nature (which are but observed sequences of phenomena), to separate in our mind these facts, which we can observe, from a presumed Will which we cannot investigate. Thus J. S. Mill in his treatise on Logic, besides defining the expression at the outset, finds it necessary, in order to guard against all confusion, frequently to amplify it into "laws or observed uniformities of Nature." The same personification of nature in "Natural Selection," or in explaining "the course of Nature," carries the mind rather to the presumed course of action of an intelligent being than to what it is particularly intended to convey—a generalization of observed phenomena,—besides that in all personifications it is so difficult entirely to discard all idea of human motives of action. Still more confusion and misunderstanding of Mr. Darwin's arguments appear to me to have prevailed relating to the "Struggle for Life." The direct signification of an active struggle between individuals or communities has been confounded with that passive and figurative struggle between species which he has so graphically depicted. Both are observed in nature: the former, more evident to the senses, usually takes place between individuals or communities widely distant in the scale of organized beings; the latter, between species or races the most closely allied. So again, in the criticisms of the proposition that characters selected are only such as are "good for the being," I observe that this expression is frequently supposed to be restricted to the immediate material benefit of the *individual*—without considering that it is meant to include any modification, however trifling, which of itself or through other properties with which it is in some mysterious manner connected contributes directly or indirectly to the escape from destruction or to the facilities of multiplication and dispersion of the *race*. I do not, however, wish to infer that better terms than any of the above could have been chosen, but only to insist that in their use we must never forget that they are figurative, not direct.

With regard to the process of verification or refutation of the Darwinian hypothesis by actual investigation, there has not been time yet for much progress. Mr. Darwin has not yet published those detailed evidences of his propositions which might give to fresh observers a fair starting-point, and it is only from naturalists who have long given up their minds to similar subjects that we can for some time expect anything important for or against his views. What has as yet been published of any merit, as far as I am aware, is more or less in their favour. Sir C. Lyell's already celebrated work on the Geological Evidences of the Antiquity of Man has now been duly appreciated by most naturalists. It would be presumptuous in me to dwell upon the merits of the most important portion, the exposition of the Glacial theory; for the details are purely geological, although in its general results it bears strongly on botanical and zoological theories of geographical dispersion. The accumulated evidences of man's antiquity which give the title to the work are also rather geological and ethnological than biological, their chief bearing upon the present question being the throwing so much further back in point of time the probability of the human species having the same limits of variability as in the present day. The third portion, relating directly to the Darwinian hypothesis, is chiefly argumentative, but full of considerations of great value, derived from his intimate acquaintance with geological facts connected with them. Some others may require further explanation—when, for instance (p. 446), it is assumed that aberrant and highly specialized types have the smallest possible aptitude for deviating in new directions, which does not seem quite consonant with the general law of unlimited divergence relied on by Darwin and Hooker. But these are minor points, and do not interfere with the great weight we should attach to Sir Charles's authority, and the eagerness with which we look for the decision he has come to on this great question. This is not very easy to ascertain; but the impression conveyed is, that he is generally convinced of the derivative origin of the present species, although he may leave it an open question whether there may not be exceptions, especially with regard to man. That Professor Huxley has no scruples in accepting the theory without any exception is evident from the perusal of his 'Lectures on our Knowledge of the Causes of the Phenomena of Organic Nature.'

Dr. J. D. Hooker is well known to have fully adopted Mr. Darwin's views. His general proficiency in physical sciences, added to great natural powers of observation, enabled him to derive peculiar advantage from the means of investigation afforded to him during his expeditions to the southern hemisphere and to the Himalayan regions, and to give a right direction to his laborious studies at home. His admirable essay on the Flora of Australia, bearing much upon the present question, and that on the Arctic Flora written in a great measure in direct reference to it, carry therefore deservedly very great weight, and must be studied by all who apply themselves to it. My most intimate acquaintance with him from his childhood enables me fully to appreciate both his accuracy and his inductive powers; and if I do not always agree to all the conclusions he comes to, it is because I think that, like Mr. Darwin himself, his generalizations sometimes go beyond what is strictly justified by the premises, even when the facts relied upon, which are themselves generalizations, are sufficiently established.

M. A. DeCandolle's interesting paper, entitled 'Étude sur l'Espèce,' from the Bibliothèque Universelle of Nov. 1862, shows that the study of the characters of a large group of very variable but very conspicuous plants, both recent and fossil, inclines him towards the adoption in a great measure of the hypothesis of a derivative origin. As, however, he does not admit the idea of a physiological species less indefinite in its limits than, nor even as definite as, those higher groups which I have been accustomed to consider as arbitrary, I cannot well trace out the line of reasoning he must have pursued.

Mr. H. W. Bates undertook with Mr. Wallace an expedition to the Amazons, of which one of the express objects was to collect facts towards solving the problem of the origin of species," and remained there four years with Mr. Wallace, and seven years more after his companion had left him. Since his return to England, besides the rich harvests he had made as a zoological collector, he has fully proved how eminently he was also qualified to make those observations we expect from a travelling naturalist. His valuable paper on the Heliconidæ of the Amazon Valley, in the last part of the twenty-third volume of our Transactions, aims especially at a practical illustration of changes taking place in strict conformity with the Darwinian hypothesis; and the observations connected with zoological and especially entomological biology dispersed through his 'Naturalist on the Amazons' bear frequently on those concurrences of circumstances which may influence the preservation of what we usually term accidental varieties, and the gradual extinction of typical forms.

Dr. Carpenter's great work on Foraminifera is pregnant with facts illustrative of an unbroken continuity of animal descent from the earliest geological periods where remains can be traced to the present day. Dr. Falconer's elaborate paper on Elephants, recent and fossil, in the third volume of the Natural History Review, exhibits, as far as can be ascertained from teeth alone, on the one hand, long periods of fixity of type in some of the species, and on the other hand close affinity between the extinct Elephants of Europe and some of the Indian ones which preceded or followed them. Professor O. Heer and Count G. de Saporta's independent researches on the European Flora of the Tertiary period, notwithstanding the more than doubtful determination of the majority of the fragments described, yet, in the few that can be satisfactorily ascertained, show a much closer connexion with recent vegetation than had been hitherto supposed. All descriptive works, in short, now published in which species are considered in a general point of view, with reference to their areas, geographical representatives, and affinities, recent or extinct, must furnish data of more or less importance to the investigator of historical biology.

As a general result it appears to me that the tide of opinion among philosophical naturalists is setting fast in favour of Mr. Darwin's hypothesis. The accuracy of his facts is no longer contested, and much of his reasoning must be admitted as unanswered and unanswerable. There are, I believe, few, if any, who really consider the subject, who would now deny that great, though very gradual, changes do result from those successive concurrences of phenomena figuratively called natural selection, or that there is every probability that a considerable number of what we term allied species may be descended from some common ancestor, which, if presented to us, we should pronounce specifically distinct from any of them. The great objections still urged are to the insufficiency of the data yet ascertained for the extension of the principle to all changes and to all species; and whilst many of Mr. Darwin's generalizations may be considered as adopted, there are others which many persons are disposed to refer for further proof, and many objects of research more or less relevant, indicated only by him, are still obscured from our view. But a conscientious investigation of all doubtful questions connected with the subject, if carried on by competent men under the influence of rival theories, must surely lead us many a step further in the exploration of that field towards which Mr. Darwin has made an immense stride, inasmuch as he has broken down the barriers which guarded its entrance, but which as yet is as nothing compared to the vast expanse which lies before us.

Next to the origin of species comes a question intimately connected with it, but which may be independently investigated—that of their dispersion and migrations, forming one of the most important objects of geographical zoology and botany. Among zoologists I do not find that so much has been done in the general consideration of geographical distribution as among botanists; I have not heard, at least, of any general work on the subject recently published. Our own publications show, however, that the subject is now attracting their attention. Dr. Sclater's paper on the Geographical Distribution of Birds in the Zoological portion of our Journal (vol. ii. p. 130), and on the Zoology of New Guinea in the same volume (p. 149), point to a fact of importance in the investigation of the geological history of our globe, which has been more fully worked out by Mr. Wallace in his paper on the Zoological Geography of the Malay Archipelago in the same Journal (vol. iv. p. 172), viz. the marked distinction between the Faunas of the eastern and western islands. We know not how far this may be confirmed in botany: the vegetation of the great eastern islands of Celebes and New Guinea has been but little investigated; but as yet the few Australian types found beyond the nearest islands have been gathered in the mountains of Borneo, where the Australian fauna is found to be entirely absent. Mr. A. Murray has given us some notes on the distribution of the insects of Old Calabar; and Mr. Frederick Smith, in a paper now printing for the next part of our Journal, has tabulated that of the Aculeate Hymenoptera forming part of the extensive collection of insects made by Mr. Wallace in the Indian Archipelago. We may hope that this experienced naturalist will himself methodize for us the general results deducible from his materials; and I trust that we may also induce Mr. Bates to communicate to us some of the numerous observations he has made in geographical entomology.

Geographical botany has of late years been much attended to, and generally pursued in a right direction. M. A. DeCandolle, whose great general work on the subject, published in 1855, I have had other opportunities of reporting upon, has resumed some questions concerning it in his paper above quoted, 'Étude sur l'Espèce,' in the Bibliothèque Universelle. Dr. Hooker's Essay on the Flora of Australia, already quoted, is the best exposition I am acquainted with of the geographical relations of the flora of any country, and acquires a double importance from the peculiarities of that flora. His paper on the Distribution of Arctic Plants, in the 23rd volume of our Transactions, gives us his views of the effects of climatic changes during the Glacial period on the contraction, extension, or other changes in the area of plants; and that on the Cedars of Lebanon in the 2nd volume of the Natural History Review is an excellent illustration of representative or geographical species or races. Dr. A. Gray, in his papers illustrative of the Flora of Japan, has worked out the hypothesis of an ancient connexion between Western North America and Eastern Asia at a latitude or with a climate admitting of the passage of those North American forms which appear to have travelled across Asia to Western Europe—an idea which I also had taken up in a paper on the Geographical Distribution of British Plants, read at one of your meetings in the close of the year 1858, but withdrawn from publication on the appearance of Mr. Darwin's work, which obliged me to reconsider several opinions I had given. Prof. O. Heer and Count G. de Saporta have, on the contrary, in their above-mentioned investigations of the Flora of the Tertiary period, considered that there is strong evidence of a direct and prolonged communication across the Atlantic between Europe and North America. Prof. Oliver, however, in the 2nd volume of the Natural History Review, has shown that the facts observed tend rather to confirm Asa Gray's hypothesis of the migration having taken place through Japan. And, generally speaking, botanists seem now to be aware that, although accuracy of detail is, in this, as in every other branch of science the indispensable foundation on which theories must be built, yet, that, as a science, geographical botany does not consist merely in the precise demonstration of the boundaries of a species fixed, by accidents of soil, climate, exposure, or treatment, in one minute portion of the area it occupies, but that, in order to arrive at general results of any value, the whole area must be taken into consideration, and viewed with regard to the evidences we possess of its greater or less extension in former times, to its relation to that of representative or allied species, to its bearing on the geological records of the history of the globe, and to its relation to the areas occupied by animals, especially by those insects and other smaller creatures whose lives are, as it were, interwoven with the plants they deform or destroy, or whose aid, as shown by Mr. Darwin, is essential to them in the performance of the most essential operations of their lives. I dwell the more upon this subject as it is one in which I have long felt a peculiar interest. Among the floras I have been more especially called upon to study or work out are that of Britain, which appears to be entirely derivative; that of Australia, which is, on the contrary, one of the most endemic we know, although engrafted in the north with the general tropical flora of Asia and Africa, and in the south with a vegetation which must have made the circuit of the habitable globe to its extreme northern and southern limits; that of Eastern Asia, which first suggested the route above alluded to as having been followed by American types in their progress towards Western Europe; those of West Tropical Africa and East Tropical America, at the only latitude, besides the extreme north, where the Atlantic appears not to have been at all times an impassable barrier; and that of the Mediterranean region, which in the evidences of great antiquity given by the number of endemic species of extremely limited areas, appears almost to rival South Africa, South-west Australia, or Central America. It is impossible to contemplate this and other diversities and complications in the distribution of species, types, or allied groups, wholly independent of actual circumstances of soil, climate, or facilities for or obstacles to dispersion, without speculating on former and different conditions of the globe; but no such speculations can have any value unless tested by the concurrent observations of botanists, zoologists, and geologists. The evidences upon which we can build up a history of preceding geological periods are, I repeat, so very few that we must call in aid every observation which can bear upon them, be it ever so remotely. This it is which makes me feel especially anxious that our zoological Fellows should remit to us, in preference to their separate Associations, all papers that bear upon subjects such as this, as interesting to botanists as to themselves. They may remember also that our publications, more than any others, are certain of coming to the hands of the working naturalists of the country.

Turning from the biology of the species to that of the individual, we again commence with its origin or birth. The principal laws, the universality of which is more or less under discussion, may be stated as follows:—1. That every individual owes its origin to a parent of the same species; and, 2. That every species, even those which habitually propagate by division, is endowed with the means of reproducing such individuals by the mutual action of two sets of organs—the fertilization by the male of a germ produced by the female. The principal exceptions contended for are spontaneous generation, or heterogeny, and agamic generation, including parthenogenesis.

Spontaneous generation was long a favourite speculation amongst casual observers, who could not otherwise explain the sudden appearance of thousands of little frogs or fish after a shower of rain, or of myriads of worms in organic substances in corruption. But science has gradually reduced the area of this mysterious operation till it is now nearly limited to those microscopic creatures which, in their full-grown state, often strain to their utmost limits the powers of our magnifying instruments, and whose germs, if they exist, may not be always appreciable to our senses. In this form the cause of heterogeny has been strongly taken up by some Continental observers. M. Pouchet, of Rouen, in 1859, devoted a volume to its advocacy, under the title of 'Traité de la Génération Spontanée.' M. L. Pasteur, on the other hand, came to an opposite conclusion, publishing the results of his researches in the Annales des Sciences Naturelles, Zoologie, sér. 4, vol. xvi. (1861), and more fully in the Annales de Chimie et de Physique for January 1862. In this very remarkable paper he gives the history of the doctrine up to that time, and a detailed account of a number of curious experiments, by which he appears completely to refute the idea of heterogeny in the production of microscopic animals or plants in organic matter in a state of fermentation, and brings forward a mass of evidence in favour of their origin in some of the numerous spores always floating in the air, although frequently inappreciable by our microscopes. Shortly afterwards Professor J. Wyman instituted a number of similar experiments at Boston, but, as it appears from his paper (dated May 6, 1862) in Silliman's Journal (vol. xxxiv. p. 79), not with the same results. It is probable, however, from his referring only to Pasteur's previous memoirs in the Annales des Sciences Naturelles, that he had not yet received the above-mentioned one in the Annales de Chimie, and that the heat employed for destroying the germs (M. Pasteur had ascertained that it required in certain cases 10° above boiling-water heat) was not always sufficient. At any rate, there is a great want of uniformity in the results of his different experiments; and Professor Wyman, although he appears to consider them on the whole as contradicting Pasteur's, yet refrains from expressing any decision of his own, giving only the arguments that his researches might supply to both sides. In the mean time the Académie des Sciences of Paris had proposed this subject for competition for the Alhumbert prize for 1862. M. Pouchet sent in a series of papers, which, however, he withdrew before the Commission began their examination (Comptes Rendus, vol. lv. pp. 544 & 785); and the Commission, consisting of MM. Milne-Edwards, Flourens, Brongniart, Coste, and Claude Bernard, appear to have actually had for consideration only M. Pasteur's memoir and a series of communications from MM. Joly and Musset, a summary of which these gentlemen published in the Comptes Rendus, vol. lv. pp. 487–491, professing to have repeated M. Pasteur's experiments with results diametrically opposite, and quoting also, in support of their views, Professor Wyman's experiments. The Commission, however, unanimously awarded the prize to M. Pasteur, with a high eulogium on the ability and care with which his experiments had been conducted, passing over MM. Joly and Musset's papers in silence. The Academy also further testified their opinion of M. Pasteur's merits by electing him, about the same time (December 1862), into a vacant seat among their number. Again, a further communication illustrative of the subject, read by M. Pasteur on the 9th March of the present year (Comptes Rendus, lvi. p. 416), appears, from some journals of the day, to have excited considerable sensation among his colleagues, as affording further convincing proofs of the correctness of his views.

Having scarcely risen from a perusal of M. Pasteur's papers, I was not a little surprised to see, in the Athenæum of March 28 of this year, a new form of spontaneous generation promulgated, as it were *ex cathedrâ*, in a review of Dr. Carpenter's 'Introduction to the Study of the Foraminifera.' We are told that these animals are produced by the action of a general polarizing force on the slime contained in the beds of mud or ooze at the bottom of seas, lakes, rivers, and other aggregates of waters. I see, however, no indication of the evidences on which this extraordinary statement is founded, nor can I find, on looking over the general chapters of Dr. Carpenter's work, anything to warrant a hypothesis so contrary to all conclusions derived from analogy. It is true that the extreme simplicity of structure of the Foraminifera is insisted on in a most graphic passage extracted by the reviewer, showing how those vital operations which we are accustomed to see carried on by an elaborate apparatus are performed without any special instruments whatever; yet we learn, from an equally strong passage in a subsequent page, that these creatures do perform all the functions which constitute in their aggregate the life-history of an animal; and, without strong evidence to the contrary, we have no right to conclude that this vital power is the result of those purely physical forces which produce crystallization, and is not transmitted from an organized being similar to themselves. It is to be regretted also that the anonymous reviewer should have adopted a tone so depreciatory of a work evincing such elaborate and extensive research, such powers of methodizing, and lucidity of exposition. Still less does it seem consistent with that impartiality which every reviewer is supposed to possess, that, when returning to the subject in the Athenæum of April 25, he should have cited as conclusive in favour of spontaneous generation, the authority of Pouchet, Joly, Musset, Schaaffhausen, Mantegazza, and Wyman, completely ignoring the refutation of Pasteur, considered so satisfactory by the French Academicians.

Propagation by division, in plants and in some of the lower animals, is too patent to the senses of the most casual observer to require special notice; in many plants, indeed, especially in moist, cool climates little favourable for the ripening of seeds, it seems to be almost the only mode adopted by nature, acting sometimes with extraordinary rapidity and at great distances, as in the case of the *Elodea canadensis*, which so suddenly choked up our water-channels in 1847 and 1848. The chief question connected with it is, how far it supplements or takes the place of generation; whether it can be carried on indefinitely, or whether the race thus formed ultimately dies out—once a favourite theory among gardeners, but now, I believe, generally abandoned, and I am not aware of any recent discussion on the subject. Agamic generation has, however, much occupied the attention of naturalists in both its aspects—that of generation by spores or germs in those lower orders of plants and animals where no sexual organs have been detected, and parthenogenesis, where such ova or germs of the female as ordinarily require fertilization by the male are developed into perfect beings without that aid.

The limits of reproduction by agamic spores have been gradually restricted to the very lowest forms of organization in both kingdoms; and even there analogy has led to the suspicion that sexual organs do exist, although as yet inappreciable by our means of observation. Among the most important recent researches under this head in the animal kingdom are those of M. Balbiani on the sexual phenomena of Infusoria, which received from the French Academy of Sciences the Montyon prize for 1862. Mr. Busk informs me that his papers on the subject are considered as of the highest value. They were originally published in Brown-Séquard's Journal de Physiologie, and have stamped their author as a most acute and accurate observer. The chief points of interest have been given in abstract in the 2nd volume of the 'Microscopic Journal' (pp. 176 and 285), so that they are readily accessible to English readers; and they have been, it is believed, generally adopted. Among cryptogamic plants, sexual organs are now known to exist almost universally in the higher orders, and their development and structure have been admirably illustrated in numerous papers of Hofmeister, now collected in a single volume, for an excellent translation of which we are indebted to our Botanical Secretary and experienced cryptogamist, Mr. Currey. From the notes also that he has kindly communicated to me we learn that, even here, *Marsilea* and the small-spored Lycopodiaceæ seem to require further observation. On this subject (the germination of *Marsilea*) Dr. Hanstein has published some observations which, although not altogether new, are more complete and better illustrated than those of any previous observer. In the Lichens there is absolutely no evidence of sexual organs; for Tulasne appears to have given up his notion of the sexual nature of the spermatia, which he now considers as gemmæ, and such Mr. Currey fully believes to be the case. In Fungi, Hofmeister's observations on *Tuber*, and Dr. Bary's on *Peronospora*, point to the probability of the existence of sexes; but, nevertheless, few will disagree with Tulasne, who, after noticing these observations, concludes, "ad hoc ævi non longe processit notitia nostra de Fungorum organis sexualibus, si qua sunt" (Selecta Fungorum Carpologia, p. 181).

Parthenogenesis has, in insects, been fully established by the writings of Siebold, Huxley, and others, and in Entomostraca by Mr. Lubbock, and I know of nothing very new having been published under that head. In plants Mr. Currey gave, in the Natural History Review, all that was known up to that date. Since that, very great doubts have been thrown on the accuracy of Karsten, who professes to have so readily found in female *Cœlebogynes* those pollen-bearing organs which had escaped the most searching and repeated scrutiny of R. Brown, F. Bauer, J. Smith, Radlkofer, Deecke, A. Braun, and others. A case analogous to that of the *Cœlebogyne*, noticed by Dr. T. Anderson in the last part of our Journal, is that of a female *Aberia* from S.E. Africa, which ripens its fruit in the Botanical Garden of Calcutta, in the absence, as he believes, of any male individuals or flowers. Dr. Ferd. Müller, in his 'Plants of Victoria,' vol. i. p. 89, mentions also a similar case of a female *Dodonæa* in the Botanic Garden of Melbourne. Gasparini, on the other hand, in the Rendiconto of the Academy of Naples for May 1862, gives an account of a series of observations tending to disprove the ripening of perfect seed in Hemp without fertilization, although he still believes it to take place in the Fig. A very curious discovery of Mr. Salter's, of which I hope he will give us a detailed account at our next Meeting, of the formation of pollen-grains within the ovules of a monstrous *Passiflora cœrulea,* may have some bearing on the present question. But the study of monstrous formations requires particular caution as to the generalities deducible from them; and parthenogenesis must, I think, now be considered as proved in the vegetable as well as in the animal kingdom, in so far as negative observations can be proved. In theory nothing can be said against it but that it is exceptional; but so also is propagation by division, only much less in degree.

There are many other points in the life-history of organized beings, to the awakening interest in which I had wished to call your attention, such as metamorphism, mutual dependence, dimorphism, monstrosities, hybridism, and others; but I have already attained the full limits which time assigns to these observations, and in conclusion can only trust that the few words I have said may indicate to our younger members how many and how varied are the biological subjects of promising interest open to their researches after hidden truths. At the same time my long experience may give me a right to remind them that systematic and descriptive accuracy must never be neglected, as that alone gives fixity to observations and experiments in Natural History, however careful they may be in other respects.

OBITUARY NOTICES.

The Secretary then read the following Notices of deceased Members.

Jean Baptiste Amici, a celebrated optician and astronomical observer, was born at Modena in 1786, and died there on the 5th of April of the present year. After filling the Chair of Mathematics for several years, he was appointed, in 1831, Director of the University of his native place, and subsequently became Director