D. Hanbury. Historical notes on Manna (from the Pharmaceutical Journal). Presented by the Author.

K. B. Heller. Darwin and Darwinism. Presented by Mr. Darwin.

J. Hogg. Results of Spectral Analysis (from the Monthly Microscopical Journal). Presented by the Author.

G. Jæger. The Darwinian Theory. Presented by Mr. Darwin.

——. On Spontaneous Generation and Fertilization. Zeitschr. wiss. Zool. xix.

L. Jenyns. Address of the President at the opening meeting of the Bath Natural-History and Antiquarian Field-Club. Presented by the Author.

M. Johnson. Jottings by a student of Heterogeny. Monthl. Microsc. Journ. iii.

E. Lartet and H. Christy. Reliquiæ Aquitanicæ, part 10. Presented by the executors of the late H. Christy.

J. Lea. Notes on microscopical Crystals included in some Minerals. Presented by the Author.

E. J. Lowe. Natural Phenomena and Chronology of the Seasons. Presented by the Author.

E. Parfitt. On Spontaneous Generation. Trans. Devon. Assoc. Sc. Lit. and Art, iii.

H. v. Post. Observations on Animal and Vegetable Statistics. Proc. R. Acad. Sc. Stockh. xxiv.

A. Quadri. Notes on the Darwinian Theory. Presented by Mr. Darwin.

H. Schacht. The Microscope, edited by F. Currey. Presented by the Editor.

C. J. Sundevall. The Animals of Aristotle: Mammals, Birds, Reptiles, and Insects (in German, from the Swedish). Presented by the Author, with additions.

G. Tate. The Geology, Botany, and Zoology of the neighbourhood of Alnwick, map and 2 plates. Presented by the Author.

May 24th, 1870.

Anniversary Meeting.

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

This day, the Anniversary of the birth of Linnæus, and the day appointed by the Charter for the Election of Council and Officers, the President opened the buisness of the Meeting with the following Address:—

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GENTLEMEN,

THE Reports which have been read to you on the finances and sta-
tistics of the Society may not be quite so favourable as usual. The
number of Fellows whose loss we have to deplore is about the
average; but the new ones elected have been below the numbers of
the last two or three years. There has also been a considerable
diminution in our receipts; but the falling off has been chiefly in
compositions, a larger number of our newly elected Fellows having
preferred annual payments. This reduces for the moment the
Society's income, although in the long run compositions are much
more favourable to the Fellows themselves than to the Society.
Some delay has taken place in the issue of our publications, owing
chiefly to difficulties with the authors of some papers as to the cor-
rection of the press. There is a general anxiety to commence print-
ing before the whole of a paper is written, which it is calculated
may be done whilst the first sheets are going through the press, and
without a final revision even of those sheets, which is reserved for
the proof; the result has been that some papers have been almost
rewritten at that stage, causing great delays and considerable ex-
pense, besides that the stoppage of any one paper once in the
printer's hands stays the progress of all subsequent ones. The
Council have therefore, on the report of a Committee specially
charged to inquire into the printing-arrangements, adopted regula-
tions by which they hope to obviate future delays, the main features
of which are, that no paper shall be forwarded to the printers until
the whole of it is complete, and that, while full provision is made
for the due correction of errors, alterations of the text are discoun-
tenanced, and if they occasion an extra expenditure beyond the liberal
allowance of 15s. per sheet, the excess shall be at the charge of the
respective authors. Notwithstanding, however, the above-mentioned
delays, we have now, since the commencement of the year,
besides the usual Journals, issued two parts of the Transactions,
one illustrated by twenty-five plates, the expense of which was, in
great measure, defrayed by the liberality of the distinguished author
of the paper it contained, Dr. Welwitsch; and for the other we have
equally to convey our thanks to Mr. Carruthers for the ten plates
illustrating his own paper. A third part is now complete, and will
be ready for delivery in the course of a day or two, and two or
three very important botanical papers are in hand for the next
issue. We have also been able to devote the usual sums towards
the maintenance and increase of our Library.

It had been my intention on the present occasion to carry on the sketches of the general progress of biological science which I had attempted in 1862, 1864, 1866, and 1868; but I have, from various causes, been unable to devote so much time as usual to the preparation of my Address, and feel obliged to confine myself to a few points connected with subjects of special interest to myself, which, within the last two or three years, have made considerable advances.

The most striking are, without doubt, the results obtained from the recent explorations of the deep-sea faunas, and from the investigation of the tertiary deposits of the arctic regions, which, although affecting two very different branches of natural science, I here couple together, as tending, both of them, to elucidate in a remarkable degree one of the most important among the disputed questions in biological history, the continuity of life through successive geological periods.

An excellent general sketch of the first discovery and progressive investigation of animal life at the bottom of the sea at great depths, up to the close of the season of 1868, is given by Dr. Carpenter in the Proceedings of the Royal Society, vol. xvii. n. 107, for Dec. 17, 1868. The results of the still more important expedition of the past year have as yet been only generally stated by Mr. Gwyn Jeffreys, in the numbers of ' Nature' for Dec. 2 and 9, 1869, and by Dr. Carpenter, in a lecture to the Royal Institution, published in the numbers of ' Scientific Opinion' for March 23 and 30 and April 6 and 13 of the present year; and further details, as to the Madreporaria, are given by Dr. Duncan in the Proceedings of the Royal Society, vol. xviii. no. 118, for March 24 of the present year; whilst, in North America, the chief conclusions to be drawn from these researches into the deep-sea fauna are clearly and concisely enumerated by Prof. Verrill, in the number of Silliman's Journal for January last; and some of the more detailed reports of the American explorations, by Louis and Alexander Agassiz and others, have been published in the Bulletin of the Museum of Comparative Zoology at Harvard College, nos. 6, 7, and 9 to 13. For the knowledge of the data furnished by the tertiary deposits of the arctic regions we are indebted almost exclusively to the acute observations and able elucidations of Prof. O. Heer, in his ' Flora Fossilis Arctica,' in his paper on the fossil plants collected by Mr. Whymper in North Greenland, published in the last part of the Philosophical Transactions for 1869, and in the as yet only short general sketch of the results of the Swedish Spitzbergen Expeditions, contained in the Genevese ' Bibliothèque Universelle, Archives Scientifiques,' for Dec. 1869.

It would be useless for me here to retrace, after Dr. Carpenter and Prof. Verrill, the outlines of the revolution which these marine discoveries have caused in the previously conceived theories, both as to the geographical distribution of marine animals, and the relative influences upon it of temperature and depth, and as to the actual temperature of the deep seas, or to enter into any details of the enormous additions thus made to our knowledge of the diversities of organic life; and it would be still further from my province to consider the geological conclusions to be drawn from them. My object is more especially to point out how these respective dips into the early history of marine animals and of terrestrial forests have afforded the strongest evidence we have yet obtained, that apparently unlimited permanency and total change can go on side by side, without requiring for the latter any general catastrophe that should preclude the former.

There was a time, as we learn, when our chalk-cliffs, now high and dry, were being formed at the bottom of the sea by the gradual growth and decay of Globigerinæ and the animals that fed on them —amongst others, for instance, *Rhizocrinus*, and *Terebratulina caput-serpentis*; and when, at a later period, the upheaval of the ground into an element where these animals could no longer live arrested their progress in that direction, they had already spread over an area sufficiently extensive for some part of their race to maintain itself undisturbed; and so, on from that time to the present day, by gradual dispersion or migration, in one direction or another, the same *Rhizocrinus* and *Terebratulina* have always been in possession of some genial locality, where they have continued from generation to generation, and still continue, with Globigerinæ and other animals, forming chalk at the bottom of the sea, unchanged in structural character, and rigidly conservative in habits and mode of life through the vast geological periods they have witnessed. So also there was a time when the hill-sides of Greenland and Spitzbergen, now enveloped in never-melting ice and snows, were, under a genial climate, clothed with forests, in which flourished *Taxodium distichum* (with *Sequoiæ*, *Magnoliæ*, and many others); and when at a later period these forests were destroyed by the general refrigeration, the Taxodium already occupied an area extensive enough to include some districts in which it could still live and propagate; and whatever vicissitudes it may have met with in some parts, or even in the whole, of its original area, it has, by gradual extension and migration, always found some spot where it has gone on and thrived, and continued its race from generation to generation down to the present day,

unchanged in character, and unmodified in its requirements. In both cases, the permanent animals of the deep-sea bottom and the permanent trees of the terrestrial forests have witnessed a more or less partial or complete change in the races amongst which they were commingled. Some of these primitive associates, not endowed with the same means of dispersion, and confined to their original areas, were extinguished by the geological or climatological changes, and replaced by other races amongst which the permanent ones had penetrated, or by new immigrants from other areas ; others, again, had spread like the permanent ones, but were less fitted for the new conditions in which they had become placed, and in the course of successive generations have been gradually modified by the Darwinian process of natural selection, the survival of the fittest only among their descendants. If, in after times, the upheaved sea-bottom becomes again submerged, the frozen land becomes again suited for vegetation, they are again respectively covered with marine animals or vegetable life, derived from more or less adjacent regions, and more or less different from that which they originally supported, in proportion to the lapse of time and extent of physical changes which had intervened. Thus it is that we can perfectly agree with Dr. Duncan, that " this persistence (of type and species through ages, whilst their surroundings were changed over and over again) does not indicate that there have not been sufficient physical and biological changes during its lasting to alter the face of all things enough to give geologists the right of asserting the succession of several periods ; " but we can, at the same time, feel that Dr. Carpenter is in one sense justified in the proposition, that we may be said to be still living in the Cretaceous period. The chalk formation has been going on over some part of the North Atlantic sea-bed, from its first commencement to the present day, in unbroken continuity and unchanged in character.

If once we admit as demonstrated the coexistence of indefinite permanency and of gradual or rapid change in different races in the same area, and under the same physical conditions, according to their constitutional idiosyncracies, and also that one and the same race may be permanent or more or less changing, according to local, climatological, or other physical conditions in which it may be placed, we have removed one of the great obstacles to the investigation of the history of races, the apparent want of uniformity in the laws which regulate the succession of forms. We may not only trace, with more confidence, such modifications of race through successive geological periods as Prof. Huxley has recently

exhibited to us in respect of the Horse, but we can understand more readily the absolute identity of certain species of plants inhabiting widely dissevered areas, of which the great majority of species are more or less different. One of the arguments brought forward against the community of origin of representative species in distant regions, such as temperate Europe and the Australian Alps, the Arctic Circle and Antartic America, the Eastern United States and Japan respectively—an argument which long appeared to me to have considerable weight—was this:—if disseverance and subsequent isolation result necessarily in a gradual modification by natural selection, how is it that when all are subjected to the same influences, the descendants of some races have become almost generically distinct in the two regions, whilst others are universally acknowledged as congeners, but specifically distinct, and others again are only slight varieties or have remained absolutely identical? To this we can now reply, with some confidence, that there is no more absolute uniformity in the results of natural selection than in any other of the phenomena of life. External influences act differently upon different constitutions. Were we to remove the whole flora and fauna of a country to a distant region, or, what comes to the same thing, change the external conditions of that flora and fauna, as to climate, physical influences, natural enemies, or other causes of destruction, means of protection, &c., we should now be taught to expect that some of the individual races would at once perish; others, more or less affected, might continue through several generations, but with decreasing vigour, and, in the course of years or ages, gradually die out, to be replaced by more vigorous neighbours or invaders. Others, again, might see amongst their numerous and ever varying offspring some few slightly modified, so as to be better suited for the new order of things; and experience has repeatedly shown that the change once begun may go on increasing through successive generations and a permanent representative species may be formed. And some few races might find themselves quite as happy and vigorous under their new circumstances as under the old, and might go on as before, unchanged and unchanging.

Taking into consideration the new lights that have been thrown upon these subjects by the above investigations and by the numerous observations called forth by the development of the great Darwinian theories, amongst which I may include a few points adverted to in a paper on Cassia which I laid before you last year, but which a press of matter has prevented our yet sending to press, it appears to me that, in plants at least, we may almost watch, as it were, the

process of specific change actually going on; or at least we may observe different races now living in different stages of progress, from the slight local variation to the distinct species and genus. As a first step we may take, for instance, those races which are regarded by the majority of botanists as very variable species, such as *Rubus fruticosus, Rosa canina, Zornia diphylla, Cassia mimosoides*, &c. We shall find in each some one form, which we call typical, generally prevalent over the greater part of the area of the race, whilst others, more or less aberrant, are more or less restricted to particular localities, the same varieties not occurring in disconnected stations with precisely the same combinations of character and in the same proportions; local and representative varieties and subspecies are being formed, but have not yet obtained sufficient advantages to prevent their being kept in check by their intercommunication (and, probably, cross-breeding) with their more robust type. The British Batologist or Rhodologist transported to the south of France or to Hungary will still find one, or perhaps two or three forms of Bramble and Dog-rose with which he is familiar; but if he wishes to discriminate the thirty or forty varieties or subspecies upon which he had spent so much labour and acuteness at home, he will find that he must recommence with a series of forms and combinations of characters quite new to him. The species is still the same; the varieties are changed. As examples of what we may call a second stage in the formation of species, we may adduce such plants as *Pelargonium australe* or *grossularoides* and *Nicotiana suaveolens* or *angustifolia*, to which I alluded in the above-mentioned paper on *Cassia*. Here we have one race, of no higher than specific grade in the ordinary acceptance of the term, inhabiting two countries which have long been widely dissevered (in the one case South Africa and Australia, in the other Chili and Australia), which, if originally introduced by accident from one country to the other, have been so at a time so remote as thoroughly to have acquired an indigenous character in both; in both they are widely spread and highly diversified; but amongst all their varieties one form only is identical in the two countries (*Pelargonium australe*, var. *erodioides*, and *P. grossularioides*, var. *anceps*; *Nicotiana suaveolens*, var. *angustifolia*, and *N. angustifolia*, var. *acuminata*), and that so comparatively a rare one that it may be regarded as being in the course of extinction; whilst all other varieties, some of them very numerous in individuals over extended areas, and all connected by nice gradations, diverge nevertheless in the two countries in different directions and with different combinations of characters, no two of them

growing in the two countries being at all connected but through the medium of that one which is still common to both. When that shall have expired, the distinct species may be considered established. A still further advance in specific change is exemplified in *Cassia* itself, in which I have shown that no less than eight or nine different modifications of type, sectional and subsectional, are common to South America, tropical Africa, and Australia, but without any specific or, at least, subspecific identity, except perhaps in a few cases where a more modern interchange may be presumed. The original common specific types are extinct, the species have risen into sections. Common types of a still higher order have disappeared in the case of Proteaceæ, an order so perfectly natural and so clearly defined that we cannot refrain from speculating on the community of origin of the African and of the Australian races, both exceedingly numerous and reducible to definite groups—large and small well-marked genera in both countries, and yet not a single genus common to the two; not only the species, but the genera themselves have become geographical. As in the case of the varieties of *Pelargonium* and *Nicotiana*, so in that of the species of *Cassia* and of the genera of Proteaceæ, it is not to be denied that precisely similar modifications of character are observed in the two countries; but these modifications are differently combined, the changes in the organs are differently correlated. In Asiatico-African *Chamæcristæ* a tendency to a particular change in the venation of the leaflet is accompanied by a certain change in the petiolar gland; in America the same change in the gland is correlated with a different alteration in the venation. In Australian Proteaceæ the glands of the torus are constantly deficient with a certain inflorescence (cones with imbricate scales) which is always accompanied by them in Africa.

In selecting the above instances for illustration of what we may, without much strain upon the imagination, suppose to be cases of progressive change in races, it is not that they are isolated cases or exceptionally appropriate; for innumerable similar ones might be adduced. In the course of the detailed examination I have had successively to make of the floras of Europe, N.W. America, Tropical America, Tropical Africa, China, and Australia, I have everywhere observed that community of general type, in regions now dissevered, is, when once varied, accompanied by more or less of divergence in more special characters in different directions in the different countries.

With regard to the succession of races which have undergone a

complete specific change through successive geological periods, we
have not in plants, as far as I am aware, any such cases of "true
linear types or forms which are intermediate between others because
they stand in a direct genetic relation to them" as Professor Huxley
appears to have made out in favour of the pedigree of the Horse in
his last Anniversary Address to the Geological Society. And I may,
in regard to plants, repeat with still greater emphasis his dictum,
that " it is no easy matter to find clear and unmistakable evidence
of filiation among fossil animals; for in order that such evidence
should be quite satisfactory, it is necessary that we should be ac-
quainted 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." The difficulty is much greater
in the case of fossil plants; for instead of bones, teeth, or shells,
portions of internal or external skeletons, the parts preserved to us
from the tertiary period are generally those least indicative of struc-
tural organization. Mr. Carruthers has recently (Geological Maga-
zine, April and July 1869, and Journal of the Geological Society,
August 1869) adduced satisfactory evidence of the close affinity of
Sigillaria and the allied genera of the coal-period with the living
Lycopodiaceæ, formerly suggested by Dr. Hooker; but, as he informs
me, no connecting links, no specimens, indeed, of the whole Order,
have as yet been found in any of the intermediate cretaceous or ter-
tiary deposits. Among the latter, the presence of numerous vegetable
types, to which we may plausibly refer as to the ancestors of living
races, is established upon unimpeachable data; but I have been
unable to find that a single case of authentic pedigree, as succes-
sively altered from the cretaceous through the abundant deposits
of the eocene and miocene period to the living races, has been as
yet as satisfactorily made out as that of the absolute identity of
Taxodium and others above mentioned, although I feel very little
doubt that such a one will yet be traced when our palæontologists
shall have ceased to confound and reason alike upon the best-proved
facts and the wildest guesses. Our late distinguished foreign
Member, Professor Unger, whose loss we have had so recently to
deplore, had indeed, shortly before his death, published, under the
name of ' Geologie der europäischen Waldbäume, part 1. Laubhölzer,'
no less than twelve tabular pedigrees of European forest races; but
it seems to me that in this, as in another of the same eminent
palæontologist's papers to which I shall presently have to refer, his
speculations have been deduced much more freely from conjectures

than from facts. There is no doubt that the presence of closely allied representatives of our Beeches, Birches, Alders, Oaks, Limes, &c. in the tertiary deposits of Central and Southern Europe is fully proved by inflorescences and fruits as well as leaves; but how can we establish the successive changes of character in a race when we have only the inflorescence of one period, the fruit of another, and the leaf of a third? I do not find a single case in which all three have been found in more than one stage; and by far the great majority of these fossil species are established on the authority of detached leaves or fragments of leaves alone.

Now let us consider for a moment what place a leaf really holds in systematic botany. Would any experienced systematic botanist, however acute, on the sole examination of an unknown leaf, presume to determine, not only its natural order and genus, but its precise characters as an unpublished species? It is true that monographists have sometimes published new species founded on specimens without flower or fruit, which from collateral circumstances of habitat, collector's notes, general resemblance, &c. they had good reason to believe really belonged to the genus they were occupied with; but then they had the advantage of ascertaining the general *facies* derived from insertion, relative position, presence or absence of stipular appendages, &c., besides the data supplied by the branch itself. And with all these aids, even the elder De Candolle, than whom no botanist was more sagacious in judging of a genus from general aspect, proved to have been in several instances far wrong in the genus, and even Order, to which he had attributed species described from leaf-specimens only. Palæontologists, on the other hand, have, in the majority of these tertiary deposits, had nothing to work upon but detached leaves or fragments of leaves, exhibiting only outward form, venation, and, to a certain degree, epidermal structure, all of which characters may be referred to that class which Professor Flower, in his introductory lecture at the Royal College of Surgeons in February last, has so aptly designated as *adaptive*, in contradistinction to essential and fundamental characters. They may, when taken in conjunction with relative individual abundance, assist in forming a general idea of the aspect of vegetation, and thus give some clue to certain physical conditions of the country; but they alone can afford no indication of genetic affinity, or consequently of origin or successive geographical distribution.

Lesquereux, in speaking of cretaceous "species, or rather forms of leaves," observes, in a note to his paper on Fossil Plants from Nebraska (Silliman's Journal, vol. xlvi. July 1868, p. 103), that "it

is well understood that when the word *species* is used in an examination of fossil plants, it is not taken in its precise sense; for indeed no *species* can be established from leaves or mere fragments of leaves. But as palæontologists have to recognize these forms described and figured, to compare them and use them for reference, it is necessary to affix to them specific names, and therefore to consider them as species." But the investigators of the tertiary floras of Central and Southern Europe have acquired the habit, not only of neglecting this distinction and naming and treating these forms of leaves as species equivalent to those established on living plants, but of founding upon them theories which must fall to the ground if such specific determination proves inaccurate. Nothing can be more satisfactory than such determinations as that of *Podogonium*, for instance, which Professor Heer has succeeded in proving, by numerous specimens of leaves, fruits, and even flowers, some of them still attached to the branches, which I had myself the pleasure of inspecting last summer under the friendly guidance of the distinguished Professor himself. This genus of Cæsalpinieæ, from its evident affinity with *Peltogyne, Tamarindus*, and others now scattered over the warmer regions of America and Africa, and more sparingly in Asia, tells a tale of much significance as to the physico-geographical relations of the Swiss tertiary vegetation, confirmed as it is by some other, equally or almost equally convincing examples. But the case appears to me to be far different with the theory so vividly expounded by Professor Unger in 1861 in his Address entitled " Neu Holland in Europa." This theory, now generally admitted, seems to me to be established on some such reasoning as this :—There are in the tertiary deposits in Europe, and especially in the earlier ones, a number of leaves that look like those of Proteaceæ ; Proteaceæ are a distinguishing feature in Australian vegetation ; *ergo*, European vegetation had in those times much of an Australian type derived from a direct land communication with that distant region.

This conviction, that Proteaceæ belonging to Australian genera were numerous in Europe in eocene times, is indeed regarded by palæontologists as one of the best-proved of their facts. They enumerate nearly one hundred tertiary species, and most of them with such absolute confidence that it would seem the height of presumption for so inexperienced a palæontologist as myself to express any doubt on the subject. And yet, although the remains of the tertiary vegetation are far too scanty to assert that Proteaceæ did not form part of it, I have no hesitation in stating that I do not believe that

a single specimen has been found that a modern systematic bota-
nist would admit to be Proteaceous unless it had been received
from a country where Proteaceæ were otherwise known to exist.
And, on other grounds, I should be most unwilling to believe that
any of the great Australian branches of the Order ever reached
Europe. As this is a statement requiring much more than mere
assertion on my part, I shall beg to enter into some detail, com-
mencing with a short summary of my grounds of disbelief in Eu-
ropean tertiary Proteaceæ, and then examining into the supposed
evidences of their existence.

The analysis and detailed descriptions I have had to make within
the last few months of between five and six hundred Proteaceæ,
and consequent investigation of their affinities and distribution,
have shown that the Order as a whole is one of the most distinct
and most clearly defined amongst Phanerogams. I do not know of a
single plant intermediate in structure between that and the nearest
allied Orders, which I cannot say of any other of the large Orders
I have worked upon. There is, moreover, especially amongst the
Nucamentaceæ, a remarkable definiteness in the majority of genera,
without intermediate species, whilst the whole Order exhibits the
greatest uniformity in some of its most essential characters, derived
from the arrangement of the floral organs and the structure of the
ovary and embryo, accompanied by a truly protean foliage. All
this points, in my mind, to unity of origin, very great antiquity, and
long isolation in early times. And the species themselves appear
to be for the most part constitutionally endowed with what I de-
signated in my last year's Address as individual durability rather
than with rapidity of propagation. The Order may be divided into
about five principal groups, more or less definite in character, but
very different in geographical distribution. First, the Nucamen-
taceæ (from which I would exclude *Andripetalum* and *Guevina*),
which we may suppose to be the most ancient, and perhaps the only
one in existence when Proteaceæ inhabited some land in direct
communication, either simultaneously or consecutively, with Extra-
tropical Africa and Australia; for it is the only group now repre-
sented in the former. It is preeminently endowed with the charac-
teristic definiteness and durability of the Order. It is very natural
as a whole; it has about 250 species in eleven distinct African
genera, and nearly 200 species in twelve equally distinct Australian
genera, no single genus common to the two countries, and the
species mostly abundant in individuals, in very restricted localities.
In both countries it is chiefly confined to the south. Africa sends

only one or two species northward as far as Abyssinia.   The Aus-
tralian portion has extended to New Zealand, where it has left a
single species, now quite differentiated from the Australian ones ;
very few species (not half a dozen) have reached Tropical Australia ;
and, if ever it extended further, no representatives have yet been
discovered in America, Asia, or even in New Caledonia.   The four
remaining groups, constituting the Folliculares, must have all been
formed since the isolation from Africa.   1. Banksieæ, two genera,
with above 100 species, have the type of distribution of the Austra-
lian Nucamentaceæ, chiefly southern, local, and abundant in indivi-
duals, with three or four species penetrating into the tropics, but
none beyond Australia.   2. Grevilleeæ, in which the genera are
somewhat less definite and the distribution more extended, have
above 300 species in about eight genera, of which the greater por-
tion are still southern and local, but yet a considerable number are
tropical, and a few extend to New Caledonia, although none beyond
that.   3. Embothrieæ, with about twenty-five species in half a
dozen genera, form part of that southern, chiefly mountain, flora
which extends from Tasmania and Victoria to New Zealand, Ant-
arctic and Chilian America, a flora which comprises many species
which we might imagine to have spread from the northern hemi-
sphere down the Andes to Antarctic America, and thence to New
Zealand and Australia, whilst others may have extended in a con-
trary direction ; and amongst these we may conjecturally include
the Embothrieæ, which in America are not found further north than
Chile ; whilst in Australia, although chiefly from the southern and
eastern mountains, two or three species are northern, and one or
two more are found in New Caledonia, but none in the Indian
archipelago, nor in Continental Asia.   4. We have lastly the tro-
pical form of Proteaceæ, the Helicieæ, which are but a slight modi-
fication in two different directions (modifications either of the flower
or of the fruit) of the *Grevillea* type, probably of a comparatively
recent date, and, although now widely spread over South America
and Asia, have nevertheless left representatives in the original
Grevillea-regions of Australia.   There are nearly 100 species in
about eight genera, almost all tropical or subtropical; three small
genera are exclusively Australian ; *Helicia* itself is Asiatic, chiefly
from the Archipelago, extending, in four species, to Tropical Aus-
tralia, in one or two species to New Caledonia, in two or three
northward to the mountains of Bengal and Sikkim, and in one
species even to Japan.   Two American genera, with about forty
species, are represented in New Caledonia by one genuine species of

each and one of an allied genus or section, and in Tropical Aus-
tralia by one species showing still the Australian connexion; and
two small genera are, as far as hitherto known, exclusively Ame-
rican, and may have been there differentiated. No Helicieæ, nor
indeed as already observed, any Folliculares, have hitherto been
discovered in Africa. If, therefore, Proteaceæ have really ever ex-
tended to Europe, it would naturally be in this Helicioid group that
we should seek for them. As far, however, as I can learn, among
the supposed century of European Proteaceæ, there is only one
which palæontologists refer to it, the *Helicia sotzkiana* of Ettings-
hausen, founded on a single leaf, which Ettingshausen himself
admits to bear much resemblance to the leaves of about twenty
genera in thirteen different families; and, upon much consideration,
he thinks it rather more like an *Helicia* than any thing else, and
therefore definitively names it as such—a decision in which it is
difficult to concur.

In answer to the above negative considerations, which, after all,
lead to presumption only, we are told that we have positive evidence
of the existence of Proteaceæ in the miocene, and still more in the
eocene formations of Europe, in leaves, fruits, and seeds. As none
of these have been found attached to the branches, nor even in
sufficiently abundant proximity to be matched with any thing like
certainty, we must take the three separately. First, as to seeds,
those referred by palæontologists to Proteaceæ are winged and
samaroid, some of them probably real seeds, shaped, without doubt,
like those of some *Hakeæ* and *Embothria*, but quite as much like
those of several Coniferæ, or of certain genera of Meliaceæ, Sapin-
daceæ, and various other Dicotyledonous Orders, there being no
evidence of internal structure, conformation of the embryo, &c., by
which alone these several samaroid seeds can be distinguished.
Moreover those figured by Ettingshausen in his paper entitled " Die
Proteaceen der Vorwelt " (Proc. Imp. Acad. Sc. Vienna, lvii. 711,
t. xxxi. f. 11, 12, 14, 15, and δ) have a venation of the wing very
different from that of any Proteaceæ I have seen, and much more
like that of a real samara of an ash. Next, as to fruits, the hard
follicles or nuts of Proteaceæ are as remarkable for their durability
as the capsules of so many Australian Myrtaceæ; and we should be
led to expect that, where Proteaceous remains are abundant, they
should include a fair proportion of fruits, as is the case with the
Conifers, Leguminosæ, &c. which have been undoubtedly identified.
These supposed Proteaceous fruits in the tertiary deposits, however,
are exceedingly rare. The only ones I have seen figured are:—

1, a supposed *Embothrium*-fruit figured by Heer in his Tertiary flora of Switzerland, t. xcvii. f. 30, an outline impression, with a deficiency in the upper portion, and without indication of internal structure ; if this deficiency were filled up, and the seeds inserted, as in the imaginary restoration, f. 31 (for which I see no warrant, and in which the seeds are in the wrong position), it would be something like, but to my eyes not much like, the follicle of an *Embothrium*, and quite as much like what Ettingshausen figures (t. xxxi. f. 5) as the veinless leaf of a *Lambertia* ; and, 2, the supposed *Persoonia* and *Cenarrhenes* drupes figured by Ettingshausen (t. xxx.) : the former, in the absence of all indication of structure, are quite as good, if not better, representations of young fruits of *Ilex*, *Myoporum*, and many others, as of *Persoonia* ; and where, in figures *c* and *d* of the same plate recent (unripe) Persoonia fruits are inserted, for comparison with the fossil figures β, γ, and δ, it appears to me that in the latter the long point is the pedicel, and the short point the style, whilst in the former, on the contrary, the short point is the pedicel, and the long one the style.   To suppose that fig. 5 of the same plant represents the fruit of a *Cenarrhenes*, which, as far as known, has always an obliquely globular drupe, requires indeed a great strain upon the imagination.   I can find no other fossil Proteaceous fruit figured or described.

Lastly, with regard to leaves, necessarily the main stay of palæontologists, I must admit that there is a certain general *facies* in the foliage of this Order that enables us in most, but not in all cases to refer to it with tolerable accuracy leafy specimens known to have come from a Proteaceous country, even without flowers or fruit ; but as to detached leaves, I do not know of a single one which, in outline or venation, is exclusively characteristic of the Order, or of any one of its genera.   If we know the genus and section of a specimen, we may determine its species by the venation ; and we may sometimes fairly guess at its genus if we know it to be Proteaceous ; but that is all.   Outline is remarkably variable in many species of *Grevillea* and others ; and venation is not always constant even on the same individual.   But then we are told, with the greatest confidence, that the structure of the stomata in these fossil leaves, as revealed by the microscope, proves them beyond all doubt to be Proteaceous.   In reply to that, I can only refer to the highest authority on these curious organs, Hugo Mohl, who, in a very careful and elaborate memoir specially devoted to the stomata of Proteaceæ, has the following passage (Vermischte Schriften, p. 248) :—" Striking as is the above-described structure of the

stomata in Proteaceæ, we should nevertheless not be justified in regarding this as a peculiarity of this family; for all the variations which we meet with in the structure of the stomata in Proteaceæ are also to be found in plants belonging to widely distant Orders."

From the above considerations, I cannot resist the opinion that all presumptive evidence is against European Proteaceæ, and that all direct evidence adduced in their favour has broken down upon cross-examination. And however much these eocene leaves may assume a general character, which may be more frequent in Australia (in Proteaceæ and other Orders) than elsewhere, all that this would prove would be, not any genetic affinity with Australian races, but some similarity of causes producing similarity of adaptive characters.

Another series of conclusions drawn by palæontologists from their recent discoveries, which appears to me to have been carried too far, relates to the region where a given species originated. The theory that every race (whether species or group of species derived from a single one) originated in a single individual, and consequently in one spot, from which it has gradually spread, is a necessary consequence of the adoption of Darwinian views; and when Mr. R. Brown ("On the Geographical Distribution of Conifers," Trans. Bot. Soc. Edinb. x. p. 195) sneers at my having qualified it as a perfect delusion, he must have totally misunderstood, or rather misread the passage he refers to in my last year's Address. The expression is there specially applied to the idea of general centres of creation whence the whole flora of a region has gradually spread, in contradistinction to the presumed origin of individual races in a single spot, which is there as distinctly admitted. The determination of where that spot is for any individual race, is a far more complicated question than either geographical botanists or palæontologists seem to suppose. "Every vegetable species," as well observed by Professor Heer, "has its separate history," and requires a very careful comparison of all the conclusions deducible as well from present distribution as from ancient remains. The very important fact that *Taxodium distichum, Sequoiæ, Magnoliæ, Salisburia,* &c. existed in Spitsberg in miocene times, so satisfactorily proved by Heer, shows that the vegetation of that country then comprised species and genera now characteristic of North America; but it appears to me that the only conclusion to be drawn (independently of climate and geology) is, that the area of these species and genera had extended continuously from the one country to the other, either at some one time, or during successive periods. The proposition that

"Spitsberg appears to have been the focus of distribution of *Taxodium distichum*," because an accidental preservation of its remains shows that it existed there in the lower miocene period, would require at least to be in some measure confirmed by a knowledge of the flora of the same and preceding periods over the remainder of its present area, the greater part of which flora, however, is totally annihilated and for ever concealed from us. The fact that *Pinus abies* existed in Spitsberg in miocene times, and that no trace of it has been found in the abundant tertiary remains of Central Europe is very instructive. It might show that that tree was of more recent introduction into the latter than the former country ; but it cannot prove that it was not still earlier in some other region, whence it may have spread successively into both territories, still less that its course of dissemination was directly from Spitsberg over Northern and Central Europe. Moreover the determination of *Pinus abies* is not so convincing as that of the *Taxodium*, resting as it does, if I correctly understand Prof. Heer's expression, on detached seeds and leaves, with a few scales of one cone, and may require further confirmation.

In the above observations it is very far from my wish to detract from the great value of Professor Heer's researches. Interested as I have been in the investigation of the history of races of plants, I have deeply felt my general ignorance of palæontology, and consequent want of means of checking any conclusions I may have drawn from present vegetation by any knowledge of that which preceded it, and the impossibility at my time of life of entering into any detailed course of study of fossils. Like many other recent botanists, I am obliged to avail myself of the general results of the labours of palæontologists ; and if I have here ventured on a few criticisms, it is only as a justification of the hope that they may in some measure distinguish proved facts from vague guesses, in order that we may know how far reliance is to be placed on their conclusions.

Spontaneous generation, or Heterogeny, is a question which continues to excite much interest. It has been the subject of detailed memoirs, of violent controversies, and of popular articles in this country, and still more on the Continent ; but the solution of the problems still involved in doubt does not seem to me to have much advanced since I alluded to the opposing theories of Pasteur and Pouchet in my Address of 1863. The present state of the case appears to me to be this: in the higher Orders of Animals every individual is known to proceed from a similar parent after sexual pairing ; in most plants, and some of the lower animals, besides the

result of that sexual pairing which they all are endowed with, re-production from the parent may take place by the separation of buds, by division, or sometimes by parthenogenesis; in some of the lower Cryptogams, the first stage in which the new beings are separated from the parent is that of spores termed agamic, from the belief that they never require previous sexual pairing, although the range of these agamic races is being gradually restricted, a remark-able advance having been recently made in this direction by Prings-heim in his paper on the pairing of the Zoospores in *Pandorina* and *Eudorina*. In all the above cases, in all organized beings which in their earliest stages are appreciable through our instruments, every individual has been proved to have proceeded in some stage or another from a similarly organized parent. But there are cases where living beings, Vibrios, Bacteria, &c., first appear under the microscope in a fully formed state, in decaying organic substances in which no presence of a parent could be detected or supposed. Three different theories have been put forward to account for their presence: 1st, that they are suddenly created out of nothing, or out of purely inorganic elements, which is perhaps the true meaning disguised under the name of spontaneous generation, a theory not susceptible of argument, and therefore rejected by most naturalists as absurd; 2ndly, that they are the result of the transformation of the particles of the organic substances in which they are found, without any action of parent Vibrios or Bacteria; and this ap-pears to be what is specially termed Heterogeny; 3rdly, that there existed in these organic substances germs which had proceeded from parent Vibrios and Bacteria, but too minute for optical apprecia-tion, and that their generation was therefore normal. The sup-porters of Heterogeny rely on the impossibility of accounting for the appearance of the Vibrios and Bacteriums in any other manner; for they say that although you treat the medium by heat in an hermeti-cally closed vessel in such a manner as to destroy all germs, and intercept all access, still these beings appear. This their opponents deny, if the experiments are conducted with proper care. So it was seven years ago; and so it is still, although the experiments have been frequently repeated in this country, in France, and in North America, almost always with varying results. All reasoning by analogy is still in favour of reproduction from a parent; but Heterogeny has of late acquired partisans, especially in Germany, among those who are prepared to break down the barriers which separate living beings from inorganic bodies.

Brown's celebrated theory of the gymnospermy of Conifers and

allied Orders has been of late the subject of keen controversy.
Objected to by Baillon, Parlatore, and others, it had been strongly
supported by Caspary, Eichler, and, lastly, by Hooker in his im-
portant Memoir on *Welwitschia,* published in our Transactions in
1863. There the question seemed to rest till last year, when two
detailed papers appeared, the one contesting, the other advo-
cating the theory.   The most elaborate is without doubt that
of Gustav Sperk in the Memoirs of the Imperial Academy of
Sciences of St. Petersburg.   He gives a very fair *résumé* of all that
had been published on the subject, and proceeds to record in detail
his own observations on the structure and anatomy of the flower in
a considerable number of Coniferæ, of *Ephedra alata, Gnetum lati-
folium,* and two species of *Cycas,* illustrated by well-executed ana-
lytical figures.   He endeavours to prove, chiefly by their anatomy
and development, that the coating which encloses the nucleus is
carpellary, not ovular, of independent origin, always free, and often
earlier developed than the nucleus,—that what is wanting in gymno-
sperms is not the ovarium or carpellary envelope, but the ovular
coating,—that these plants are in fact gymnosperms in the sense of
having naked nuclei and embryo-sacs, not naked ovules.

P. Van Tieghem, on the contrary, in the ' Annales des Sciences
Naturelles,' ser. 5, vol. x., considers the gymnospermy of the ovules
of Conifers to be proved by the anatomical structure of the organs
on which they rest.   He says that, as in normal Dicotyledons, the
ovules are developed from, and continuous with, the margins of
carpellary leaves, but these carpellary leaves are open, variously
or imperfectly developed, and constitute solitary leaves on a secon-
dary branch in the axil of the subtending bract, this secondary
branch being arrested in its development, and the carpellary leaf
facing the bract; the paper is illustrated by a large number of
diagrams.   These two Memoirs, published simultaneously at St.
Petersburg and at Paris, contain of course no reference to each
other.   How far each author may or may not have proved his case,
I cannot now take upon myself to inquire into.   Neither of them
appears to have had any knowledge of the views of Professor Oliver,
who in his review of Hooker's Memoir on *Welwitschia* (Nat. Hist.
Review, 1863) suggests the analogy of the disputed organ with the
axial developments known under the name of floral disks.   Both
writers, however, confirm the anomalous structure of the flower in this
great class of plants, and the position of the plants themselves in many
respects intermediate between the higher Cryptogams and Dicotyle-
dons, their connexion with the former being clearly shown by the

researches of Carruthers and other palæontologists, and with Dicotyle-
dons through *Welwitschia* by Hooker in his above-mentioned Memoir.

Teratology is a subject which has again risen into importance as
aiding in the history of the variations worked upon by natural selec-
tion in the formation of species.   There had always been a tendency
to attribute monsters and prodigies, whether in the organic or the in-
organic world, to an infraction of the laws by which natural pheno-
mena are regulated, by the immediate interposition *ad hoc* of a
supreme will for temporary motives inscrutable to man, in which all
that the man of science was called upon to do was to establish their
authenticity, and detail their abnormities.   This, however, was
considered by D'Alembert sufficient to constitute Teratology as one
of the great branches of Natural History taken in its most extended
sense ; for in his once celebrated ' Système Figuré des Connaissances
Humaines,' *Histoire Naturelle* has three great branches :—*Uniformité
de la Nature,* or the study of the laws which govern the organic or
inorganic world, terrestrial and celestial ; *Ecarts de la Nature,* the
science of prodigies and monsters ; and *Usages de la Nature,* or arts
and manufactures.   Jeremy Bentham, in his ' Essay on Nomenclature
and Classification,' of which I published a French edition now nearly
half a century since, strongly criticised such a classification, " by
which a middle-sized man is placed in one niche, a tall man and a
short man together in another"*.   Mr. Galton, however, in his re-
cently published interesting researches on Hereditary Genius, shows
us, after Quetelet, that even in this respect the laws which govern
the deviations from the average height of man, both above and below
that average, are uniform under similar conditions, and may well be
studied together.   We may not, indeed, with D'Alembert, combine
the history of animal and vegetable monstrosities with that of mineral
monsters and celestial prodigies (whatever these may be) ; but the
course which Biology has taken in the last few years has shown the
necessity of accurately investigating in each branch all observed
departures from what appears to be the ordinary course, before the
real laws of that ordinary course can be ascertained.   A work,
therefore, in which these observed aberrations are carefully collected,
tested, and methodized, cannot fail to be of great use to the physio-
logist; and such a work with regard to plants (the want of which,
brought down to the present state of the science, I alluded to in
my Address of 1864) has now been provided for us by Dr. Masters,
in his ' Vegetable Teratology '—a work which we should especially

* Essay on Nomenclature and Classification, or *Chrestomathia*, part. ii. 1817,
p. 157 ; French edition, 1823, p. 48.

like to see deposited in local libraries at home and abroad, to which observers resident in the country could have ready access. Monstrosities or deviations from the ordinary forms in plants are comparatively rare and evanescent; they can be best observed in their fresh state, and often require watching in the course of their development. Country residents have the best means of doing so; and to them it is very important to have a systematic work at hand by which they can ascertain whether the aberration they have met with is one well known, or of frequent occurrence, or whether it presents any new feature, adding another item to our store of data, and therefore requiring closer observation and accurate record.

In making use, however, of Teratology in explanation of structure and affinities, great care is required. It is not every one who can handle these phenomena with the tact of a Darwin. In the course of my systematic labours I have met with several instances where teratologists have been led into conclusions which have proved to be far wide of the truth, owing to their having confined themselves to teratology to the neglect of homology and organogeny. This importance of teratological facts to the physiologist who is able duly to appreciate their bearing, and the discredit cast on their study owing to their misuse in hasty and incautious speculations, are alluded to in Dr. Masters's Introduction. But beyond some explanations of causes suggested by the bringing together series of facts showing a physiological connexion with each other and with more normal formations, he enters little into the various questions the solution of which has been more or less attempted by the aid of teratology. These questions, indeed, could not have been discussed without fully working out on each occasion normal organogeny, development, and homology, and thus leading him far beyond the object of the present work, which was to present to the future physiologist such a digested record of facts as should best show their relative bearing to each other, to normal conditions, and to any observed causes of disturbance. This object appears to have been well fulfilled, and the method adopted by the author probably the best suited to the purpose. A classification founded upon the nature of the causes inducing the several changes might, indeed, as he observes, have been theoretically the best, but is wholly impracticable until these causes shall have been satisfactorily ascertained. For the inquiry into these causes this teratological digest supplies a record of one class of facts, a necessary one, but only one of many classes on which it must be founded.