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ERRATA.

- P. 101, l. 4, for "ethyl," read "silver."
 " 102, l. 13, for "43.5," read "44.8" (as the half sum of equivalents U and Ni.)
 " 102, l. 25, for "Mg," read "Mn."
 " 107, l. 8, for "C," read "B."
 " 110, l. 2 from bottom, for "Co," read "Cr."
 " 111, l. 1, for "No," read "Nb."
 " 272, l. 8 from bottom, for "HENTZ," read "HEINTZ."
 Vol. XXVIII, p. 135, l. 33 from top, for Vol. VI, read Vol. IV.—P. 356, line 20 from bottom, after the word "cultivation," insert a period, putting a capital W for the word "where." Same page, line 19 from bottom, omit "then."—P. 357, line 10 from top, for "deposition of sand," read "deposition of mud."

THE
AMERICAN
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[SECOND SERIES.]

ART. I.—*On the Origination and Distribution of Species:—*
Introductory Essay to the Flora of Tasmania; by Dr. JOSEPH
*D. HOOKER.**

§ 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 extra-tropical 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

* *To the Editors of the American Journal of Science, &c.*—The sheets of this *Introductory Essay*, having been obligingly communicated to me in advance of the publication of the concluding part of the *Flora of Tasmania*, to which it belongs, I asked and have received the distinguished author's permission to reprint them, or a considerable portion of them, in your Journal, and now offer them for that purpose. This is in order that we may have before us, at the earliest date, an essay which cannot fail to attract the immediate and profound attention of scientific men; but which, if confined to the pages of the *Flora of Tasmania*, would be seen by very few American readers. To those who have intelligently observed the course of scientific investigation, and the tendency of speculation, it has for some time been manifest that a re-statement of the Lamarkian hypothesis is at hand. We have this, in an improved and truly scientific form, in the theories which, recently propounded by Mr. Darwin, followed by Mr. Wallace, are here so ably and altogether independently maintained. When these views are fully laid before them, the naturalists of this country will be able to take part in the interesting discussion which they will not fail to call forth.

To save room, a few paragraphs are omitted which do not directly bear upon the subject in hand.

A. G.

SECOND SERIES, VOL. XXIX, No. 85.—JAN., 1860.

2 *J. D. Hooker, Introductory Essay to the Flora of Tasmania.*

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 neighboring countries, and of the recently published hypotheses of Mr. Darwin and Mr. Wallace. * * *

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 fur-

ther 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 opinions; the progress of systematic science being, in short, obviously unfavorable 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

* 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.

4 *J. D. Hooker, Introductory Essay to the Flora of Tasmania.*

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 labor 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 '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 favorably 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 organism 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 favor of species being original creations, and genera, etc., arbitrarily limited groups of these.

The difference between varieties and species and genera in respect to definable limitation is however one of degree only, and if increased materials and observation confirm the doctrine which I have for many years labored 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 endeavor 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

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

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 color 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 these 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 northwestern Himalaya. A white and a rose-colored 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 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 accompanied 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

* 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 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 which is 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

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. *Orchidæ* and *Leguminosæ* are also well-limited orders (though not so absolutely as the former), but they, on the contrary, consist of comparatively exceedingly well-marked genera and species. *Melanthaceæ* and *Scrophulariæ*, 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.*

always recognized; these may be called *objective* orders; *Orchidæ* 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.

* 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.

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 (Introduct. 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 behavior 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.†

* 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 also as if they were 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.

10 J. D. Hooker, *Introductory Essay to the Flora of Tasmania.*

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 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 support 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 Kohlrabi, 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

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

† 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 their any disposition in the neglected cultivated races to revert to the wild form.

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 vigor during its life as any wild variety is, but its offspring 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 ob-

* 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.

servations 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 favors longevity and apparent permanence of specific type. The ultimate effect of all these operations is of course favorable 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 favorable to the production of a hybrid, and unfavorable 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, or by that of any other plant,† they eventu-

* 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 'Gardener's Chronicle,' 1858, p. 828.

† 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

ally 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 favor 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

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 fonctions 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.

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 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.

The universality of this feature (of groups having defined areas) affords to my mind all but conclusive evidence in favor 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

* 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.

hold its own, and not succumb to the enervating or etiolating or smothering influences of its neighbors. 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 Northwestern 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 of organs in plants, where many are present, and where those of low morphological importance may have a comparatively high physiological significance.

18 *J. D. Hooker, Introductory Essay to the Flora of Tasmania.*

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 unfavorable 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 favorable 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, color, 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 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 Northwest 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.

* 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.

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 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,

* Linn. Trans., xx, 235.

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

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, in the whole of the group including the Low Archipelago and the Society Islands, extending over more than 2000 miles, I observe but one 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 favorably 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.

* 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) uncolored, on account of the doubtful evidence regarding it, which however is in favor of its being in the same condition as Elizabeth's 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. [A specimen of this plant was gathered by Prof. Dana on the mountains of Tahiti.—Eds.]

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 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 28° 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 endeavored 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 to 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 temperate 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.*

* 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 redistribution 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.

29. It remains then to examine whether, supposing the glacial epoch 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 evidence in the fact that, in the meridian 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

* 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 alpine 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.

Bonin Islands (also rising); and in 30° is Japan, with which this botanical relationship exists.

It is objected by Mr. Darwin to this line of argument (as to that on p. 15, 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 to 14,000 feet in elevation, whilst the lower portions of some of the largest of these islands are formed of rocks of various ages.

(To be continued.)

ART. II.—*Some General Views on Archæology*; by A. MORLOT.*

A CENTURY scarcely has elapsed since the time when it would have been thought impossible to reconstruct the history of our globe, prior to the appearance of mankind. But, though contemporary historians were wanting during this immense pre-human era, the latter has not failed in leaving us a well-arranged series of most significant vestiges: the animal and vegetable tribes, which have successively appeared and disappeared, have left their fossil remains in the successively deposited strata. Thus has been composed, gradually and slowly, a history of creation, written, as it were, by the Creator himself. It is a great book, the leaves of which are the stratified rocks, following each other in the strictest chronological order, the chapters being the mountain-chains. This great book has long been closed to man. But science, constantly extending its realm and improving its method of induction, has taught the geologist to study those marvellous archives of creation, and we behold him now unfolding the past ages of our world, with a variety of details and a certainty of conclusions well calculated to inspire us with grateful admiration.

The development of archæology has been very similar to that of geology. Not long ago we should have smiled at the idea of reconstructing the by-gone days of our race, previous to the first beginning of history properly so called. The void was filled up, partly by representing that ante-historical antiquity as having been only of short duration, and partly by exaggerating the value and the age of those vague and confused notions which constitute tradition.

* This article is an introduction to a paper entitled, *Geologico-Archæological Studies in Denmark and Switzerland*, appearing in the *Bulletin de la Société Vaudoise des Sciences Naturelles*, for 1859, and of which a separate edition, comprising the present pages, will be published.

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