

noticed here. The new character unexpectedly brought to light is that of the position in the acorn of the five atrophied ovules as respects the seed, or kernel, which results from the fertilization of the sixth ovule, the only one which ever matures. DeCandolle shows that the aborted ovules do not disappear as the fruit grows, but persist, just as they are well known to do in the Horse-chestnut and Buckeye, and that they may be found in the ripe acorn upon examination. It appears that the ovules in this genus are by no means always suspended from the summit of the cells, as generally thought, indeed, that they are ascending from the base or near it in the common European Oak, and persist there in the acorn; while, however, they are found above the seed in four of the five natural sections under which DeCandolle arranges the species. But in the great section *Lepidobalanus*, which comprises all the Oaks of the northern parts of the world, except one of California, these ovules are situated sometimes at the base, rarely about the middle, and sometimes near the summit of the seed. Moreover, all the Oaks which mature their fruit the first year bear their atrophied ovules at the base of the seed, or at least below its middle. Oaks of biennial maturation are divided in this respect, some having these ovules below, others above; but most of the North American species appear to be in the latter category.

This character of annual or biennial maturation, which DeCandolle thinks has been neglected for half a century [surely not in this country, where it has been familiar, both popularly and scientifically, ever since the time of the elder Michaux], was taken up by the acute and excellent Gay in the Old World, who showed that two Cork Oaks had been confounded under the name of *Quercus Suber*, one with annual, the other with biennial maturation of the fruit. This character, being of easy application to herbarium specimens of any goodness, as well as obvious in the living tree, would naturally be much relied on in classification. But, as in the case of the two Cork Oaks, so in general, it is not coördinated with other important differences, and therefore it serves merely to distinguish related species, or to subdivide conveniently that portion of the *Lepidobalanus* group in which the ovules are inferior.

DeCandolle notices a peculiarity in the embryo of our Live Oak (*Q. virens*), viz.: that its cotyledons are perfectly united into one homogeneous substance, while nothing of the sort appears in its near relative *Q. Ilex* of the Old World. He wishes the germination of the Live Oak to be examined in this respect, apparently to determine whether the mass consists of two united cotyledons or of a single cylindrical one. *A priori*, we could have little doubt; but we solicit fresh acorns of the coming season, or germinating ones the present year from some southern posts. A. G.

2. *Species, considered as to Variation, Geographical Distribution, and Succession.*—*Etude sur l'Espèce, à l'occasion d'une Revision de la Famille des Cupulifères, par M. ALPH. DECANDOLLE.*—This is the title of a second paper by Mr. DeCandolle growing out of his study of the Oaks. It was published in the November number of the *Bibliothèque Universelle*, and separately issued as a pamphlet. 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*,¹ 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 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 not have equally 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 mate-

¹ 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 organized 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.

rials increase, so do the difficulties. Forms, which appeared totally distinct, approach or blend through intermediate gradations; characters, stable in a limited number of instances 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: over 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, may 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. coccifera*, *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. Tlapahuensis* 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. Seemannii* Liebm., with *Q. salicifolia* Née. 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 Prodrômus, 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 practiced 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 connection, 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.