ON

# THE FLORA OF AUSTRALIA,

## ITS ORIGIN, AFFINITIES, AND DISTRIBUTION;

BEING AN

## Introductory Essay

TO THE

## FLORA OF TASMANIA.

BY

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# INTRODUCTORY ESSAY

TO

# THE FLORA OF TASMANIA.*

## § 1.

### *Preliminary Remarks.*

THE Island of Tasmania does not contain a vegetation peculiar to itself, nor constitute an independent botanical region. Its plants are, with comparatively few exceptions, natives of extratropical Australia; and I have consequently found it necessary to study the vegetation of a great part of that vast Continent, in order to determine satisfactorily the nature, distribution, and affinities of the Tasmanian Flora.

From the study of certain extratropical genera and species in their relation to those of Tasmania, I have been led to the far more comprehensive undertaking of arranging and classifying all the Australian plants accessible to me. This I commenced in the hope of being able thereby to extend our knowledge of the affinities of its Flora, and, if possible, to throw light on a very abstruse subject, viz. the origin of its vegetation, and the sources or causes of its peculiarity. This again has induced me to proceed with the inquiry into the origin and distribution of existing species; and, as I have already treated of these subjects in the Introduction to the New Zealand Flora, I now embrace the opportunity afforded me by a similar Introduction to the Tasmanian Flora, of revising the opinions I then entertained, and of again investigating the whole subject of the creation of species by variation, with the aid of the experience derived from my subsequent studies of the Floras of India and Australia in relation to one another and to those of neighbouring countries, and of the recently published hypotheses of Mr. Darwin and Mr. Wallace.

No general account of the Flora of Australia having hitherto been published, nor indeed a complete Flora of any part of it, I have been obliged, as a preliminary measure, to bring together and arrange the scattered materials (both published and unpublished) relating to its vegetation to which I had access. Those which are published consist of very numerous papers relating to the general botany of Australia, in scientific periodicals, and appended to books of travel, amongst which by far the most important are Brown's 'General Remarks, Geographical and Systematical, on the Botany of Terra Australis,' published in the Appendix to Captain Flinders' Voyage, now nearly half a century ago; Allan Cunningham's Appendix to Captain King's Voyage, which appeared in 1827; Lindley's Report on the Swan River Botany; and Mueller's, on the Tropical Botany of Australia. There are also some special essays or descriptive works on the Floras of certain parts of the continent: of

* Reprinted from the first volume of Dr. Hooker's 'Flora of Tasmania;' published in June, 1859.

these the most important are Brown's 'Prodromus,' of which the only published volume appeared in 1810; the 'Plantæ Preissianæ,' edited by Professor Lehmann, and containing descriptions, by various authors, of about 2250 species (including Cryptogamiæ) of Swan River plants; Dr. Mueller's various Reports on the Flora of Victoria, and his numerous papers on the vegetable productions of that colony; and Lindley's Appendices to Mitchell's Travels.

The unpublished materials chiefly consist of the vast collections of Australian plants made during the last half-century, and these having been obtained from all parts of the continent, and carefully ticketed as to locality, etc., supply abundant materials for the investigation of the main features of the Australian Flora. In another part of this Essay I propose to give a short summary of the labours of the individuals by whom these and other Australian collections have been principally obtained, and of the routes followed by the expeditions which they accompanied.

The majority of the collections were, either wholly or in part, transmitted to Sir William Hooker, forming the largest Australian herbarium in existence, and of which the published portion is in value greatly exceeded by the unpublished; for although about two-thirds of the plants have been described, only about half of these have been brought together in a systematic form; nor, since the publication of Brown's Appendix to Flinders' Voyage, has the Flora of the whole continent been considered from a general point of view. And, before entering on the field of inquiry so successfully explored by Brown half a century ago, I must pay my tribute to the sagacity and research exhibited in the essay to which I have alluded. At the time of its publication, not half the plants now described were discovered, vast areas were yet unexplored, and far too little was known of the vegetation of the neighbouring islands to admit of the Australian Flora being studied in its relation to that of other countries. Nevertheless we are indebted to Brown's powers of generalization for a plan of the entire Flora, constructed out of fragmentary collections from its different districts, which requires but little correction from our increased knowledge, though necessarily very considerable amplification. Although he could not show the extent and exact nature of its affinities, he could predict many of them, and by his detection of the representatives of plants of other countries under the masks of structural peculiarity which disguise them in Australia, he long ago gave us the key to the solution of some of those great problems of distribution and variation, which were then hardly propounded, but which are now prominent branches of inquiry with every philosophical naturalist.

In the Introductory Essay to the New Zealand Flora, I advanced certain general propositions as to the origin of species, which I refrained from endorsing as articles of my own creed: amongst others was the still prevalent doctrine that these are, in the ordinary acceptation of the term, created as such, and are immutable. In the present Essay I shall advance the opposite hypothesis, that species are derivative and mutable; and this chiefly because, whatever opinions a naturalist may have adopted with regard to the origin and variation of species, every candid mind must admit that the facts and arguments upon which he has grounded his convictions require revision since the recent publication by the Linnean Society of the ingenious and original reasonings and theories of Mr. Darwin and Mr. Wallace.

Further, there must be many who, like myself, having hitherto refrained from expressing any positive opinion, now, after a careful consideration of these naturalists' theories, find the aspect of the question materially changed, and themselves freer to adopt such a theory as may best harmonize with the facts adduced by their own experience.

The Natural History of Australia seemed to me to be especially suited to test such a theory, on account of the comparative uniformity of its physical features being accompanied with a great

variety in its Flora; of the differences in the vegetation of its several parts; and of the peculiarity both of its Fauna and Flora, as compared with those of other countries. I accordingly prepared a classified catalogue of all the Australian species in the Herbarium, with their ranges in longitude, latitude, and elevation, as far as I could ascertain them, and added what further information I could obtain from books. At the same time I made a careful study of the affinities and distribution of all the Tasmanian species, and of all those Australian ones which I believed to be found in other countries. I also determined as accurately as I could the genera of the remainder, and especially of those belonging to genera which are found in other countries, and I distinguished the species from one another in those genera which had not been previously arranged. In this manner I have brought together evidence of nearly 8000 flowering plants having been collected or observed in Australia, of which I have seen and catalogued upwards of 7000. About two-thirds of these are ascertained specifically with tolerable accuracy, and the remainder are distinguished from one another, and referred to genera with less certainty, being either undescribed, or described under several names, whilst some are members of such variable groups that I was left in doubt how to dispose of them.

To many who occupy themselves with smaller and better worked botanical districts, such results as may be deduced from the skeleton Flora I have compiled for Australia may seem too crude and imperfect to form data from which to determine its relations. But it is not from a consideration of specific details that such problems as those of the relations of Floras and the origin and distribution of organic forms will ever be solved, though we must eventually look to these details for proofs of the solutions we propose. The limits of the majority of species are so undefinable that few naturalists are agreed upon them;* to a great extent they are matters of opinion, even amongst those persons who believe that species are original and immutable creations; and as our knowledge of the forms and allies of each increases, so do these differences of opinion; the progress of systematic science being, in short, obviously unfavourable to the view that most species are limitable by descriptions or characters, unless large allowances are made for variation. On the other hand, when dealing with genera, or other combinations of species, all that is required is that these be classified in natural groups; and that such groups are true exponents of affinities settled by Nature is abundantly capable of demonstration. It is to an investigation of the extent, relations, and proportions of these natural combinations of species, then, that we must look for the means of obtaining and expressing the features of a Flora; and if in this instance the exotic species are well ascertained, it matters little whether or not the endemic are in all cases accurately distinguished from one another. Further, in a Flora so large as that of Australia, if the species are limited and estimated by one mind and eye, the errors made under each genus will so far counteract one another, that the mean results for the genera and orders will scarcely be affected. As it is, the method adopted has absorbed many weeks of labour during the last five years, and a much greater degree of accuracy could only have been obtained by a disproportionately greater outlay of time, whilst it would not have materially affected the general results.

With regard to my own views on the subjects of the variability of existing species and the fallacy of supposing we can ascertain anything through these alone of their ancestry or of originally created types, they are, in so far as they are liable to influence my estimate of the value of the facts collected for the analysis of the Australian Flora, unaltered from those which I maintained in the

* The most conspicuous evidence of this lies in the fact, that the number of known species of flowering plants is by some assumed to be under 80,000, and by others over 150,000.

'Flora of New Zealand:' on such theoretical questions, however, as the origin and ultimate permanence of species, they have been greatly influenced by the views and arguments of Mr. Darwin and Mr. Wallace above alluded to, which incline me to regard more favourably the hypothesis that it is to variation that we must look as the means which Nature has adopted for peopling the globe with those diverse existing forms which, when they tend to transmit their characters unchanged through many generations, are called species. Nevertheless I must repeat, what I have fully stated elsewhere, that these hypotheses should not influence our treatment of species, either as subjects of descriptive science, or as the means of investigating the phenomena of the succession of organic forms in time, or their dispersion and replacement in area, though they should lead us to more philosophical conceptions on these subjects, and stimulate us to seek for such combinations of their characters as may enable us to classify them better, and to trace their origin back to an epoch anterior to that of their present appearance and condition. In doing this, however, the believer in species being lineally related forms must employ the same methods of investigation and follow the same principles that guide the believer in their being actual creations, for the latter assumes that Nature has created species with mutual relations analogous to those which exist between the lineally-descended members of a family, and this is indeed the leading idea in all natural systems. On the other hand, there are so many checks to indiscriminate variation, so many inviolable laws that regulate the production of varieties, the time required to produce wide variations from any given specific type is so great, and the number of species and varieties known to propagate for indefinite periods a succession of absolutely identical members is so large, that all naturalists are agreed that for descriptive purposes species must be treated as if they were at their origin distinct, and are destined so to remain. Hence the descriptive naturalist who believes all species to be derivative and mutable, only differs in practice from him who asserts the contrary, in expecting that the posterity of the organisms he describes as species may, at some indefinitely distant period of time, require redescription.

I need hardly remark that the classificatory branch of Botany is the only one from which this subject can be approached, for a good system must be founded on a due appreciation of all the attributes of individual plants,—upon a balance of their morphological, physiological, and anatomical relations at all periods of their growth. Species are conventionally assumed to represent, with a great amount of uniformity, the lowest degree of such relationship; and the facts that individuals are more easily grouped into species limited by characters, than into varieties, or than species are into limitable genera or groups of higher value, and that the relationships of species are transmitted hereditarily in a very eminent degree, are the strongest appearances in favour of species being original creations, and genera, etc., arbitrarily limited groups of these.

The difference between varieties and species and genera in respect of definable limitation is however one of degree only, and if increased materials and observation confirm the doctrine which I have for many years laboured to establish, that far more species are variable, and far fewer limitable, than has been supposed, that hypothesis will be proportionally strengthened which assumes species to be arbitrarily limited groups of varieties. With the view of ascertaining how far my own experience in classification will bear out such a conclusion, I shall now endeavour to review, without reference to my previous conclusions, the impressions which I have derived from the retrospect of twenty years' study of plants. During that time I have classified many large and small Floras, arctic, temperate, and tropical, insular and continental: embracing areas so extensive and varied as to justify, to my apprehension, the assumption that the results derived

from these would also be applicable to the whole vegetable kingdom. I shall arrange these results successively under three heads; viz. facts derived from a study of classification; secondly, from distribution; thirdly, from fossils; after which I shall examine the theories with which these facts should harmonize.

## § 2.

### *On the General Phenomena of Variation in the Vegetable Kingdom.*

1. All vegetable forms are more or less prone to vary as to their sensible properties, or (as it has been happily expressed in regard to all organisms), "they are in a state of unstable equilibrium."* No organ is exactly symmetrical, no two are exact counterparts, no two individuals are exactly alike, no two parts of the same individual exactly correspond, no two species have equal differences, and no two countries present all the varieties of a species common to both, nor are the species of any two countries alike in number and kind.

2. The rate at which plants vary is always slow, and the extent or degree of variation is graduated. Sports even in colour are comparatively rare phenomena, and, as a general rule, the best-marked varieties occur on the confines of the geographical area which a species inhabits. Thus the scarlet Rhododendron (*R. arboreum*) of India inhabits all the Himalaya, the Khasia Mountains, the Peninsular Mountains, and Ceylon; and it is in the centre of its range (Sikkim and the Khasia) that those mean forms occur which by a graduated series unite into one variable species the rough, rusty-leaved form of Ceylon, and the smooth, silvery-leaved form of the North-western Himalaya. A white and a rose-coloured sport of each variety is found growing with the scarlet in all these localities, but everywhere these sports are few in individuals. Also certain individuals flower earlier than others, and some occasionally twice a year, I believe in all localities.

3. I find that in every Flora all groups of species may be roughly classified into three large divisions: one in which most species are apparently unvarying; another in which most are conspicuously varying; and a third which consists of a mixture of both in more equal proportions. Of these the unvarying species appear so distinct from one another that most botanists agree as to their limits, and their offspring are at once referable by inspection to their parents; each presents several special characters, and it would require many intermediate forms to effect a graduated change from any one to another. The most varying species, on the contrary, so run into one another, that botanists are not agreed as to their limits, and often fail to refer the offspring with certainty to their parents, each being distinguished from one or more others by one or a few such trifling characters, that each group may be regarded as a continuous series of varieties, between the terms of which no hiatus exists suggesting the intercalation of any intermediate variety. The genera *Rubus, Rosa, Salix,* and *Saxifraga,* afford conspicuous examples of these unstable species; *Veronica, Campanula,* and *Lobelia,* of comparatively stable ones.

4. Of these natural groups of varying and unvarying species, some are large and some small; they are also very variously distributed through the classes, orders, and genera of the Vegetable Kingdom; but, as a general rule, the varying species are relatively most numerous in those classes, orders, and genera which are the simplest in structure.† Complexity of structure is generally ac-

* Essays: Scientific, Political, and Speculative; by Herbert Spencer: p. 280.

† Mr. Darwin, after a very laborious analysis of many Floras, finds that the species of large genera are relatively more variable than those of small; a result which I was long disposed to doubt, because of the number of variable

companied with a greater tendency to permanence in form: thus Acotyledons, Monocotyledons, and Dicotyledons are an ascending series in complexity and in constancy of form. In Dicotyledons, *Salices, Urticeæ, Chenopodiaceæ,* and other Orders with incomplete or absent floral envelopes, vary on the whole more than *Leguminosæ, Lythraceæ, Myrtaceæ,* or *Rosaceæ,* yet members of these present, in all countries, groups of notoriously varying species, as *Eucalyptus* in Australia, *Rosa* in Europe, and *Lotus, Epilobium,* and *Rubus* in both Europe and Australia. Again, even genera are divided: of the last named, most or all of the species are variable; of others, as *Epacris, Acacia,* and the majority of such as contain upwards of six or eight species, a larger or smaller proportion only are variable. But the prominent fact is, that this element of mutability pervades the whole Vegetable Kingdom; no class nor order nor genus of more than a few species claims absolute exemption, whilst the grand total of unstable forms generally assumed to be species probably exceeds that of the stable.

5. The above remarks are equally applicable to all the higher divisions of plants. Some genera and orders are as natural, and as limitable by characters, as are some species; others again, though they contain many very well-marked subordinate plans of construction, yet are so connected by intermediate forms with otherwise very different genera or orders, that it is impossible to limit them naturally. And as some of the best marked and limited species consist of a series of badly marked and illimitable varieties, so some of the most natural* and limitable orders and genera may respectively consist of only undefinable groups of genera or of species. For instance, both *Gramineæ* and *Compositæ* are, in the present state of our knowledge, absolutely limited Orders, and extremely natural ones also; but their genera are to a very eminent degree arbitrarily limited, and their species extremely variable. *Orchideæ* and *Leguminosæ* are also well-limited Orders (though

small genera and the fact that monotypic genera seldom have their variations recorded in systematic works, but an examination of his data and methods compels me to acquiesce in his statement. It has also been remarked (Bory de Saint-Vincent, Voy. aux Quatre Iles de l'Afrique) that the species of islands are more variable than those of continents, an opinion I can scarcely subscribe to, and opposed to Mr. Darwin's facts, inasmuch as insular Floras are characterized by peculiar genera, and by having few species in proportion to genera. Bisexual trees and shrubs are generally more variable than unisexual, which however is only a corollary from what is stated above regarding plants of simple structure of flower. On the whole, I think herbs are more variable than shrubby plants, and annuals than perennials. It would be curious to ascertain the relative variableness of social and scattered plants. The individuals of a social plant, in each area it is social upon, are generally very constant, but individuals from different areas often differ much. The *Pinus sylvestris, Mughus,* and *uncinata* are cases in point, if considered as varieties of one; as are the Cedars of Atlas, Algeria, and the Himalaya.

* It should be borne in mind that the term *natural,* as applied to Orders or other groups, has often a double significance; every natural order is so in the sense of each of its members being more closely related to one or more of its own group than to any of another; but the term is often used to designate an easily limited natural order, that is, one whose members are so very closely related to each other by conspicuous peculiarities that its differential characters can be expressed, and itself always recognized; these may be called *objective* Orders; *Orchideæ* and *Gramineæ* are examples. Any naturalist, endowed with fair powers of observation and generalization, recognizes the close affinity between a pseudobulbous epiphytical, and a terrestrial tuberous-rooted Orchid, or between the Bamboo and Wheat, though the differences are exceedingly great in habit and in organs of vegetation and reproduction. Other orders are as natural and may be as well limited, but having no conspicuous characters in common, and presenting many subordinate distinct plans of structure, may be regarded as *subjective.* Such are *Ranunculaceæ* and *Leguminosæ,* of which a botanist must have a special and extensive knowledge before he can readily recognize very many of their members. No degree of natural sagacity will enable an uninstructed person to recognize the close affinity of *Clematis* and *Ranunculus,* or of *Acacia* and *Cytisus,* though these are really as closely related as the Orchids and Grasses mentioned above. We do not know why some Orders are subjective and some objective; but if the theory of creation by variation is a true one, we ought through it to reach a solution.

not so absolutely as the former), but they, on the contrary, consist of comparatively exceedingly well-marked genera and species. *Melanthaceæ* and *Scrophularineæ*, on the other hand, are not limitable as Orders, and contain very many differently constructed groups; but their genera, and to a great extent their species also, are well-marked and limitable. The circumstance of a group being either isolated or having complex relations, is hence no indication of its members having the same characters.

Again, as with species, so with genera and orders, we find that upon the whole those are the best limited which consist of plants of complex floral structure: the Orders of Dicotyledons are better limited than those of Monocotyledons, and the genera of Dichlamydeæ than those of Achlamydeæ.*

Now my object in dwelling on this parallelism between the characteristics of individuals in relation to species, of species in relation to genera, and of genera in relation to Orders, is because I consider (Introd. Essay to Fl. N. Z.) that it is to the extinction of species and genera that we are indebted for our means of resolving plants into limitable genera and orders. This view is now, I believe, generally admitted, even by those who still regard species as the immutable units of the Vegetable Creation; and it therefore now remains to be seen how far we are warranted in extending it to the limitation of species by the elimination of their varieties through natural causes.†

6. The evidence of variability thus deduced from a rapid general survey of the prominent facts elicited from a study of the principles of classification, are to a certain extent tested by the behaviour of plants under cultivation, which operates either by hastening the processes of Nature (in rapidly inducing variation), or by effecting a prolepsis or anticipation of those processes (in producing sports, *i. e.* better marked varieties, without graduated stages), or by placing the plant in conditions to which it would never have been exposed in the ordinary course of natural events, and which eventually either kill it or give origin to a series of varieties which might otherwise have never existed.‡

* There are too many exceptions to this to admit of our concluding at once that it is attributable to any simple and uniform law of variation; but it may be explained by assuming that the degree or amount of variation is differently manifested at different epochs in the history of the group. Thus, if a genus is numerically increasing, and consequently running into varieties, it will present a group of species with complex relations *inter se;* if, on the contrary, it is numerically decreasing, such decrease must lead to the extinction of some varieties, and hence result in the better limitation of the remainder. The application of this assumption to the fact of the best limited groups being most prevalent among the higher classes (*i. e.* among those most complicated in their organization), would at first sight appear an argument against progression, were it not for the consideration that the higher tribes of plants have in another respect proved themselves superior, in that they have not only far surpassed the lower in number of genera and species, but in individuals, and also in bulk and stature. And lastly, as all the highest orders of plants contain numerous species and often genera of as simple organization as any of the lower orders are, it follows that that physical superiority which is manifested in greater extent of variation, in better securing a succession of race, in more rapid multiplication of individuals, and even in increase of bulk, is in some senses of a higher order than that represented by mere complexity or specialization of organ.

† It follows as a corollary to the proposition (That species, etc., are naturally rendered limitable by the destruction of varieties), that there must be some intimate relation between the rate of increase and the duration of genera (or other groups of species) on the one hand, and the limitability of their species on the other. Thus, when a genus consists of a multitude of illimitable forms, we may argue with much plausibility that it is on the increase, because no intermediates have as yet been destroyed, and that the birth of individuals and the production of new forms is proceeding at a greater proportional rate than in an equally large genus of which the species are limitable.

‡ My friend Mr. Wallace treats of animals under domestication, not only as if they were in very different physical conditions from those in a state of nature, inasmuch as every sense and faculty is continually fully exercised and strengthened by wild animals, whilst certain of these lie dormant in the domesticated, but as if they were

7. Now the prominent phenomena presented by species under cultivation are analogous in kind and extent to those which we have derived from a survey of the affinities of plants in a state of nature: a large number remain apparently permanent and unalterable, and a large number vary indefinitely. Of the permanent there is little to remark, except that they belong to very many orders of plants, nor are they always those which are permanent in a state of nature. Many plants, acknowledged by all to be varieties, may be propagated by seed or otherwise, when their offspring retains for many successive generations the characters of the variety. On the other hand, species which have remained immutable for many generations under cultivation, do at length commence to vary, and having once begun, are thereafter peculiarly prone to vary further.

8. The variable cultivated species present us with the most important phenomena for investigating the laws of mutability and permanence; but these phenomena are so infinitely varied, complex, and apparently contradictory, as to defeat all attempts to elucidate the history of any individual case of variation by a study of its phases alone. It would often appear doubtful whether the natural operations of a plant tend most to induce or to oppose variation; and we hence find the advocates of original permanent creations, and those of mutable variable species, taking exactly opposite views in this respect, the truth, I believe, being that both are right. Nature has provided for the possibility of indefinite variation, but she regulates it as to extent and duration; she will neither allow her offspring to be weakened or exhausted by promiscuous hybridization and incessant variation, nor will she suffer a new combination of external conditions to destroy one of these varieties without providing a substitute when necessary; hence some species remain so long hereditarily immutable as to give rise to the doctrine that all are so normally, while others are so mutable as to induce a belief in the very opposite doctrine, which demands incessant lawless change.

9. It would take far too long a time were I to attempt any analysis of the phenomena of cultivation, as illustrative of those of variability in a state of nature. There are however some broad facts which should be borne in mind in treating of variation by cross impregnation and hybridity.

10. Variation is effected by graduated changes; and the tendency of varieties, both in nature and under cultivation, when further varying, is rather to depart more and more widely from the original type, than to revert to it: the best marked varieties of a wild species occurring on the confines of the area the species inhabits, and the best marked varieties of the cultivated species being those last produced by the gardener. I am aware that the prevalent opinion is that there is a strong tendency in cultivated, and indeed in all varieties, to revert to the type from which they departed; and I have myself quoted this opinion, without questioning its accuracy,* as tending to sup-

subject to the influence of fundamentally different laws. He says, "No inferences as to varieties in a state of nature can be deduced from the observation of those occurring among domestic animals. The two are so much opposed that what applies to the one is almost sure not to apply to the other." But, in the first place, of the same species of wild animals some families must be placed where certain faculties and senses are far more exercised than others, and the difference in this respect between the conditions of many families of wild animals is as great as those between many wild and tame families; and secondly, other senses and faculties, latent and unknown in the wild animal, but which are as proper to the species as any it exercised in its wild state, are manifested or developed by it under domestication. An animal in a state of nature is not then, as Mr. Wallace assumes, "in the full exercise of every part of its organization;" were it so, it could not vary or alter with altered conditions, nor could other faculties remain to be called into play under domestication. The tendency of species when varying cannot be to depart from the original type in a wild condition and to revert to it under domestication, for man cannot invert the order of Nature, though he may hasten or retard some of its processes.

* Fl. N. Zeal., Introd. Essay, p. x., and Flora Indica, Introduction, p. 14.

port the views of those who regard species as permanent. A further acquaintance with the results of gardening operations leads me now to doubt the existence of this centripetal force in varieties, or at least to believe that in the phrase "reversion to the wild type," many very different phenomena are included. In the first place, the majority of cultivated vegetables and cerealia, such as the Cabbage and its numerous progeny, and the varieties of wall-fruit, show when neglected no disposition to assume the characters of the wild states of these plants;* they certainly degenerate, and even die if Nature does not supply the conditions which man (by anticipation of her operations, or otherwise) has provided; they become stunted, hard, and woody, and resemble their wild progenitors in so far as all stunted plants resemble wild plants of similar habit; but this is not a reversion to the original type, for most of these cultivated races are not *merely* luxuriant forms of the wild parent. In neglected fields and gardens we see plants of Scotch Kale, Brussels Sprouts, or Kohl-rabi, to be all as unlike their common parent, the wild *Brassica oleracea,* as they are unlike one another; so, too, most of our finer kinds of apples, if grown from seed, degenerate and become crabs, but in so doing they become crab states of the varieties to which they belong, and do not revert to the original wild Crab-apple. And the same is true to a great extent of cultivated Roses, of many varieties of trees, of the Raspberry, Strawberry, and indeed of most garden plants. It has also been held, that by imitating the conditions under which the wild state of a cultivated variety grows, we may induce that variety to revert to its original state; but, except in the false sense of reversion above explained, I doubt if this is supported by evidence. Cabbages grown by the seaside are not more like wild Cabbages than those grown elsewhere, and if cultivated states disseminate themselves along the coast, they there retain their cultivated form. This is however a subject which would fill a volume with most instructive matter for reflection, and which receives a hundredfold more illustration from the Animal than from the Vegetable Kingdom. I can here only indicate its bearing on the doctrine of variation, as evidence that Nature operates upon mutable forms by allowing great variation, and displaying little tendency to reversion.† With this law the suggestive observation of M. Vilmorin well accords, that when once the constitution of a plant is so broken that variation is induced, it is easy to multiply the varieties in succeeding generations.

It may be objected to this line of argument that our cultivated plants are, as regards their constitution, in an artificial condition, and are, if unaided, incapable of self-perpetuation; but an artificially induced condition of constitution is not necessarily a diseased or unnatural one, and, so far as our cultivated plants are concerned, all we do is to place them under conditions which Nature does not provide *at the same particular place and time.* That Nature might supply the conditions at other places and times may be inferred from the fact that the plant is found to be provided with the means of availing itself of them when provided, while at the same time it retains all its functions, not only unimpaired, but in many cases in a more highly developed state. We have no reason to suppose that we have violated Nature's laws in producing a new variety of wheat,—we may have only anticipated them; nor is its constitution impaired because it cannot, unaided, perpetuate its race; it is in as sound and unbroken health and vigour during its life as any wild variety is, but its offspring

* Hence the great and acknowledged difficulty of determining the wild parent species of most of our cultivated fruits, cerealia, etc., and in fact of almost every member of our Flora Cibaria. This would not be so were there any disposition in the neglected cultivated races to revert to the wild form.

† It is not meant by this that any character of a species which may be lost in its variety never reappears in the descendants of the latter, for some occasionally do so in great force; what is meant is, that the newly acquired characters of the variety are never so entirely obliterated that it has no longer a claim to be considered a variety.

has so many enemies that they do not perpetuate its race. In the case of annual plants, those only can secure the succession of their species which produce more seeds annually than can be eaten by animals or destroyed by the elements. Cultivated wheat will grow and ripen its seed in almost all soils and climates, and as its seeds are produced in great abundance, and can be preserved alive in any quantity, in the same climate, and for many years, it follows that it is not to the artificial or peculiar condition of the plant itself, and still less to any change effected by man upon it, that its annual extinction is due, but to causes that have no effect whatever upon its own constitution, and over which its constitutional peculiarities can exercise no control.

11. Again, the phenomena of cross impregnation amongst individuals of all species appear, according to Mr. Darwin's accurate observations, to have been hitherto much underrated, both as to extent and importance. The prominent fact that the stamens and pistil are so often placed in the same flower, and come to maturity at the same epoch, has led to the doctrine that flowers are usually self-impregnated, and that the effect is a conservative one as regards the permanence of specific forms. The observations of Carl Sprengel and others have, however, proved that this is not always the case, and that while Nature has apparently provided for self-fertilization, she has often insidiously counteracted its operation, not only by placing in flowers lures for insects which cross-fertilize them, but often by interposing insuperable obstacles to self-fertilization, in the shape of structural impediments to the access of the pollen to the stigma of its own flower.* In all these instances the double object of Nature may be traced; for self-impregnation (or "breeding in"), while securing identity of form in the offspring, and hence hereditary permanence, at the same time tends to weakness of constitution, and hence to degeneracy and extinction: on the other hand, cross-impregnation, while tending to produce diversity of form in the offspring, and hence variation and apparent mutability, yet by strengthening the offspring favours longevity and apparent permanence of specific type. The ultimate effect of all these operations is of course favourable to the hypothesis that variability is the rule, and permanence the exception, or at any rate only a transitory phenomenon.

12. Hybridization, or cross-impregnation between species or very well marked varieties, again, is a phenomenon of a very different kind, however similar it may appear in operation and analogous in design. Hybridizable genera are rarer than is generally supposed, even in gardens, where they are so often operated upon, under circumstances the most favourable to the production of a hybrid, and unfavourable to self-impregnation. Hybrids are almost invariably barren, and their characters are not those of new varieties. The obvious tendency of hybridization between varieties or other very closely allied forms (in which case the offspring may be fertile) is not to enlarge the bounds of variation, but to contract them; and if between very different forms, it will only tend to confound these. That some supposed species may have their origin in hybridization cannot be denied, but we are now dealing with phenomena on a large scale, and balancing the tendencies of causes uniformly acting, whose effects are unmistakable, and which can be traced throughout the Vegetable Kingdom. In gardening operations the number of hybridized genera is small, their offspring doomed, and since they are more readily impregnated by the pollen of either parent than by their own,

* Thus, in *Lobelia fulgens*, the pollen is entirely prevented by natural causes from reaching the stigma of its own flower. In kidney beans impregnation takes place imperfectly except the carina is worked up and down artificially, which is effected by bees, who may thus either impregnate the flower with its own pollen or with that brought from another plant. I am indebted to Mr. Darwin for both these facts: see 'Gardeners' Chronicle,' 1858, p. 828.

or by that of any other plant,* they eventually revert to one of their parents: on the other hand, the number of varieties is incalculable, the power to vary further is unimpaired in their progeny, and these tend to depart further and further in sensible properties from the original parent.

In conformity with my plan of starting from the variable and not the fixed aspect of Nature, I have now set down the prominent features of the Vegetable Kingdom, as surveyed from this point of view. From the preceding paragraphs the evidence appears to be certainly in favour of proneness to change in individuals, and of the power to change ceasing only with the life of the individual; and we have still to account for the fact that there are limits to these mutations, and laws that control the changes both as to degree and kind; that species are neither visionary nor even arbitrary creations of the naturalist; that they are, in short, realities, whether only temporarily so or not.

13. Granting then that the tendency of Nature is first to multiply forms of existing plants by graduated changes, and next by destroying some to isolate the rest in area and in character, we are now in a condition to seek some theory of the *modus operandi* of Nature that will give temporary permanence of character to these changelings. And here we must appeal to theory or speculation; for our knowledge of the history of species in relation to one another, and to the incessant mutations of their environing physical conditions, is far too limited and incomplete to afford data for demonstrating the effects of these in the production of any one species in a native state.

Of these speculations by far the most important and philosophical is that of the delimitation of species by natural selection, for which we are indebted to two wholly independent and original thinkers, Mr. Darwin and Mr. Wallace.† These authors assume that all animal and vegetable forms are variable, that the average amount of space and annual supply of food for each species (or other group of individuals) is limited and constant, but that the increase of all organisms tends to proceed annually in a geometrical ratio; and that, as the sum of organic life on the surface of the globe does not increase, the individuals annually destroyed must be incalculably great; also that each species is ever warring against many enemies, and only holding its own by a slender tenure. In the ordinary course of nature this annual destruction falls upon the eggs or seeds and young of the organisms, and as it is effected by a multitude of antagonistic, ever-changing natural causes, each more destructive of one organism than of any other, it operates with different effect on each group of individuals, in every locality, and at every returning season. Here then we have an infinite number of varying conditions, and a superabundant supply of variable organisms, to accommodate themselves to these conditions. Now the organisms can have no power of surviving any change in these conditions, except they are endowed with the means of accommodating themselves to it. The exercise of this power may be accompanied by a visible (morphological) change in the form or structure of the individual, or it may not, in which case there is still a change, but a physiological one, not outwardly

* A very able and careful experimenter, M. Naudin, performed a series of experiments at the Jardin des Plantes at Paris, in order to discover the duration of the progeny of fertile hybrids. He concludes that the fertile posterity of hybrids disappears, to give place to the pure typical form of one or other parent. "Il se peut sans doute qu'il y ait des exceptions à cette loi de retour, et que certains hybrides, à la fois très-fertiles et très-établis, tendent à faire souche d'espèce; mais le fait est loin d'être prouvé. Plus nous observons les phénomènes d'hybridité, plus nous inclinons à croire que les espèces sont indissolublement liées à une fonction dans l'ensemble des choses, et que c'est le rôle même assigné à chacune d'elles qui en détermine la forme, la dimension et la durée." (Annales des Sc. Nat. sér. 4. v. 9.)

† Journal of the Linnean Society of London, Zoology, vol. iii. p. 45.

manifested; but there is always a morphological change if the change of conditions be sudden, or when, through lapse of time, it becomes extreme. The new form is necessarily that best suited to the changed condition, and as its progeny are henceforth additional enemies to the old, they will eventually tend to replace their parent form in the same locality. Further, a greater proportion of the seeds and young of the old will annually be destroyed than of the new, and the survivors of the old, being less well adapted to the locality, will yield less seed, and hence have fewer descendants.

In the above operations Nature acts slowly on all organisms, but man does so rapidly on the few he cultivates or domesticates; he selects an organism suited to his own locality, and by so modifying its surrounding conditions that the food and space that were the share of others falls to it, he ensures a perpetuation of his variety, and a multiplication of its individuals, by means of the destruction of the previous inhabitants of the same locality; and in every instance, where he has worked long enough, he finds that changes of form have resulted far greater than would suffice to constitute conventional species amongst organisms in a state of nature, and he keeps them distinct by maintaining these conditions.

Mr. Darwin adduces another principle in action amongst living organisms as playing an important part in the origin of species, viz. that the same spot will support most life when peopled with very diverse forms, as is exemplified by the fact that in all isolated areas the number of Classes, Orders, and Genera is very large in proportion to that of Species.

## § 3.

### *On the General Phenomena of Distribution in Area.*

Turning now to another class of facts, those that refer to the distribution of plants on the surface of the globe, the following are the most obvious:—

14. The most prominent feature in distribution is that circumscription of the area of species, which so forcibly suggests the hypothesis that all the individuals of each species have sprung from a common parent, and have spread in various directions from it. It is true that the area of some (especially Cryptogamic and Aquatic plants) is so great that we cannot indicate any apparent centre of diffusion, and that others are so sporadic that they appear to have had many such centres; but these species, though more numerous than is usually supposed, are few in comparison with those that have a definite or circumscribed area.

With respect to this limitation in area,* species do not essentially differ from varieties on the one hand, or from genera and higher groups on the other; and indeed, in respect of distribution, they hold an exactly intermediate position between them, varieties being more restricted in locality than species, and these again more than genera.

* It is a remarkable fact that there are some striking anomalies in the distribution of plants into provinces, as compared with animals. Thus there is no peculiarity in the vegetation of Australia to be compared with the rarity of placental mammals, nor with the fact of so many of the mammals, birds, and fish of Tasmania differing from those of the continent of Australia. Nearer home, we find the basin of the Mediterranean with a tolerably uniform Flora on the European and North African sides, but these ranking as different zoological provinces. The much narrower delimitation in area of animals than plants, and greater restriction of Faunas than Floras, should lead us to anticipate that plant types are, geologically speaking, more ancient and permanent than the higher animal types are, and so I believe them to be, and I would extend the doctrine even to plants of highly complex structure.

The universality of this feature (of groups having defined areas) affords to my mind all but conclusive evidence in favour of the hypothesis of similar forms having had but one parent, or pair of parents. And further, this circumscription of species and other groups in area, harmonizes well with that principle of divergence of form, which is opposed to the view that the same variety or species may have originated at different spots. It also follows that, as a general rule, the same species will not give rise to a series of similar varieties (and hence species) at different epochs; whence the geological evidence of contemporaneity derived from identity of fossil forms may be relied upon.

The most obvious cause of this limitation in area no doubt exists in the well-known fact that plants do not necessarily inhabit those areas in which they are constitutionally best fitted to thrive and to propagate; that they do not grow where they would most like to, but where they can find space and fewest enemies. We have seen (13) that most plants are at warfare with one or more competitors for the area they occupy, and that both the number of individuals of any one species and the area it covers are contingent on the conditions which determine these remaining so nicely balanced that each shall be able at least to hold its own, and not succumb to the enervating or etiolating or smothering influences of its neighbours. The effects of this warfare are to extinguish some species, to spare only the hardier races of others, and especially to limit the remainder both as to area and characters. Exceptions occur in plants suited to very limited or abnormal conditions, such as desert plants, the chief obstacles to whose multiplication are such inorganic and principally atmospheric causes as other plants cannot overcome at all; such plants have no competitors, are generally widely distributed, and not very variable.*

15. The three great classes of plants, Acotyledons, Monocotyledons, and Dicotyledons (Gymnospermous and Angiospermous), are distributed with tolerable equality over the surface of the globe, inasmuch as we cannot indicate any of the six continents (Europe, Asia, Africa, North and South America, and Australia) as being peculiarly rich in one to the exclusion of another. Further, the distribution of some of the larger Orders is remarkably equable, as *Compositæ, Leguminosæ, Gramineæ,* and others; facts which (supposing existing species to have originated in variation) would seem to indicate that the means of distribution have overcome, or been independent of the existing apparent impediments, and that the power of variation is equally distributed amongst these classes, and continuously exerted under very different conditions. I do not mean that all the classes are equally variable, but that each displays as much variety in one continent as in another.

16. Those Classes and Orders which are the least complex in organization are the most widely distributed, that is to say, they contain a larger proportion of widely diffused species. Thus the species of Acotyledons are more widely dispersed than those of Monocotyledons, and these again more so than those of Dicotyledons; so also the species of *Thallophytes* are among the most widely dispersed of Acotyledons, the *Gramineæ* of Monocotyledons, and the *Chenopodiaceæ* of Dicotyledons. This tendency of the least complex species to be most widely diffused is most marked in Acotyledons, and least so in Dicotyledons,† a fact which is analogous to that already stated (4), that the least complex are also the most variable.

* Though invariable forms, they may be, and often are, themselves varieties or races of a species that inhabits more fertile spots, as *Poa bulbosa*, which is a very well-marked and constant form of *P. pratensis*, occurring in dry sandy soil, from England to North-western India, its "meadow" relative being a very variable species in the same countries, and always struggling for existence amongst other Grasses, etc.

† Very much, no doubt, because of the difficulty in classifying Dicotyledons by complexity of organization; in other words, of our inability to estimate in a classificatory point of view the relative value of the presence or absence

17. Though we rarely find the same species running into the same varieties at widely sundered localities (unless starved or luxuriant forms be called varieties), yet we do often find a group of species represented in many distant places by other groups of allied forms; and if we suppose that individuals of the parent type have found their way to them all, the theory that existing species have originated in variation, and that varieties depart further from the parent form, will account for such groups of allied species being found at distant spots; as also for these groups being composed of representative species and genera.

18. No general relations have yet been established between the physical conditions of a country and the number of species or varieties which it contains, further than that the tropical and temperate regions are more fertile than the polar, and that perennial drought is eminently unfavourable to vegetation. It is not even ascertained whether the tropical climates produce more species than the temperate.

19. Though we cannot explain the general relations between the vegetation and physical condition of any two countries that contrast in these respects, we may conclude as a general rule that those tracts of land present the greatest variety in their vegetation that have the most varied combinations of conditions of heat, light, moisture, and mineral characters. It is, in the present state of our knowledge, impossible to measure the amount of the fluctuations of these conflicting conditions in a given country, nor if we could can we express them symbolically or otherwise so as to make them intelligible exponents of the amount of variety in the vegetation they affect; but the following facts in general distribution appear to me to be favourable to the idea that there is such a connection.

There are certain portions of the surface of the globe characterized by a remarkable uniformity in their phænogamic vegetation. These may be luxuriantly clothed, and abound in individuals, but are always poor in species. Such are the cooler temperate and subarctic lake regions of North America, Fuegia and the Falkland Islands, the Pampas of Buenos Ayres, Siberia and North Russia, Ireland and Western Scotland, the great Gangetic plain, and many other tracts of land. Now all these regions are characterized by a great uniformity in most of their physical characters, and an absence of those varying conditions which we assume to be stimulants to variation in a locality. On the other hand, it is in those tracts that have the most broken surface, varied composition of rocks, excessive climate (within the limits of vegetable endurance), and abundance of light, that the most species are found, as in South Africa, many parts of Brazil and the Andes, Southern France, Asia Minor, Spain, Algeria, Japan, and Australia.

20. The Polar regions are chiefly peopled from the colder temperate zones, and the species from the latter which have spread into them are very variable, but only within comparatively small limits, particularly in stature, colour, and vesture. Many of these polar and colder temperate plants are also found, together with other species closely allied to them, on the mountains of the warm temperate, and even tropical zones; to which it is difficult to conceive that they can have been transported by agencies now in operation.

21. The Floras of islands present many points of interest. The total number of species they contain seems to be invariably less than an equal continental area possesses, and the relative numbers of species to genera (or other higher groups) is also much less than in similar continental areas.

The further an island is from a continent, the smaller is its Flora numerically, the more

of organs in plants, where many are present, and where those of low morphological importance may have a comparatively high physiological significance.

peculiar is its vegetation, and the smaller its proportion of species to genera. In the case of very isolated islands, moreover, the generic types are often those of very distant countries, and not of the nearest land. Thus the St. Helena and Ascension forms are not so characteristic of tropical Africa as of the Cape of Good Hope. Those of Kerguelen's Land are Antarctic American, not African nor Indian. The Sandwich Islands contain many North-west American and some New Zealand forms. Japan presents us with many genera and species unknown except to the *eastward* of the Rocky Mountains, in North America.* So too American, Abyssinian, and even South African genera and species are found in Madeira and the Canary Islands; and Fuegian ones in Tristan d'Acunha.

22. There is a strict analogy in this respect between the Floras of islands and those of lofty mountain-ranges, no doubt in both cases owing to the same causes. Thus, as Japan contains various peculiar N.E. American species which are not found in N.W. America nor elsewhere on the globe, and the Canaries and Azores possess American genera not found in Europe nor Africa, so the lofty mountains of Borneo contain Tasmanian and Himalayan representatives; the Himalayas contain Andean, Rocky Mountain, and Japanese genera and species; and the alps of Victoria and Tasmania contain assemblages of New Zealand, Fuegian, Andean, and European genera and species. We cannot account for any of these cases of distribution between islands and mountains except by assuming that the species and genera common to these distant localities have found their way across the intervening spaces under conditions which no longer exist.

23. There is much to be observed in the condition and distribution of the introduced or naturalized plants of a country, which may be applied to the study of the origin of its indigenous vegetation. The greater proportion of these are the annual and other weeds of cultivated land, and plants which attach themselves to nitrogenous soils; naturalized perennials, shrubs, and trees occur consecutively in rapidly diminishing proportions. I can find no decided relation between complexity of structure and proneness to migrate, nor much between facilities for transport or power of endurance or vitality in the seed, and extent of distribution by artificial means. I shall return to this subject (which I have elsewhere discussed at length with reference to the Galapagos Archipelago†) when treating of the naturalized plants of Australia.

24. I venture to anticipate that a study of the vegetation of islands with reference to the peculiarities of their generic types on the one hand, and of their geological condition (whether as rising or sinking) on the other, may, in the present state of our knowledge, advance the subjects of distribution and variation considerably. The incompleteness of the collections at my command from the Polynesian islands, has frustrated my attempts to illustrate this branch of inquiry by extending my researches from the Australian Flora over that of the Pacific. I may however indicate as a general result, that I find the sinking islands, those (so determined by Darwin's able investigations) characterized as atolls, or as having barrier reefs, to contain comparatively fewer species and fewer peculiar generic types than those which are rising. Thus, commencing from the east coast of Africa, I find in the Indian Ocean the following islands marked in Darwin's chart‡ as bounded with fringing reefs or active volcanos, and hence rising:—The Seychelles, Madagascar, Mauritius, Bourbon, Ceylon, the Andamans, Nicobar, and Sumatra; the vegetation of all which is characterized by great diversity and much peculiarity of generic type: whereas those marked as

* Whilst these sheets are passing through the press, I have been informed by Professor Asa Gray that the Flora of Japan and N.E. Asia is much more closely allied to that of the Northern United States than to that of America west of the Rocky Mountains.

† Linn. Trans. xx. 235.

‡ See his works on volcanic islands and on coral reefs.

atolls or barrier reefs, as the Maldives, Laccadives, and Keeling Island, contain few species, and those the same as grow on the nearest continents. In the Pacific Ocean, again, the groups of islands most remarkable for their ascertained number of very peculiar generic types are the Sandwich group, Galapagos, Juan Fernandez, Loochoo and Bonin, all of which are rising, and most have active volcanos: those with the least amount of peculiarity are the Society group and Fijis, both of which are sinking. In the present state of our knowledge it is not safe to lay much stress on these apparent facts, especially as the New Hebrides and New Caledonia, which lie very close together, and both, I believe, contain much peculiarity, are in opposite geological conditions, the Hebrides rising and Caledonia sinking; and the Friendly* and Fiji groups, equally near one another, and with, I suspect, very similar vegetation, are also represented as being in opposite conditions. On the other hand, whole of the group including the Low Archipelago and the Society Islands, extending over more than 2000 miles, I observe but one rising spot,† namely, Elizabeth Island, a mere speck of land, but which is the only known habitat of one of the most remarkable genera of *Compositæ*.‡

25. Many of the above facts in the general distribution of species cannot be wholly accounted for by the supposition that natural causes have dispersed them over such existing obstacles as seas, deserts, and mountain-chains; moreover, some of these facts are opposed to the theory that the creation of existing species has taken place subsequent to the present distribution of climates, and of land and water, and to that of their dispersion having been effected by the now prevailing aquatic, atmospheric, and animal means of transport.

Similar climates and countries, even when altogether favourably placed for receiving colonists from each other, and with conditions suitable to their reciprocal exchange, do not, as a rule, interchange species. Causes now in operation will not account for the fact that only 200 of the New Zealand Flowering Plants are common to Australia, and still less for the contrasting one that the very commonest, most numerous, and universally distributed Australian genera and species, as *Casuarina, Eucalyptus, Acacia, Boronia, Helichrysum, Melaleuca,* etc., and all the Australian *Leguminosæ* (including a European genus and species), are absent from New Zealand. Causes now in operation cannot be made to account for a large assemblage of Flowering Plants characteristic of the Indian peninsula being also inhabitants of tropical Australia, while not one characteristic Australian genus has ever been found in the peninsula of India. Still less will these causes account for the presence of Antarctic and European species in the Alps of Tasmania and Victoria, or for the reappearance of Tasmanian genera on the isolated lofty mountain of Kina-Balou, in Borneo.

These and a multitude of analogous facts have led to the study of two classes of agents, both of which may be reasonably supposed to have had a powerful effect in determining the distribution of plants; these are changes of climates, and changes in the relative positions and elevations of land.

26. Of these, that most easy of direct application is the effect of humidity in extending the

* I find that there is a remarkable difference between the Floras of the New Hebrides and Caledonia on the one hand, and those of the Fiji islands and those to the east of them on the other. In the former, New Zealand and Australian types abound; in the latter, almost exclusively Indian forms. The differences between the Floras of Fiji, Samoa, Tonga, Tahiti, and that of India, are in species and not in genera, and many species are common to all.

† Mr. Darwin has left Aurora Island (another of the group) uncoloured, on account of the doubtful evidence regarding it, which however is in favour of its being in the same condition as Elizabeth Island. From a list of species communicated by Mr. Dana, it appears to contain no peculiar plants.

‡ *Fitchia.* See Lond. Journ. Bot. 1845, iv. p. 640. t. 23, 24.

range of species into regions characterized by what would otherwise be to them destructive temperatures.

I have, in the 'Antarctic Flora,' shown that the distribution of tropical forms is extended into cold regions that are humid and equable further than into such as are dry and excessive; and, conversely, that temperate forms advance much further into humid and equable tropical regions than into dry and excessive ones; and I have attributed the extension of Tree-ferns, Epiphytal Orchids, Myrtaceæ, etc., into high southern latitudes, to the moist and equable climate of the south temperate zone. I have also shown how conspicuously this kind of climate influences the distribution of mountain plants in India, where tropical forms of Laurel, Fig, Bamboo, and many other genera, ascend the humid extratropical mountains of Eastern Bengal and Sikkim to fully 9000 feet elevation; and temperate genera, and in some cases species, of Quercus, Salix, Rosa, Pinus, Prunus, Camellia, Rubus, Kadsura, Fragaria, Æsculus, etc., descend the mountains even to the level of the sea, in lat. 25°. In a tropical climate the combined effects of an equable climate and humidity in thus extending the distribution of species, often amount to 5000 feet in elevation or depression (equivalent to 15° Fahr. of isothermals in latitude), a most important element in our speculations on the comparative range of species under existing or past conditions; and when to this is added that the average range in altitude of each Himalayan tropical and temperate and alpine species of Flowering Plant is 4000 feet, which is equivalent to 12° of isothermals of latitude, we can understand how an elevation of a very few thousand feet might, under certain climatic conditions, suffice to extend the range of an otherwise local species over at least 25° parallels of latitude, and how a proportionally small increase of elevation in a meridional chain where it crosses the Equator, may enable temperate plants to effect an easy passage from one temperate zone to the other.

27. To explain more fully the present distribution of species and genera in area, I have recourse to those arguments which are developed in the Introductory Essay to the New Zealand Flora, and which rest on geological evidence, originally established by Sir Charles Lyell, that certain species of animals have survived great relative changes of sea and land. This doctrine, which I in that Essay endeavoured to expand by a study of the distribution of existing Southern species, has, I venture to think, acquired additional weight since then, from the facts I shall bring forward under the next head of Geological Distribution, and which seem to indicate that many existing Orders and Genera of plants of the highest development may have flourished during the Eocene and Cretaceous periods, and have hence survived complete revolutions in the temperature and geography of the middle and temperate latitudes of the globe.

28. Mr. Darwin has greatly extended in another direction these views of the antiquity of many European species, and their power of retaining their *facies* unchanged during most extensive migrations, by his theory of the simultaneous extension of the glacial temperature in both hemispheres, and its consequent effect in cooling the tropical zone. He argues that, under such a cold condition of the surface of the globe, the temperate plants of both hemispheres may have been almost confined to the tropical zone, whence afterwards, owing to an increment of temperature, they would be driven up the mountains of the tropics, and back again to those higher temperate latitudes where we now find most of them. I have already (New Zealand Essay) availed myself of the hypothesis of an austral glacial period, to account for Antarctic species being found on the alps of Australia, Tasmania, and New Zealand; and if as complete evidence of such a proportionally cooled state of the intertropical regions were forthcoming as there is of a glacial condition of the temperate zones, it would amply suffice to account for the presence of European and Arctic species in the Antarctic and south tem-

perate regions, and of the temperate species of both hemispheres on the mountains of intermediate tropical latitudes.

On the other hand, we have sufficient evidence of many of what are now the most tropical Orders of plants having inhabited the north temperate zone before the glacial epoch; and it is difficult to conceive how these Orders could have survived so great a reduction of the temperature of the globe as should have allowed the preglacial temperate Flora to cross the Equator in any longitude. It is evident that, under such cold, the most tropical Orders must have perished, and their re-creation after the glacial epoch is an inadmissible hypothesis.*

29. It remains then to examine whether, supposing the glacial epochs of the northern and southern hemispheres to have been contemporaneous, the relations of land and sea may not have been such as that a certain meridian may have retained a tropical temperature near the Equator, and thus have preserved the tropical forms. Such conditions might perhaps be attained by supposing two large masses of land at either pole, which should contract and join towards the Equator, forming one meridional continent, while one equatorial mass of land should be placed at the opposite meridian. If the former continent were traversed by a meridional chain of mountains, and so disposed that the polar oceanic currents should sweep towards the Equator for many degrees along both its shores, its equatorial climate would be throughout far more temperate than that of the opposite equatorial mass of land, whose climate would be tropical, insular, and humid.

30. The hypothesis of former mountain chains having afforded to plants the means of migration, by connecting countries now isolated by seas or desert plains, is derived from the evidence afforded by geology of the extraordinary mutation in elevation that the earth's surface has experienced since the appearance of existing forms of animals and plants. In the Antarctic Flora I suggested as an hypothesis that the presence of so many Arctic-American plants in Antarctic America might be accounted for by supposing that the now depressed portions of the Andean chain had, at a former period, been so elevated that the species in question had passed along it from the north to the south temperate zone;† and there are some facts in the distribution of species common to the mountain Floras of the Himalaya and Malay Islands, and of Australia and Japan, that would well accommodate themselves to a similar hypothesis. Of such submerged meridional lands we have some slender

* The question of the state of the mean temperature of the globe during comparatively recent geological periods is yearly deriving greater importance in relation to the problem of distribution. Upon this point geologists are not altogether clear, nor at one with the masters of physical science. Lyell (Principles, ed. ix. chap. vii.) attributes the glacial epoch to such a disposition of land and sea as would sufficiently cool the temperate zones; and he implies that this involves or necessitates a lowering of the mean temperature of the whole globe. Another hypothesis is, that there was a lowering of the mean temperature of the globe wholly independent of any material change in the present relations of sea and land, which cold induced the glacial epoch. A third theory is that such a redisposition of land and sea as would induce a glacial epoch in our hemisphere need not be great, nor necessitate a decrement of the mean temperature of the whole earth.

† The continuous extension of so many species along the Cordillera (of which detailed evidence is given in the Antarctic Flora) from the Rocky Mountains to Fuegia, is a most remarkable fact, considering how great the break is between the Andes of New Granada and those of Mexico, and that the intermediate countries present but few resting-places for alpine plants. That this depression of the chain has had a powerful effect in either limiting the extension of species which have appeared since its occurrence, or in inducing changes of climate which have extinguished species once common to the north and south, is evidenced by the fact that a number of Fuegian and South Chili plants extend northward as alpines to the very shores of the Gulf of Mexico, but do not inhabit the Mexican Andes, whilst as many Arctic species advance south to the Mexican Andes, but do not cross the intermediate depression and reappear in the Bolivian Andes.

evidence in the fact that, in the meridians of Australia and Japan, we have, first, the north-west coast of Australia sinking, together with the Louisiade Archipelago to its north; then, approaching the Line, the New Ireland group is sinking, as are also the Caroline Islands, in lat. 7° N. Beyond this, however, in lat. 15° N., are the Marianne Islands (rising), of whose vegetation nothing is known; in 27° N., the Bonin Islands (also rising); and in 30° N. is Japan, with which this botanical relationship exists.

It is objected by Mr. Darwin to this line of argument (as to that at p. xv concerning the Pacific Islands), that all these sinking areas are volcanic islands, having no traces of older rocks on them; but I do not see that this altogether invalidates the hypothesis, for many of the loftiest mountains throughout the Malayan Archipelago, New Zealand, and the Pacific Islands, are volcanic; some are active, and many attain 10–14,000 feet in elevation, whilst the lower portions of some of the largest of these islands are formed of rocks of various ages.

## § 4.

### *On the General Phenomena of the Distribution of Plants in Time.*

A third class of facts relates to the antiquity of vegetable forms and types on the globe, as evidenced by fossil plants. The chief facts relating to these are the following:—

31. The earliest Flora of which we know much scientifically, is that of the Carboniferous formation. We have indeed plants that belonged to an earlier vegetation, but they do not differ in any important respects from those of the carboniferous formation.

Now the ascertained features of the coal vegetation may be summed up very briefly. There existed at that time,—

*Filices;* in the main entirely resembling their modern representatives, and some of which may even be generically, though not specifically, identical with them.

*Lycopodiaceæ;* the same in their main characters as those now existing, and, though of higher specialization of stem, of greater stature, of different species, and perhaps also genera, from modern *Lycopodiaceæ,* yet identical with these in the structure of their reproductive organs and their contents, and in the minute anatomy of their tissues.

*Coniferæ.* The evidence of this Order is derived chiefly from the anatomical characters of the Dicotyledonous wood so abundantly found in the coal, and which seems to be identical in all important respects with the wood of modern genera of that Order, to which must be added the probability of *Trigonocarpon* and *Nœggerathia* being Gymnospermous, and allied to *Salisburia.** On the other hand, it must not be overlooked that no Coniferous strobili have been hitherto detected in the Carboniferous formation.

*Cycadeæ.* Some fragments of wood, presenting a striking similarity in anatomical characters to that of *Cycadeæ,* have been found in the carboniferous series.

In the absence of the fructification of *Calamites, Calamodendron, Halonia, Anabathra,* etc., there are no materials for any safe conclusions as to their immediate affinities, beyond that they all seem to be allied to Ferns or *Lycopodiaceæ;* but the same can hardly be said of the affinities of *Volkmannia,*† *Antholithes* and others, which have been referred, with more or less probability, to Angiospermous Dicotyledons.

The Permian Flora is for the most part specifically distinct from the Carboniferous, but many of

* Phil. Trans. 1855, p. 149. † See Quarterly Journal of Geological Society, May, 1854.

its genera are the same. The prevalent types are Gymnospermous Dicotyledons, especially *Cycadeæ*, and a great abundance of Tree-ferns.

The New Red Sandstone, or Trias group, presents plants more analogous to those of the Oolite than to those of the Carboniferous epoch, but they have also much in common with the latter. *Voltzia*, a remarkable genus of Conifers, appears to be peculiar to this period.

In the Lias numerous species of *Cycadeæ* have been found, with various Conifers and many Ferns. No other Dicotyledonous or any Monocotyledonous plants have as yet been discovered, but it is difficult to believe that none such should have existed at a period when wood-boring and herb-devouring insects, belonging to modern genera, were extremely abundant, as has been proved by the researches of Mr. Brodie and Mr. Westwood.*

The Oolite contains numerous *Cycadeæ*, *Coniferæ*, and Ferns, and more herbivorous genera of insects; and here Monocotyledonous vegetables are recognizable in *Podocarya* and other Pandaneous plants. A cone of *Pinus* has been discovered in the Purbeck, and one of *Araucaria* in the inferior Oolite of Somersetshire.

In the Cretaceous group, Dicotyledons of a very high type appear. A good many species are enumerated† by Dr. Debey, of Aix-la-Chapelle, including a species of *Juglans*, a genus belonging to an Order of highly-developed floral structure and complex affinities.‡

*Characeæ* appear for the first time at this epoch, and are apparently wholly similar in structure to those of the present day.

The Tertiary strata present large assemblages of plants of so many existing Genera and Orders, that it can hardly be doubted but that even the earliest Flora of that period was almost as complex and varied as that of our own. In the lowest Eocene beds are found *Anonaceæ*, *Nipa*, *Acacia*, and *Cucurbitaceæ*.§ In the Bagshot sands some silicified wood has been found, which may confidently be referred to *Banksia*, and which is, in fact, scarcely distinguishable from recent and fossil Australian Banksia wood.||

* These insects include species of the existing common European genera, *Elater*, *Gryllus*, *Hemerobius*, *Ephemera*, *Libellula*, *Panorpa*, and *Carabus*. Of all conspicuous tribes of plants the *Cycadeæ*, *Filices*, *Coniferæ*, and *Lycopodiaceæ* perhaps support the fewest insects, and the association of the above-named insects with a vegetation consisting solely or mainly of plants of these Orders is quite inconceivable.

† Quart. Journ. Geol. Soc. vii. pt. 1. misc. p. 110.

‡ Professor Oswald Heer, of Zurich, in an interesting little paper (Quelques Mots sur les Noyers), in Bibl. Univ. Genev. Sep. 1858, argues from the fact of the early appearance of *Juglans* in the geological series, that this genus must be a low type of the Dicotyledonous class to which it belongs. The position of *Juglans* is unsettled in the present state of our classification of Dicotyledonous Orders, as it has equal claims to be ranked with *Terebinthaceæ*, which are very high in the series, and with *Cupuliferæ*, which are placed very low; and were the grounds for our thus ranking these Orders based on characters of ascertained relative value, such an argument might be admissible; but the system which sunders these Orders is a purely artificial one, and *Juglans* with its allies would prove it so, if other proofs were wanting; for it absolutely combines *Terebinthaceæ* and *Cupuliferæ* into one natural group, in which (as in so many others) there is a gradual passage from great complexity of floral organs to great simplicity.

§ I am far from considering the identification of these and the other genera which I have enumerated in various strata as satisfactory, but I conclude that they may be taken as evidence of as highly developed and varied plants having then existed as are now represented by these genera.

|| I am indebted to the late Robert Brown for this fact, and for the means of comparing the specimens, which are beautifully opalized. I ascertained that he was satisfied with the evidence of this wood having really been dug up near Staines, though it is so perfectly similar in every respect to the opalized Banksia-wood of Tasmania as to suggest to his mind and my own the most serious doubts as to its English origin.

In the brown coal of the Eocene and Miocene periods, Fan-palms, Conifers, and various existing genera of *Myriceæ, Laurineæ,* and *Plataneæ* are believed to have been identified. Wesel and Weber describe from the brown coal of the Rhine a rich and varied Flora, representing numerous families never now seen associated, and including some of the peculiar and characteristic genera of the Australian, South African, American, Indian, and European Floras.*

In the Mollasse and certain Miocene formations at Œningen and elsewhere in Germany, Switzerland, and Tuscany,† 900 species of Dicotyledons‡ have been observed, all apparently different from existing ones. They have been referred, with more or less probability, to Fan-palms, Poplars (three species), evergreen *Laurineæ, Ceratonia, Acacia, Tamarindus, Banksia, Embothrium, Grevillea, Cupressus,* several species of *Juglans* (one near the North-American *J. acuminata,* another near the common Walnut of Europe and Asia, *J. nigra,* and a third near the North-American *J. cinerea*) ; also a Hickory, near the *Carya alba* (a genus now wholly American), and a *Pterocarya* closely allied to *P. Caucasica.*

The rise of the Alps was subsequent to this period; and in the European deposits immediately succeeding that event, in Switzerland (at Durnten and Utznach) are found evidences of the following existing species,—Spruce, Larch, Scotch Fir, Birch, a Hazel (different from that now existing), *Scirpus lacustris, Phragmites communis,* and *Menyanthes trifoliata.*

The glacial epoch followed, during and since which there has probably been little generic change in the vegetation of the globe.

32. So much for the main facts hitherto regarded as established in Vegetable Palæontology; they are of little value as compared with those afforded by the Animal Kingdom, even granting that they are all well made out, which is by no means the case. In applying them theoretically to the solution of the question of creation and distribution, the first point which strikes us is the impossibility of establishing a parallel between the successive appearances of vegetable forms in time, and their complexity of structure or specialization of organs, as represented by the successively higher groups in the Natural method of classification. Secondly, that the earliest recognizable Cryptogams

* See Quart. Journ. Geol. Soc. xv. misc. 3, where an abstract is given, with some excellent cautions, by C. J. F. Bunbury, Esq. The Australian genera include *Eucalyptus, Casuarina, Leptomeria, Templetonia, Banksia, Dryandra,* and *Hakea.* I am not prepared to assert that these identifications, or the Australian ones of the Mollasse, are all so unsatisfactory that the evidence of Australian types in the brown coal and Mollasse should be altogether set aside; but I do consider that not one of the above-named genera is identified at all satisfactorily, and that many of them are not even problematically decided.

† During the printing of this sheet I have received from my friend M. De Candolle a very interesting memoir on the tertiary fossil plants of Tuscany, by M. C. Gaudin and the Marquis C. Strozzi, in which some of the genera here alluded to are described. The age of these Tuscan beds is referred by Prof. O. Heer to a period intermediate between those of Utznach and Œningen. The most important plants described are, Coniferæ, 6 sp.; Salix, 2; Liquidambar, 1; Alnus, 1; Carpinus, 1; Populus, 2; Fagus, 1; Quercus, 5; Ulmus, 2; Planera, 1; Ficus, 1; Platanus, 1; Oreodaphne, 1; Laurus, 2; Persea, 1; Acer, 2; Vitis, 1; Juglans, 4; Carya, 1; Pterocarya, 1. There are 49 extinct species in all, of which 46 are referred, without even a mark of doubt or caution, to existing genera, and this in almost all cases from imperfect leaves alone! Without questioning the good faith or ability of the authors of this really valuable and interesting memoir, I cannot withhold my protest against this practice of making what are at best little better than surmises, appear under the guise of scientifically established identifications. What confidence can be placed in the positive reference of supposed fossil Fungi to *Sphæria,* or of pinnated leaves to *Sapindus,* and other fragments of foliage to existing genera of *Laurineæ, Ficus,* and *Vitis?*

‡ O. Heer, Sur les Charbons feuilletés de Durnten et Utznach, in Mem. Soc. Helvet. Sc. Nat. 1857; Bibl. Univers. Genev. August, 1858.

should not only be the highest now existing, but have more highly differentiated vegetative organs than any subsequently appearing; and that the dicotyledonous embryo and perfect exogenous wood with the highest specialized tissue known (the coniferous, with glandular tissue*), should have preceded the monocotyledonous embryo and endogenous wood in date of appearance on the globe, are facts wholly opposed to the doctrine of progression, and they can only be set aside on the supposition that they are fragmentary evidence of a time further removed from that of the origin of vegetation than from the present day; to which must be added the supposition that types of *Lycopodiaceæ*, and a number of other Orders and Genera, as low as those now living, existed at that time also.

Another point is the evidence,† said to be established, of genera now respectively considered peculiar to the five continents having existed cotemporaneously at a comparatively recent geological epoch in Europe, and the very close affinity, if not identity, of some of these with existing species. The changes in the level and contour of the different parts of the earth's surface which have occurred since the period of the chalk, or even since that preceding the rise of the Alps, imply a very great amount of difference between the past and present relations of sea and land and climate; and it is no doubt owing to these changes that the *Araucariæ*, which once inhabited England, are no longer found in the northern hemisphere, and that the Australian genera which inhabited Europe at a period preceding the rise of the Alps have since been expelled.

Such facts, standing at the threshold of our knowledge of vegetable palæontology, should lead us to expect that the problem of distribution is an infinitely complicated one, and suggest the idea that the mutations of the surface of our planet, which replace continents by oceans, and plains by mountains, may be insignificant measures of time when compared with the duration of some existing genera and perhaps species of plants, for some of these appear to have outlived the slow submersion of continents.

35. From the sum then of our theories, as arranged in accordance with ascertained facts, we may make the following assumptions:—That the principal recognized families of plants which inhabited the globe at and since the Palæozoic period still exist, and therefore have as families survived all intervening geological changes. That of these types some have been transferred, or have migrated, from one hemisphere to another. That it is not unreasonable to suppose that further evidence may be forthcoming which will show that all existing species may have descended genealogically from fewer pre-existing ones; that we owe their different forms to the variation of individuals, and the power of limiting them into genera and species to the destruction of some of these varieties, etc., and the increase in individuals of others. Lastly, that the fact of species being with so much uniformity the ultimate and most definable group (the leaves as it were of the family tree), may possibly be owing to the tendency to vary being checked, partly by the ample opportunities each brood of a

* The vexed question of the true position of Gymnospermous plants in the Natural System assumes a somewhat different aspect under the view of species being created by progressive evolution. In the haste to press the recent important discoveries in vegetable impregnation and embryogeny into the service of classification, the long-established facts regarding the development of the stem, flower, and reproductive organs themselves of Gymnospermous plants have been relatively underrated or wholly lost sight of; and if an examination of the doctrines of progression and variation lead to a better general estimation of the comparative value of the characters presented by these organs, the acceptance or rejection of the doctrines themselves is, in the present state of science, a matter of secondary importance.

† See first foot-note of p. xxi (*): what I have there said of the supposed identifications of the Australian genera applies to many of those of the other enumerated quarters of the globe.

variety possesses of being fertilized by the pollen of its nearest counterpart, partly by the temporary stability of its surrounding physical conditions, and partly by the superabundance of seeds shed by each individual, those only vegetating which are well suited to existing conditions: an appearance of stability is also, in the case of many perennials, due to the fact that the individuals normally attain a great age,* and thus survive many generations of other species, of which generations some present characters foreign to their parents.

36. In the above line of argument I have not alluded to the question of the origin of those families of plants which appear in the earliest geological formations, nor to that of vegetable life in the abstract, conceiving these to be subjects upon which, in the present state of science, botany throws no light whatever. Regarded from the classificatory point of view, the geological history of plants is not altogether favourable to the theory of progressive development, both because the earliest ascertained types are of such high and complex organization,† and because there are no known fossil plants which we can certainly assume to belong to a non-existing class or even family, nor that are ascertained to be intermediate in affinity between recent classes or families.‡

The progress of investigation may ultimately reveal the true history of the unrecognized vegetable remains with which our collections abound, and may discover to us amongst them new and unexpected organisms, suggesting or proving a progressive development; but in the meantime the fact remains that the prominent phenomena of vegetable palæontology do not advance us one step towards a satisfactory conception of the first origin of existing Natural Orders of plants.

Taking the Conifers as an example, whatever rank is given to them by the systematist, that they should have preceded Monocotyledons and many Dicotyledons in date of appearance on the globe, is a fact quite incompatible with progressive development in the scientific acceptation of the term, whilst to argue from their apparently early appearance that they are low in a classificatory system is begging the question.

Another fact to be borne in mind is, that we have no accurate idea of what systematic progression is in botany. We know little of high and low in the Vegetable Kingdom further than is expressed by the sequence of the three classes, Dicotyledons, Monocotyledons, and Acotyledons; and amongst Acotyledons, of Thallogens being lower than Acrogens, and of these that the Mosses, etc., are lower than Filices and their allies. It is true that we technically consider multiplication and complexity of floral whorls in phænogamic plants as indications of superior organization; but very many

* In considering the relative amount and rate at which different plants vary, it should be remembered that we habitually estimate them not only loosely but falsely. We assume annuals to be more variable than perennials, but we probably greatly overrate the amount to which they really are so, because a brief personal experience enables us to study many generations of an annual under many combinations of physical conditions; whereas the same experience embraces but a fractional period of the duration of (comparatively) very few perennials. It has also been well shown by Bentham (in his paper on the British Flora, read (1858) before the Linnæan Society) that an appearance of stability is given to many varieties of perennials, through their habitual increase by buds, offsets, etc., which propagate the individual; and in the case of *Rubi*, which comparatively seldom propagate by seed, a large tract of ground may be peopled by parts of a single individual.

† I have elsewhere stated that I consider the evidence of *Algæ* having existed at a period preceding vascular Cryptogams to be of very little value. (Lond. Journ. Bot. viii. p. 254.)

‡ It must not be supposed that in saying this I am even expressing a doubt as to there having been plants intermediate in affinity between existing Orders and Classes. Analogy with the animal kingdom suggests that some at any rate of the plants of the coal epoch do hold such a relationship; but should they not do so, I consider this fact to be of little value in the present inquiry, for I incline to believe that the ascertained geological history of plants embraces a mere fraction of their whole history.

of the Genera and Orders most deficient in these respects are so manifestly reduced members of others, which are indisputably the most complex in organization in the whole Vegetable Kingdom, that no good classification even has been founded on these considerations alone.*

37. Again, it is argued by both Mr. Darwin and Mr. Wallace that the general effects of variation by selection must be to establish a general progressive development of the whole animal kingdom. But here again in botany we are checked by the question, What is the standard of progression? Is it physiological or morphological? Is it evidenced by the power of overcoming physical obstacles to dispersion or propagation, or by a nice adaptation of structure or constitution to very restricted or complex conditions? Are cosmopolites to be regarded as superior to plants of restricted range, hermaphrodite plants to unisexual, parasites to self-sustainers, albuminous-seeded to exalbuminous, gymnosperms to angiosperms, water plants to land, trees to herbs, perennials to annuals, insular plants to continental? and, in fine, what is the significance of the multitudinous differences in point of structure and complexity, and powers of endurance, presented by the members of the Vegetable Kingdom, and which have no recognized physiological end and interpretation, nor importance in a classificatory point of view? It is extremely easy to answer any of these questions, and to support the opinion by a host of arguments, morphological, physiological, and teleological; but any one gifted with a quick perception of relations, and whose mind is stored with a sufficiency of facts, will turn every argument to equal advantage for both sides of the question.

To my mind, however, the doctrine of progression, if considered in connection with the hypothesis of the origin of species being by variation, is by far the most profound of all that have ever agitated the schools of Natural History, and I do not think that it has yet been treated in the unprejudiced spirit it demands. The elements for its study are the vastest and most complicated which the naturalist can contemplate, and reside in the comprehension of the reciprocal action of the so-called inorganic on the organic world. Granting that multiplication and specialization of organs is the evidence and measure of progression, that variation explains the *rationale* of the operation which results in this progression, the question arises, What are the limits to the combinations of physical causes which determine this progression, and how can the specializing power of Nature stop short of causing every race or family ultimately to represent a species? While the psychological philosophers persuade us that we see the tendency to specialize pervading every attribute of organic life, mental and physical; and the physicists teach that there are limits to the amount and duration of heat, light, and every other manifestation of physical force which our senses present or our intellects perceive, and which are all in process of consumption; the reflecting botanist, knowing that his ultimate results must accord with these facts, is perplexed at feeling that he has failed to establish on independent evidence the doctrines of variation and progressive specialization, or to co-ordinate his attempts to do so with the successive discoveries in physical science.

* The subject of the retrogression of types has never yet been investigated in botany, nor its importance estimated in inquiries of this nature. To whatever Order we may grant the dignity of great superiority or complexity, we find that Order containing groups of species of very simple organization; these are moreover often of great size and importance, and of wide geographical distribution. Such groups, if regarded *per se*, appear to be far lower in organization than other groups which are many degrees below them in the classified series; and our only clue to their real position is their evident affinity with their complex co-ordinates;—destroy the latter by a geological or other event, and all clue to the real position of the former may be lost. Are such groups of simply-constructed species created by retrogressive variation of the higher, or did the higher proceed from them by progressive variation? If the latter, did the simpler forms precede in origin the highest forms of all other groups which rank below them in the classified series?

38. Before dismissing this subject, I may revert once more to the opposite doctrine, which regards species as immutable creations, and this principally to observe that the arguments in its favour have neither gained nor lost by increased facilities for investigation, or by additional means for observation. The facts are unassailable that we have no direct knowledge of the origin of any wild species; that many are separated by numerous structural peculiarities from all other plants; that some of them invariably propagate their like; and that a few have retained their characters unchanged under very different conditions and through geological epochs. Recent discoveries have not weakened the force of these facts, nor have successive thinkers derived new arguments from them; and if we hence conclude from them that species are really independent creations and immutable, though so often illimitable, then is all further inquiry a waste of time, and the question of their origin, and that of their classification in Genera and Orders, can, in the present state of science, never be answered, and the only known avenues to all means of investigation must be considered as closed till the origin of life itself is brought to light.

39. Of these facts the most important, and indeed the only one that affords a tangible argument, is that of genetic resemblance. To the tyro in Natural History all similar plants may have had one parent, but all dissimilar plants must have had dissimilar parents. Daily experience demonstrates the first position, but it takes years of observation to prove that the second is not always true. There are, further, certain circumstances connected with the pursuit of the sciences of observation which tend to narrow the observer's views of the attributes of species; he begins by examining a few individuals of many extremely different kinds or species, which are to him fixed ideas, and the relationships of which he only discovers by patient investigation; he then distributes them into Genera, Orders, and Classes, the process usually being that of reducing a great number of dissimilar ideas under a few successively higher general conceptions; whilst with the history of the ideas themselves, that is, of species, he seldom concerns himself. In a study so vast as botany, it takes a long time for a naturalist to arrive at an accurate knowledge of the relations of Genera and Orders if he aim at being a good systematist, or to acquire an intimate knowledge of species if he aim at a proficiency in local Floras, and in both these pursuits the abstract consideration of the species itself is generally lost sight of; the systematist seldom returns to it, and the local botanist, who finds the minutest differences to be hereditary in a limited area, applies the argument derived from genetic resemblance to every hereditarily distinct form.

40. It has been urged against the theory that existing species have arisen through the variation of pre-existing ones and the destruction of intermediate varieties, that it is a hasty inference from a few facts in the life of a few variable plants, and is therefore unworthy of confidence, if not of consideration; but it appears to me that the opposite theory, which demands an independent creative act for each species, is an equally hasty inference from a few negative facts in the life of certain species,* of which some generations have proved invariable within our extremely limited experience. These theories must not, however, be judged of solely by the force of the very few absolute facts on which they are based; there are other considerations to be taken into account, and especially the conclusions to which they lead, and their bearing upon collateral biological phenomena, under which points of view the theory of independent creations appears to me to be greatly at a disadvantage; for according to it every fact and every phenomenon regarding the origin and continuance of species, but that of their occasional variation, and their extinction by natural causes, and regarding the *rationale* of classi-

* See paragraph 4, where I have stated that the grand total of unstable species probably exceeds that of the stable.

fication, is swallowed up in the gigantic conception of a power intermittently exercised in the development, out of inorganic elements, of organisms the most bulky and complex as well as the most minute and simple; and the consanguinity of each new being to its pre-existent nearest ally, is a barren fact, of no scientific significance or further importance to the naturalist than that it enables him to classify. The realization of this conception is of course impossible; the boldest speculator cannot realize the idea of a highly organized plant or animal starting into life within an area that has been the field of his own exact observation* and research; whilst the more cautious advocate hesitates about admitting the origin of the simplest organism under such circumstances, because it compels his subscribing to the doctrine of the "spontaneous generation" of living beings of every degree of complexity in structure and refinement of organization.

On the other hand, the advocate of creation by variation may have to stretch his imagination to account for such gaps in a homogeneous system as will resolve its members into genera, classes, and orders; but in doing so he is only expanding the principle which both theorists allow to have operated in the resolution of some groups of individuals into varieties: and if, as I have endeavoured to show, all those attributes of organic life which are involved in the study of classification, representation, and distribution, and which are barren facts under the theory of special creations, may receive a rational explanation under another theory, it is to this latter that the naturalist should look for the means of penetrating the mystery which envelopes the history of species, holding himself ready to lay it down when it shall prove as useless for the further advance of science, as the long serviceable theory of special creations, founded on genetic resemblance, now appears to me to be.

The arguments deduced from genetic resemblance being (in the present state of science), as far as I can discover, exhausted, I have felt it my duty to re-examine the phenomena of variation in reference to the origin of existing species; these phenomena I have long studied independently of this question, and when treating either of whole Floras or of species, I have made it my constant aim to demonstrate how much more important and prevalent this element of variability is than is usually admitted, as also how deep it lies beneath the foundations of all our facts and reasonings concerning classification and distribution. I have hitherto endeavoured to keep my ideas upon variation in subjection to the hypothesis of species being immutable, both because a due regard to that theory checks any tendency to careless observation of minute facts, and because the opposite one is apt to lead to a precipitate conclusion that slight differences have no significance; whereas, though not of specific importance, they may be of high structural and physiological value, and hence reveal affinities that might otherwise escape us. I have already stated how greatly I am indebted to Mr. Darwin's† *rationale* of the phenomena of variation and natural selection in the production of species; and though it does not positively establish the doctrine of creation by variation, I expect that every additional fact and observation relating to species will gain great additional value from being viewed in reference to it, and that it will materially assist in developing the principles of classification and distribution.

* It is a curious fact (illustrative of a well-known tendency of the mind), that the few writers who have in imagination endeavoured to push the doctrine of special creations to a logical issue, either place the scene of the creative effort in some unknown, distant, or isolated corner of the globe, removed far beyond the ken of scientific observation, or suppose it to have been enacted at a period when the physical conditions of the globe differed both in degree and kind from what now obtain; thus in both cases arguing *ad ignotum ab ignoto.*

† In this Essay I refer to the brief abstract only (Linn. Journ.) of my friend's views, not to his work now in the press, a deliberate study of which may modify my opinion on some points whereon we differ. Matured conclusions on these subjects are very slowly developed.

in the Botanical Gardens and Expeditions; and amongst private individuals, to Sir William M'Arthur; George M'Leay, Esq.; G. Bennett, Esq., and the distinguished naturalist, W. S. M'Leay, Esq., of Sydney.

P.S. At a meeting of the Linnæan Society, held on the 3rd of November, and after the printing of this Essay was completed, I heard an admirable paper read on the Geographical Distribution of Animals in the Malayan, New Guinea, and Australian continents and islands, by Mr. Alfred Wallace, who is still indefatigably investigating the zoology of those countries. The total absence of information as to the vegetation of New Guinea precludes my attempting any botanical corroboration of one of Mr. Wallace's most striking facts, viz. the complete difference between the zoology of Celebes and Borneo. These countries are separated by the Straits of Macassar, which are very deep, and the former belongs to the Australian zoological province, but the latter to the Malayan. The Straits of Lombok, to the south of those of Macassar, again, are, though only sixteen miles broad, also very deep, and separate in that latitude the Malayan from the Australian zoological province.

In Mr. Wallace's paper (which I have not seen) he appears to have adopted the same general views regarding the distribution of animals which I have promulgated for that of plants in the Introductory Essays to this and the New Zealand Flora; and establishes it on independent evidence of his own obtaining and of convincing strength. Mr. Wallace has further arrived independently at the same conclusion regarding the permanence of vegetable as compared with animal forms, which I have put forth at p. xii. in note.

I would further observe here, to avoid ambiguity, that my friend Mr. Darwin's just completed work "On the Origin of Species by Natural Selection," from the perusal of much of which in MS. I have profited so largely, had not appeared during the printing of this Essay, or I should have largely quoted it.

*Kew, November*, 4, 1859.