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VIII.—*Species considered as to Variation, Geographical Distribution, and Succession.* By Prof. Asa Gray*.

It is well known to botanists that M. DeCandolle has been assiduously engaged in the elaboration of the order Cupuliferæ for the 'Prodromus,' and has had before him the authentic types of almost every published species, and an amount of materials as to many of them which, so far as dried specimens may serve, leaves little to be asked. A less inspiring task could hardly be assigned to a botanist than the systematic elaboration of the genus *Quercus* and its allies. The vast materials assembled under DeCandolle's hands, while disheartening for their bulk, offered small hope of novelty. The subject was both extremely trite and extremely difficult. Happily, it occurred to DeCandolle that an interest might be imparted to an onerous undertaking, and a work of necessity be turned to good account for science, by studying the Oaks in view of the question of *Species*.

What this term *Species* means, or should mean, in natural history, what the limits of species, *inter se* or chronologically, or in geographical distribution, their modifications, actual or probable, their origin, and their destiny,—these are questions which surge up from time to time; and now and then, in the progress of science, they come to assume a new and hopeful interest. Botany and zoology, geology and what our author, feeling the want of a new term, proposes to name *Epiontology*†,

* From Silliman's American Journal for May 1863.

† A name which, at the close of his article, DeCandolle proposes for *the study of the succession of organized beings*, to comprehend therefore palæontology and all included under what is called geographical botany and zoology, the whole forming a science parallel to geology,—the latter devoted to the history of unorganized bodies, the former to that of or-

all lead up to and converge into this class of questions, while recent theories shape and point the discussion. So we look with eager interest to see what light the study of the Oaks, by a very careful, experienced, and conservative botanist, particularly conversant with the geographical relations of plants, may throw upon the subject.

The course of investigation in this instance does not differ from that ordinarily pursued by working botanists; nor, indeed, are the theoretical conclusions other than those to which a similar study of other orders might equally have led. The Oaks afford a very good occasion for the discussion of questions which press upon our attention, and perhaps they offer peculiarly good materials, on account of the number of fossil species.

Preconceived notions about species being laid aside, the specimens in hand were distributed, according to their obvious resemblances, into groups of apparently identical or nearly identical forms, which were severally examined and compared. Where specimens were few, as from countries little explored, the work was easy, but the conclusions, as will be seen, of small value. The fewer the materials, the smaller the likelihood of forms intermediate between any two, and, what does not appear being treated upon the old law-maxim as non-existent, species are readily enough defined. Where, however, specimens abound, as in the case of the Oaks of Europe, of the Orient, and of the United States, of which the specimens amounted to hundreds, collected at different ages, in varied localities, by botanists of all sorts of views and predilections, here alone were data fit to draw useful conclusions from. Here, as DeCandolle remarks, he had every advantage, being furnished with materials more complete than any one person could have procured from his own herborizations, more varied than if he had observed a hundred times over the same forms in the same district, and more impartial than if they had all been amassed by one person with his own ideas or predispositions. So that vast herbaria, into which contributions from every source have flowed for years, furnish the best possible data—at least are far better than any practicable amount of personal herborization—for the comparative study of related forms occurring over wide tracts of territory. But as the materials increase, so do the difficulties. Forms which appeared totally distinct approach or blend through intermediate gradations; characters stable in a limited number of instances,

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ganized beings, as respects origin, distribution, and succession. We are not satisfied with the word, notwithstanding the precedent of *palæontology*, since *ontology*, the science of being, has an established meaning as referring to mental existence, *i. e.* is a synonym or a department of metaphysics.

or in a limited district, prove unstable occasionally, or when observed over a wider area; and the practical question is forced upon the investigator, What here is probably fixed and specific, and what is variant, pertaining to individual, variety, or race?

In the examination of these rich materials, certain characters were found to vary upon the same branch, or upon the same tree, sometimes according to age or development, sometimes irrespective of such relations or of any assignable reasons. Such characters, of course, are not specific, although many of them are such as would have been expected to be constant in the same species, and are such as generally enter into specific definitions. Variations of this sort DeCandolle, with his usual painstaking, classifies and tabulates, and even expresses numerically their frequency in certain species. The results are brought well to view in a systematic enumeration.

(1.) Of characters which *frequently* vary upon the same branch: upwards of a dozen such are mentioned.

(2.) Of those which *sometimes* vary upon the same branch: a smaller number of these are mentioned.

(3.) Those so rare that they might be called monstrosities.

Then he enumerates characters, ten in number, which he has never found to vary on the same branch, and which, therefore, have better claim to be employed as specific. But, as among them he includes the duration of the leaves, the size of the cupule, and the form and size of its scales, which are by no means wholly uniform in different trees of the same species, even these characters must be taken with allowance. In fact, having first brought together, as groups of the lowest order, those forms which varied upon the same stock, he next had to combine similarly various forms which, though not found associated upon the same branch, were thoroughly blended by intermediate degrees.

"The lower groups (varieties or races) being thus constituted, I have given the rank of *species* to the groups next above these, which differ in other respects, *i. e.* either in characters which were not found united upon certain individuals, or in those which do not show transitions from one individual to another. For the Oaks of regions sufficiently known, the species thus formed rest upon satisfactory bases, of which the proof can be furnished. It is quite otherwise with those which are represented in our herbaria by single or few specimens. These are *provisional species*—species which may hereafter fall to the rank of simple varieties. I have not been inclined to prejudge such questions; indeed in this regard I am not disposed to follow those authors whose tendency is, as they say, to reunite species. I never reunite them without proof in each particular case; while the botanists to whom I refer do so on the ground of analogous variations or transitions occurring in the same genus or in the same family. For example, resting on the fact that *Quercus Ilex*, *Q. cocci-*

fera, *Q. acutifolia*, &c. have the leaves sometimes entire and sometimes toothed, upon the same branch, or present transitions from one tree to another, I might readily have united my *Q. Tlapuxahuensis* to *Q. Sartorii* of Liebmann, since these two differ only in their entire or their toothed leaves. From the fact that the length of the peduncle varies in *Q. robur* and many other Oaks, I might have combined *Q. Seemanni*, Liebm., with *Q. salicifolia*, Nees. I have not admitted these inductions, but have demanded visible proof in each particular case. Many species are thus left as provisional; but in proceeding thus, the progress of the science will be more regular, and the synonymy less dependent upon the caprice or the theoretical opinions of each author."

This is safe and, to a certain degree, judicious, no doubt, as respects published species. Once admitted, they may stand until they are put down by evidence, direct or circumstantial. Surely a species may rightfully be condemned on good circumstantial evidence. But what course does DeCandolle pursue in the case, of every-day occurrence to most working botanists having to elaborate collections from countries not so well explored as Europe, when the forms in question, or one of the two, are as yet unnamed? Does he introduce as a new species every form which he cannot connect by ocular proof with a near relative from which it differs only in particulars which he sees are inconstant in better-known species of the same group? We suppose not. But if so, little improvement for the future upon the state of things revealed in the following paragraph can be expected.

"In the actual state of our knowledge, after having seen nearly all the original specimens, and in some species as many as 200 representatives from different localities, I estimate that, out of the 300 species of *Cupuliferæ* which will be enumerated in the 'Prodromus,' two-thirds at least are *provisional* species. In general, when we consider what a multitude of species were described from a single specimen, or from the forms of a single locality, of a single country, or are badly described, it is difficult to believe that above one-third of the actual species in botanical works will remain unchanged."

Such being the results of the *want* of adequate knowledge, how is it likely to be when our knowledge is largely increased? The judgment of so practised a botanist as DeCandolle is important in this regard; and it accords with that of other botanists of equal experience.

"They are mistaken," he pointedly asserts, "who repeat that the greater part of our species are clearly limited, and that the doubtful species are in a feeble minority. This seemed to be true so long as a genus was imperfectly known, and its species

were founded upon few specimens—that is to say, were provisional. Just as we come to know them better, intermediate forms flow in, and doubts as to specific limits augment."

DeCandolle insists, indeed, in this connexion, that the higher the rank of the groups, the more definite their limitation, or, in other terms, the fewer the ambiguous or doubtful forms,—that genera are more strictly limited than species, tribes than genera, orders than tribes, &c. We are not convinced of this. Often, where it has appeared to be so, advancing discovery has brought intermediate forms to light, perplexing to the systematist. "They are mistaken," we think more than one systematic botanist will say, "who repeat that the greater part of our natural orders and tribes are absolutely limited," however we may agree that we will limit them. Provisional genera, we suppose, are proportionally hardly less common than provisional species; and hundreds of genera are kept up on considerations of general propriety or general convenience, although well known to shade off into adjacent ones by complete gradations. Somewhat of this greater fixity of higher groups, therefore, is rather apparent than real. On the other hand, that varieties should be less definite than species, follows from the very terms employed. They are ranked as varieties rather than species, just because of their less definiteness.

Singular as it may appear, we have heard it denied that spontaneous varieties occur. DeCandolle makes the important announcement that, in the Oak genus, the best-known species are just those which present the greatest number of spontaneous varieties and subvarieties. The maximum is found in *Q. robur*, with twenty-eight varieties, all spontaneous. Of *Q. Lusitanica* eleven varieties are enumerated, of *Q. Calliprinos* ten, of *Q. coccifera* eight, &c. And he significantly adds that "these very species which offer such numerous modifications are themselves ordinarily surrounded by other forms provisionally called species because of the absence of known transitions, or variations, but to which some of these will probably have to be joined hereafter." The inference is natural, if not inevitable, that the difference between such species and such varieties is only one of degree, either as to amount of divergence or of hereditary fixity, or as to the frequency or rarity, at the present time, of intermediate forms.

This brings us to the second section of DeCandolle's article, in which he passes on, from the observation of the present forms and affinities of Cupuliferous plants, to the consideration of their probable history and origin. Suffice it to say that he frankly accepts the inferences derived from the whole course of observation, and even contemplates with satisfaction a probable

historical connexion between congeneric species. He accepts and, by various considerations drawn from the geographical distribution of European *Cupuliferæ*, fortifies the conclusion (long ago arrived at by Edward Forbes) that the present species, and even some of their varieties, date back to about the close of the Tertiary epoch, since which time they have been subject to frequent and great changes of habitation or limitation, but without appreciable change of specific form or character,—that is, without profounder changes than those within which a species, at the present time, is known to vary. Moreover he is careful to state that he is far from concluding that the time of the appearance of a species in Europe at all indicates the time of its origin. Looking back still further into the Tertiary epoch, of which the vegetable remains indicate many analogous, but few, if any, identical forms, he concludes, with Heer and others, that specific changes of form, as well as changes of station, are to be presumed. And finally, that "the theory of a succession of forms through the deviation of anterior forms is the most natural hypothesis, and the most accordant with the known facts in palæontology, geographical botany, and zoology, of anatomical structure and classification; but direct proof of it is wanting; and moreover, if true, it must have taken place very slowly—so slowly, indeed, that its effects are discernible only after a lapse of time far longer than our historic epoch."

In contemplating the present state of the species of *Cupuliferæ* in Europe, DeCandolle comes to the conclusion that, while the Beech is increasing, and extending its limits southward and westward (at the expense of *Coniferæ* and Birches), the common Oak, to some extent, and the Turkey Oak decidedly, are diminishing and retreating,—and this wholly irrespective of man's agency. This is inferred of the Turkey Oak from the great gaps found in its present geographical area, which are otherwise inexplicable, and which he regards as plain indications of a partial extinction. Community of descent of all the individuals of species is of course implied in these and all similar reasonings.

An obvious result of such partial extinction is clearly enough brought to view. The European Oaks (like the American species) greatly tend to vary; that is, they manifest an active disposition to produce new forms. Every form tends to become hereditary, and so to pass from the state of mere variation to that of race; and of these competing incipient races some only will survive. *Quercus robur* offers a familiar illustration of the manner in which one form may, in the course of time, become separated into two or more distinct ones.

To Linnæus this Common Oak of Europe was all of one species. But of late years the greater number of European botanists

have regarded it as including three species, *Q. pedunculata*, *Q. sessiliflora*, and *Q. pubescens*. DeCandolle looks with satisfaction to the independent conclusion which he reached from a long and patient study of the forms (and which Webb, Gay, Bentham, and others had equally reached), that the view of Linnæus was correct, inasmuch as it goes to show that the idea and the practical application of the term *species* have remained unchanged during the century which has elapsed since the publication of the 'Species Plantarum.' But, the idea remaining unchanged, the facts might appear under a different aspect, and the conclusion be different, under a slight and very supposable change of circumstances. Of the twenty-eight spontaneous varieties of *Q. robur* which DeCandolle recognizes, all but six, he remarks, fall naturally under the three subspecies, *pedunculata*, *sessiliflora*, and *pubescens*, and are therefore forms grouped around these as centres; and, moreover, the few connecting forms are by no means the most common. Were these to die out, it is clear that the three forms which have already been so frequently taken for species would be what the group of four or five provisionally admitted species which closely surround *Q. robur* (see p. 85) now are. The best example of such a case, as having in all probability occurred through geographical segregation and partial extinction, is that of the Cedar, thus separated into the Deodar, the Lebanon, and the Atlantic Cedars—a case admirably worked out by Dr. Hooker two or three years ago*.

A special advantage of the *Cupuliferæ* for determining the probable antiquity of existing species in Europe, DeCandolle finds in the size and character of their fruits. However it may be with other plants (and he comes to the conclusion generally that marine currents and all other means of distant transport have played only a very small part in the actual dispersion of species), the transport of acorns and chestnuts by natural causes across an arm of the sea, in a condition to germinate (and much more the spontaneous establishment of a forest of oaks or chestnuts in this way), DeCandolle conceives to be fairly impossible in itself, and contrary to all experience. From such considerations, *i. e.* from the actual dispersion of the existing species, with occasional aid from Post-tertiary deposits, it is thought to be shown that the principal *Cupuliferæ* of the Old World attained their actual extension before the present separation of Sicily, Sardinia, and Corsica, or of Britain, from the European continent.

This view once adopted, and this course once entered upon, has to be pursued further. *Quercus robur* of Europe, with its

* Nat. Hist. Review, January 1862; see Sillimann's Journal, ser. 2. vol. xxiv. p. 148.

bevy of admitted derivatives, and its attending species only provisionally admitted to that rank, is very closely related to certain species of Eastern Asia, and of Oregon and California—so closely that "a view of the specimens by no means forbids the idea that they have all originated from *Q. robur*, or have originated, with the latter, from one or more preceding forms so like the present ones that a naturalist could hardly know whether to call them species or varieties." Moreover there are fossil leaves from diluvian deposits in Italy, figured by Gaudin, which are hardly distinguishable from those of *Q. robur*, on the one hand, and from those of *Q. Douglasii*, &c., of California, on the other. No such leaves are found in any Tertiary deposit in Europe; but such are found of that age, it appears, in North-west America, where their remote descendants still flourish. So that the probable genealogy of *Q. robur*, traceable in Europe up to the commencement of the present epoch, looks eastward and far into the past on far distant shores.

Q. Ilex, the Evergreen Oak of Southern Europe and Northern Africa, reveals a similar archæology; but its presence in Algeria leads DeCandolle to regard it as a much more ancient denizen of Europe than *Q. robur*; and a Tertiary Oak (*Q. ilicoides*), from a very old Miocene bed in Switzerland, is thought to be one of its ancestral forms. This high antiquity once established, it follows, almost of course, that the very nearly related species in Central Asia, in Japan, in California, and even our own Live Oak with its Mexican relatives, may probably enough be regarded as early offshoots from the same stock with *Q. Ilex*.

In brief, not to continue these abstracts and remarks, and without reference to Darwin's particular theory (which DeCandolle at the close very fairly considers), if existing species, or many of them, are as ancient as they are now generally thought to be, and were subject to the physical and geographical changes (among them the coming and the going of the Glacial epoch) which this antiquity implies—if in former times they were as liable to variation as they now are—and if the individuals of the same species may claim a common local origin, then we cannot wonder that "the theory of a succession of forms by deviations from anterior forms" should be regarded as "the most natural hypothesis," nor at the general advance made towards its acceptance in some form or other.

The question being, not how plants and animals originated, but how came the existing animals and plants to be just where they are and what they are, it is plain that naturalists interested in such inquiries are mostly looking for the answer in one direction. The general drift of opinion, or at least of expectation, is exemplified by this essay of DeCandolle; and the set and force

of the current are seen by noticing how it carries along naturalists of widely different views and prepossessions, some faster and further than others, but all in one way. The tendency is, we may say, to extend the law of continuity, or something analogous to it, from inorganic to organic nature, and in the latter to connect the present with the past in some sort of material connexion. The generalization may, indeed, be expressed so as not to assert that the connexion is genetic, as in Mr. Wallace's formula: "Every species has come into existence coincident both in time and space with preexisting closely allied species." Edward Forbes, who may be called the originator of this whole line of inquiry, long ago expressed a similar view. But the only material sequence we know, or can clearly conceive, in plants and animals is that from parent to progeny; and, as DeCandolle implies, the origin of species and that of races can hardly be much unlike, nor governed by other than the same laws, whatever these may be.

The progress of opinion upon this subject in one generation is not badly represented by that of DeCandolle himself, who is by no means prone to adopt new views without much consideration. In an elementary treatise, published in the year 1835, he adopted and, if we rightly remember, vigorously maintained, Schouw's idea of the double or multiple origin of species, at least of some species—a view which has been carried out to its ultimate development only perhaps by Agassiz, in the denial of any necessary genetic connexion among the individuals of the same species, or of any original localization more restricted than the area now occupied by the species. But in 1855, in his 'Géographie Botanique,' the multiple hypothesis, although in principle not abandoned, is seen to lose its point, in view of the probable high antiquity of existing species. The actual vegetation of the world being now regarded as a continuation, through numerous geological, geographical, and more recently historical changes, of anterior vegetations, the actual distribution of plants is seen to be a consequence of preceding conditions and geological considerations; and these alone may be expected to explain all the facts, many of them so curious and extraordinary, of the actual geographical distribution of the species. In the present essay, not only the distribution, but the origin, of congeneric species is regarded as something derivative: whether derived by slow and very gradual changes in the course of ages, according to Darwin, or by a sudden inexplicable change of their Tertiary ancestors, as conceived by Heer, DeCandolle hazards no opinion. It may, however, be inferred that he looks upon "natural selection" (which he rather underrates) as a real but insufficient cause; while some curious remarks (pp. 57, 58) upon

the number of monstrosities annually produced, and the possibility of their enduring, may be regarded as favourable to Heer's view.

As an index to the progress of opinion in the direction referred to, it will be interesting to compare Sir Charles Lyell's well-known chapters of twenty or thirty years ago, in which the permanence of species was ably maintained, with his treatment of the same subject in a work just issued in England, which, however, has not yet reached us.

A belief in the derivation of species may be maintained along with a conviction of great persistence of specific characters. This is the idea of the excellent Swiss vegetable palæontologist, Heer, who imagines a sudden change of specific type at certain periods; and it perhaps is that of Pictet. Falconer adheres to somewhat similar views in his elaborate paper on Elephants, living and fossil, in the 'Natural History Review' for January 1863. Noting that "there is clear evidence of the true Mammoth having existed in America long after the period of the northern drift, when the surface of the country had settled down into its present form," and also in Europe so late as to have been a cotemporary of the Irish Elk, and, on the other hand, that it existed in England so far back as before the deposition of the Boulder Clay, also that four well-defined species of fossil Elephant are known to have existed in Europe, that "a vast number of the remains of three of these species have been exhumed over a large area in Europe, and, even in the geological sense, an enormous interval of time has elapsed between the formation of the most ancient and the most recent of these deposits, quite sufficient to test the persistence of specific characters in an Elephant," he presents the question, "Do, then, the successive Elephants occurring in these strata show any signs of a passage from the older form into the newer?"

To which the reply is, "If there is one fact which is impressed on the conviction of the observer with more force than any other, it is the persistence and uniformity of the characters of the molar teeth in the earliest known Mammoth and his most modern successor.....Assuming the observation to be correct, what strong proof does it not afford of the persistence and constancy, throughout vast intervals of time, of the distinctive characters of those organs which are most concerned in the existence and habits of the species? If we cast a glance back on the long vista of physical changes which our planet has undergone since the Neozoic epoch, we can nowhere detect signs of a revolution more sudden and pronounced, or more important in its results, than the intercalation and sudden disappearance of the glacial period. Yet the 'dicyclotherian' Mammoth lived before it, and

passed through the ordeal of all the hard extremities it involved, bearing his organs of locomotion and digestion all but unchanged. Taking the group of four European fossil species above enumerated, do they show any signs in the successive deposits of a transition from the one form into the other? Here, again, the result of my observation, in so far as it has extended over the European area, is, that the specific characters of the molars are constant in each, within a moderate range of variation, and that we nowhere meet with intermediate forms."……Dr. Falconer continues (p. 80):—

"The inferences which I draw from these facts are not opposed to one of the leading propositions of Darwin's theory. With him, I have no faith in the opinion that the Mammoth and other extinct Elephants made their appearance suddenly, after the type in which their fossil remains are presented to us. The most rational view seems to be, that they are in some shape the modified descendants of earlier progenitors. But if the asserted facts be correct, they seem clearly to indicate that the older Elephants of Europe, such as *E. meridionalis* and *E. antiquus*, were not the stocks from which the later species, *E. primigenius* and *E. africanus*, sprang, and that we must look elsewhere for their origin. The nearest affinity, and that a very close one, of the European *E. meridionalis* is with the Miocene *E. planifrons* of India, and of *E. primigenius* with the existing Indian species.

"Another reflection is equally strong in my mind—that the means which have been adduced to explain the origin of species by 'natural selection,' or a process of variation from external influences, are inadequate to account for the phenomena. The law of phyllotaxis, which governs the evolution of leaves around the axis of a plant, is as nearly constant in its manifestation as any of the physical laws connected with the material world. Each instance, however different from another, can be shown to be a term of some series of continued fractions. When this is coupled with the geometrical law governing the evolution of form, so manifest in some departments of the animal kingdom (*e. g.* the spiral shells of the Mollusca), it is difficult to believe that there is not in nature a deeper-seated and innate principle, to the operation of which natural selection is merely an adjunct. The whole range of the Mammalia, fossil and recent, cannot furnish a species which has had a wider geographical distribution, and passed through a longer term of time, and through more extreme changes of climatal conditions, than the Mammoth. If species are so unstable, and so susceptible of mutation through such influences, why does that extinct form stand out so signally a monument of stability? By his admirable researches and earnest writings, Darwin has, beyond all his cotemporaries, given an impulse to the philosophical investigation of the most backward and obscure branch of the biological sciences of his day: he has laid the foundations of a great edifice; but he need not be surprised if, in the pro-

gress of erection, the superstructure is altered by his successors, like the Duomo of Milan from the Roman to a different style of architecture."

Entertaining ourselves the opinion that something more than natural selection is requisite to account for the orderly production and succession of species, we offer two incidental remarks upon the above extract.

First, we find in it, in the phrase "natural selection, or a process of variation from external influences," an example of the very common confusion of two distinct things, viz. *variation* and *natural selection*. The former has never yet been shown to have its cause in "external influences," nor to occur at random. As we have elsewhere insisted, if not inexplicable, it has never been explained: all we can yet say is, that plants and animals are prone to vary, and that some conditions favour variation. Perhaps in this Dr. Falconer may yet find what he seeks: for "it is difficult to believe that there is not in [its] nature a deeper-seated and innate principle, to the operation of which natural selection is merely an adjunct." The latter, which is the *ensemble* of the external influences, including the competition of the individuals themselves, picks out certain variations as they arise, but in no proper sense can be said to originate them.

Secondly, although we are not quite sure how Dr. Falconer intends to apply the law of phyllotaxis to illustrate his idea, we fancy that a pertinent illustration may be drawn from it in this way. There are two *species* of phyllotaxis, perfectly distinct, and, we suppose, not mathematically reducible the one to the other,—viz. (1), that of alternate leaves, with its varieties; and (2) that of verticillate leaves, of which opposite leaves present the simplest case. That, although generally constant, a change from one variety of alternate phyllotaxis to another should occur on the same axis, or on successive axes, is not surprising, the different sorts being terms of a regular series—although, indeed, we have not the least idea as to how the change from the one to the other comes to pass. But it is interesting, and in this connexion perhaps instructive, to remark that, while some dicotyledonous plants hold to the verticillate (*i. e.* opposite-leaved) phyllotaxis throughout, a larger number (through the operation of some deep-seated and innate principle, which we cannot fathom) change abruptly into the other species at the second or third node, and change back again in the flower, or else effect a synthesis of the two species in a manner which is puzzling to understand. Here is a change from one fixed law to another, as unaccountable, if not as great, as from one specific form to another.

An elaborate paper on the vegetation of the Tertiary period, in the south-east of France, by Count Gaston de Saporta, published in the 'Ann. Sc. Nat.' in 1862 (vol. xvi. pp. 309–344) which we have not space to analyse, is worthy of attention from the general inquirer, on account of its analysis of the Tertiary flora into its separate types—Cretaceous, Austral, Tropical, and Boreal—each of which has its separate and different history; and for the announcement that "the *hiatus* which, in the idea of most geologists, intervened between the close of the Cretaceous and the beginning of the Tertiary appears to have had no existence, so far as concerns the vegetation; that in general it was not by means of a total overthrow, followed by a complete new emission of species, that the flora has been renewed at each successive period; and that while the plants of Southern Europe inherited from the Cretaceous period more or less rapidly disappeared, as also the austral forms, and later the tropical types (except the Laurel, the Myrtle, and the *Chamærops humilis*), the boreal types, coming later, survived all the others, and now compose, either in Europe, or in the north of Asia, or in North America, the basis of the actual arborescent vegetation. Especially "a very considerable number of forms nearly identical with Tertiary forms now exist in America, where they have found, more easily than in our [European] soil (less vast and less extended southward), refuge from ulterior revolutions." The extinction of species is attributed to two kinds of causes—the one material or physical, whether slow or rapid, the other inherent in the nature of organic beings, incessant, but slow, in a manner latent, but somehow, assigning to the species, as to the individuals, a limited period of existence, and, in some equally mysterious but wholly natural way, connected with the development of organic types—"by *type* meaning a collection of vegetable forms constructed upon the same plan of organization, of which they reproduce the essential lineaments with certain secondary modifications, and which appear to run back to a common point of departure."

In this community of types, no less than in the community of certain existing species, Saporta recognizes a prolonged material union between North America and Europe in former times. Most naturalists and geologists reason in the same way, some more cautiously than others; yet perhaps most of them seem not to perceive how far such inferences imply the doctrine of the common origin of related species.

For obvious reasons such doctrines are likely to find more favour with botanists than with zoologists. But with both the advance in this direction is seen to have been rapid and great, yet to us not unexpected. We note also an evident disposition,

notwithstanding some endeavours to the contrary, to allow derivative hypotheses to stand or fall upon their own merits, to have, indeed, upon philosophical grounds, certain presumptions in their favour, and to be, perhaps, quite as capable of being turned to good account as to bad account in natural theology*.

Among the leading naturalists, indeed, such views, taken in the widest sense, have one (and, so far as we are now aware, only one) thoroughgoing and thoroughly consistent opponent, viz. M. Agassiz.

Most naturalists take into their very conception of a species, explicitly or by implication, the notion of a material connexion resulting from the descent of the individuals composing it from a common stock, of local origin. M. Agassiz wholly eliminates community of descent from his idea of species, and even conceives a species to have been as numerous in individuals and as widespread over space, or as segregated in discontinuous spaces, from the first as at a later period.

The station which it inhabits, therefore, is with other naturalists in nowise essential to the species, and may not have been the region of its origin. In M. Agassiz's view the habitat is supposed to mark the origin, and to be a part of the character, of the species. The habitat is not merely the place where it is, but a part of what it is.

Most naturalists recognize varieties of species; and many, like DeCandolle, have come to conclude that varieties of the highest grade, or races, so far partake of the characteristics of species, and are so far governed by the same laws, that it is often very difficult to draw a clear and certain distinction between the two. M. Agassiz will not allow that varieties or races exist in nature, apart from man's agency.

Most naturalists believe that the origin of species is supernatural, their dispersion or particular geographical area natural, and their extinction, when they disappear, also the result of physical causes. In the view of M. Agassiz, if rightly under-

* What the Rev. Principal Tulloch remarks in respect to the philosophy of miracles has a pertinent application here. We quote at secondhand:—

"The stoutest advocates of interference can mean nothing more than that the Supreme Will has so moved the hidden springs of nature that a new issue arises on given circumstances. The ordinary issue is supplanted by a higher issue. The essential facts before us are a certain set of phenomena, and a Higher Will moving them. How moving them? is a question for human definition, the answer to which does not and cannot affect the divine meaning of the change. Yet when we reflect that this Higher Will is everywhere reason and wisdom, it seems a juster as well as a more comprehensive view to regard it as operating by subordination and evolution, rather than by interference or violation."

stood, all three are equally independent of physical cause and effect, are equally supernatural.

In comparing preceding periods with the present and with each other, most naturalists and palæontologists now appear to recognize a certain number of species as having survived from one epoch to the next, or even through more than one formation, especially from the Tertiary into the Posttertiary period, and from that to the present age. M. Agassiz is understood to believe in total extinctions and total new creations at each successive epoch, and even to recognize no existing species as ever cotemporary with extinct ones, except in the case of recent exterminations.

These peculiar views, if sustained, will effectually dispose of every form of derivative hypothesis.

Returning for a moment to DeCandolle's article, we are disposed to notice his criticism of Linnæus's "definition" of the term *species* (Phil. Bot. No. 157), "Species tot numeramus quot diversæ formæ in principio sunt creatæ," which he declares illogical, inapplicable, and the worst that has been propounded. "So, to determine if a form is specific, it is necessary to go back to its origin, which is impossible. A definition by a character which can never be verified is no definition at all."

Now, as Linnæus practically applied the idea of species with a sagacity which has never been surpassed and rarely equalled, and, indeed, may be said to have fixed its received meaning in natural history, it may well be inferred that in the phrase above cited he did not so much undertake to frame a logical *definition* as to set forth the *idea* which, in his opinion, lay at the foundation of species, on which basis A. L. Jussieu did construct a logical definition: "nunc rectius definitur perennis individuorum similium successio continuata generatione renascentium." The fundamental idea of species, we would still maintain, is that of a chain, of which genetically connected individuals are the links. That, in the practical recognition of species, the essential characteristic has to be *inferred*, is no great objection, the general fact that like engenders like being an induction from a vast number of instances, and the only assumption being that of the uniformity of nature. The idea of gravitation, that of the atomic constitution of matter, and the like, equally have to be verified inferentially. If we still hold to the idea of Linnæus, and of Agassiz, that existing species were created independently and essentially all at once at the beginning of the present era, we could not improve the propositions of Linnæus and of Jussieu. If, on the other hand, the time has come in which we may accept, with DeCandolle, their successive origination, at the commencement of the present era or before, and even by derivation from

other forms, then the "in principio" of Linnæus will refer to that time, whenever it was, and his proposition be as sound and wise as ever.

In his 'Géographie Botanique' (ii. 1068–1077) DeCandolle discusses this subject at length, and in the same interest. Remarking that of the two great facts of species, viz. *likeness among the individuals* and *genealogical connexion*, zoologists have generally preferred the latter*, while botanists have been divided in opinion, he pronounces for the former as the essential thing, in the following argumentative statement:—

"Quant à moi, j'ai été conduit, dans ma définition de l'espèce, à mettre décidément la ressemblance au-dessus des caractères de succession. Ce n'est pas seulement à cause des circonstances propres au règne végétal, dont je m'occupe exclusivement; ce n'est pas non plus afin de sortir ma définition des théories et de la rendre le plus possible utile aux naturalistes descripteurs et nomenclateurs, c'est aussi par un motif philosophique. En toute chose il faut aller au fond des questions, quand on le peut. Or, pourquoi la reproduction est-elle possible, habituelle, féconde indéfiniment, entre des êtres organisés que nous dirons de la même espèce? Parce qu'ils se ressemblent et uniquement à cause de cela. Lorsque deux espèces ne peuvent, ou, s'il s'agit d'animaux supérieurs, ne peuvent et ne veulent se croiser, c'est qu'elles sont très-différentes. Si l'on obtient des croisements, c'est que les individus sont analogues; si ces croisements donnent des produits féconds, c'est que les individus étaient plus analogues; si ces produits eux-mêmes sont féconds, c'est que la ressemblance était plus grande; s'ils sont féconds habituellement et indéfiniment, c'est que la ressemblance intérieure et extérieure était très-grande. Ainsi le degré de ressemblance est le fond; la reproduction en est seulement la manifestation et la mesure, et il est logique de placer la cause au-dessus de l'effet."

We are not at all convinced. We still hold that genealogical connexion, rather than mutual resemblance, is the fundamental thing—first on the ground of fact, and then from the philosophy of the case. Practically, no botanist can say what amount of dissimilarity is compatible with unity of species; in wild plants it is sometimes very great, in cultivated races often enormous. DeCandolle himself informs us that the different variations which the same oak-tree exhibits are significant indications of a disposition to set up separate varieties, which, becoming hereditary, may constitute a race; he evidently looks upon the extreme forms, say of *Quercus robur*, as having thus originated; and on this ground (inferred from transitional forms), and not from their

* Particularly citing Flourens: "La ressemblance n'est qu'une condition secondaire; la condition essentielle est la descendance: ce n'est pas la ressemblance, c'est la succession des individus, qui fait l'espèce."

mutual resemblance, as we suppose, he includes them in that species. This will be more apparent should the discovery of the transitions which he leads us to expect hereafter cause the four provisional species which attend *Q. robur* to be merged in that species. It may rightly be replied, that this conclusion would be arrived at from the likeness step by step in the series of forms; but the cause of the likeness here is obvious. And this brings in our "*motif philosophique.*"

Not to insist that the likeness is, after all, the variable, not the constant element,—to learn which is the essential thing (resemblance among the individuals, or their genetic connexion), we have only to ask which can be the cause of the other.

In hermaphrodite plants (the normal case), and even as the question is ingeniously put by DeCandolle in the above extract, the former surely cannot be the *cause* of the latter, though it may, in case of crossing, offer *occasion*. But, on the ground of the most fundamental of all things in the constitution of plants and animals, the fact, incapable of further analysis, that individuals reproduce their like, that characteristics are inheritable*, the likeness is a direct natural consequence of the genetic succession; and it is logical to place the cause above the effect.

We are equally disposed to combat a proposition of DeCandolle's about genera, elaborately argued in the 'Géographie Botanique,' and incidentally reaffirmed in his present article, viz. that genera are more natural than species, and are more correctly distinguished by people in general, as is shown by vernacular names. But we have no space left in which to present some evidence to the contrary.

Here we must abruptly close our long exposition of a paper which, from the scientific position, ability, and impartiality of its author, is likely at this time to produce a marked impression. We would also direct attention to an earlier article in the same important periodical (viz. in the Bibl. Univ. for May 1862), on the European Flora and the Configuration of Continents in the Tertiary Epoch, a most interesting abstract of, and commentary on, the introductory part of Heer's 'Flora Tertiaria Helvetiæ,' as re-edited and translated into French by Gaudin, with additions by the author.

* See Silliman's Journal, ser. 2. vol. xxix. (March 1860) p. 165, for the enunciation of this obvious principle.