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LINNÆUS AS AN EVOLUTIONIST.¹

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Not more than two decades have passed since with most people who had interested themselves in such matters, and with quite all who had not, evolutionistic theory and Darwinism were synonymous; the supposition being that Charles Darwin had been the original inventor, as well as the strong promulgator, of the hypothesis of the descent of present-time species of living things from earlier types. That misunderstanding nowhere now prevails; and while a multitude of talkers and writers on all sorts of topics use freely the term evolution, Darwinism is less frequently mentioned; for it is coming to be realized somewhat generally that there were "Darwinians" not a few, not only before the Darwin of the nineteenth century, but even before that almost as remarkable grandsire Darwin of the eighteenth. There were evolutionists among the Greeks of five and twenty centuries ago, and even among the earliest luminaries of Christian philosophy and theology of a period only less remote; while after the revival of learning, and of an interest in nature study, evolutionistic ideas found expression not infrequently: and of late, historians of science are bringing all this to light.

The catalogue of more or less distinctly evolutionistic naturalists who lived before the end of the eighteenth century, and who gave some expression to their ideas on this topic, is not a short one; but the name of Linnæus has not, in so far as I can learn, been placed on that list hitherto, except very hypothetically.²

¹ Read before the Biological Society of Washington, November 11, 1905.

² In the environment of the idea of evolution Linnæus may be considered not as a positive but as one of the negative factors.—Osborne, *From the Greeks to Darwin*, p. 128.

For any possible expression of views as to the origins of groups of plants and the permanency or mutability of such groups, one would naturally look, not to his many volumes of taxonomic and descriptive writings, but to just such a work as the *Philosophia Botanica*. Yet there one looks in vain for any expression that is not positively and unmistakably contrary to the idea of evolution.

In respect to the origin of genera, that which he says—and with Aristotelian brevity and conciseness—is this: “Every genus is natural and was in the beginning of things created such.”³ And because of this—which might well enough be called the supernatural rather than the natural origin of genera—because of this origin, he argues that: “No one genus is ruthlessly to be divided and treated as if there were two; neither are any two or more to be put together as if constituting only one.”

In the light of such a pronouncement, one could not attribute to Linnæus any notion of the gradual evolution of such groups of species as constitute genera; and if a genus is to have such origin, so, by the necessities of logic, are species also made; and he says: “All species are certain diversities of form which the Infinite Being created so in the beginning; which forms according to immutable laws of generation, produce always their like.” From this he proceeds to establish more firmly, if possible, the immutability of species by defining generation as being the actual “continuation of the species;” and he concludes by calling attention to how, as by necessity, this origin of all species precludes the possibility of any new species ever arising. And thus, under the heading of species does our author seem to have builded even a more insurmountable wall against the possibility of one’s successfully claiming him for the camp of the evolutionists.

There remains one other category of plant forms, of lower rank than species, recognized by Linnæus; that of varieties. Unless I err, he claimed that he had been the first of systematists to recognize varieties and to teach the distinctions between variety and species. Will he so define variety as to leave an opening for the possible development of a species out of that which started forth at first as a mere variety? If we use our own reason, and credit Linnæus with not momentarily forgetting to use his, we may not look to see him contradict himself

³ Genus omne est naturale, in primordio tale creatum. Linn. Syst. Nat. and Philos. Bot.

quite so promptly. He has said, and that in the paragraph next preceding the definition of variety, that all species—not most of them but all of them—were constituted such by the Creator in the very beginning of the existence of plant life and form. He will not subvert this proposition; at least, not in the very next sentence. His notion of a variety is, that it is such alteration of a species as may have been induced by changed conditions of climates, soil, temperature, exposure to or shelter from high winds, or any such items of mere environment; and he does not fail to add that, on the restoration of the plant to its old environment, it reverts to the original type form. One sees at a glance that this is not our twentieth-century idea of a botanical variety; but it is the Linnæan idea, and with that alone we are here concerned. The man makes so small account of varieties, from the taxonomic point of view, that he concludes his discussion of the topic with an apology for giving them place and mention in his books of systematic botany. "Variation," he says, "is in such matters as the size of the plant, doubleness of flower, a crisped or curled foliage, a difference of color, odor, flavor, etc." But he adds: "Many varieties of plants are in favor with gardeners, and agriculturists, others with florists, while still others are in esteem with pharmacists." From these expressions it is plain that Linnæus did not consider these changeable and even transient forms worthy of any serious consideration by botanists proper, and admitted them to his books only as in condescension to the wants of those classes of tradespeople whom he mentions. It may here be added that in almost all more recent botany, varieties, such as Linnæus had in mind when he wrote the definition, find no place. One looks for the account of them, if anywhere, in the calendars and catalogues of gardeners, pomologists, nurserymen and florists.

I have long understood how very definitely and absolutely this fine book, the *Philosophia Botanica*, excludes every idea of a possibly evolutionary origin for any species of plant.

And yet, Linnæus was an evolutionist. Nor is this so passing strange, in a world where men in great numbers—even some of high standing and great ability—say one thing, and think the very opposite.

That he entertained doubts as to the truthfulness of the proposition that everything that ought to be called a species had been made as it is in the beginning, is a discovery that I made quite fortuitously.

In the study of some species of *Thalictrum* I had need to consult a certain page of the Species Plantarum. Reading his account of *T. flavum*, and next below it that of *T. lucidum*, his concluding note regarding the species last named quite startled me. His Latin sentence here, as in many another place, is highly figurative, quite after the style of many a classic rhetorician and poet; and I read it again, and very carefully, to see if the idea which the first reading conveyed to my mind was quite that which the author meant to convey. There could be no doubt. Putting it into plain English prose; making it read as one would now write the same thought, his note on *Thalictrum lucidum* is this: "The plant is possibly not so very distinct from *T. flavum*. It seems to me to be the product of its environment."⁴

As helping toward a full understanding of this pregnant remark it must be said that the species *flavum* inhabits the cool moist meadows of northern Europe, while *lucidum* belongs to southern France and to Spain. Each has then decidedly its own environment. Each was known to be equally established as a permanent and indigenous plant form. Linnæus's reason for naming *flavum* as the parent and *lucidum* as the offspring, was a reason no better than this. *T. flavum* was of his own northern country and he knew it well. *T. lucidum* was a southerner, and he was less familiar with it; probably had never seen it but in a northern garden. That was all. It was a thing far enough from being amenable to his definition of a variety. It seemed a species; yet he doubted that it was any more than a daughter species to *Thalictrum flavum*. The one had been created a species in the beginning, the other was probably not so old; more likely to have come into existence away down among the more arid hills of Spain; but it had come to stay. Rather many plant forms that had been reckoned good species before Linnæus and that are now again so considered everywhere today, were with Linnæus mere varieties of other species. But he declined so to treat *Thalictrum lucidum*. If the relation between this denizen of the fervid South and his plant of the frigid Scandinavian peninsula should be declared nothing more than the relation between a specific type and its variation, botanists would be asking how long before he would make an end of species altogether. He was not himself convinced that it was a mere variety,

⁴ Planta, an satis distincta a *T. flavo?* videtur temporis filia. Species Plantarum, 1 Ed., p. 547; 2 ed., p. 770.

and so he retains it as a probable species, yet to his half secret thinking not at first created such, but the descendant of another species.

Familiar as I had been for many years with the *Species Plantarum* as a book of reference, this one discovery upon which I had now stumbled, seemed so much like a new revelation of the mind of Linnæus that within a very few days I had read every one of the 1682 pages of the edition of the year 1764 in search of other kindred expressions regarding the possibility of the descent of some species from others.

Only three pages away from the record of his thought about the origin of the *Thalictrum*, under *Clematis maritima* occurs this remark: "Magnol, and also Ray have adjudged this to be a variety of *C. Flammula*. I should rather think it is derived from *C. recta* under altered conditions." Now while this remark, standing by itself, might indicate an opinion that the plant under discussion was a mere variety of *Clematis recta*, yet Linnæus did not so place it in this or any other of his books. He gives it the rank of a species, distinctly, and must needs have done so in view of his own definition of varieties as transient forms, developed mostly under cultivation. *Clematis maritima*, as its name indicates, is a seaside species, unchanged in its character from immemorial ages. He knew all this and held it to be not a variety but a derivative species; not one so created in the beginning.

Again, next to the familiar *Achillæa Ptarmica*, of almost all Europe, he places the name and description of *Achillæa alpina* known only from the mountains of Siberia. No botanical authority has ever seemed to think of this as possibly a mere variety of *A. Ptarmica* of Europe; no more does Linnæus; but while according it full specific rank, and as if forgetful of all he had said in the *Philosophia Botanica* upon such matters, he appends to his technical account of *A. alpina* this most evolutionistic suggestion: May not the Siberian mountain soil and climate have molded this out of *A. Ptarmica*?⁵

Among the more elegant flowering plants adorning the borders of subsaline marshes southward in the United States is one which Linnæus denominated *Hibiscus Virginicus*.⁶ It is exclusively North

⁵ An locus potuerat ex præcedenti formasse hanc? *Species Plantarum*, 2 Ed., p. 1266.

⁶ *Kosteletzkya Virginica* of more recent authors.

American, and even here of somewhat restricted range. A similar species, of distribution as limited and peculiar, belongs to southern Europe, inhabiting the shores of the Adriatic Sea. Now between these two kinds of *Kosteletzkya* occupying widely sundered continents, and neither one much more than local, each along its own little line of seaboard—between these two Linnæus apprehends the existence of a more intimate relationship than the most advanced evolutionists of the twentieth century would be likely to affirm. He remarks a very close superficial likeness between them; so close that, were that all, he would declare them to be specifically one and the same; but, in the characters of their little seed pods or capsules they are so unlike that on this account separate specific rank must be accorded both, and so he places them; concluding, however, with this thoroughly evolutionistic query: May not the Venetian species have sprung from the Virginian?⁷ The more probable theory of the evolutionist of our time would be, that both are descendants from some common ancestor that had a more general distribution and is now extinct. But, that Linnæus was disposed to regard the Virginian species as having been created such as it is, and the Venetian as having originated from that in after times, is enough to warrant our regarding him as an evolutionist.

I shall cite but one more instance of Linnæus's tacit acceptance of species as derived from other species through altered environment. The case is that of the cultivated beet. The genus *Beta*, in his view, consists of two species only, one the *Beta maritima* indigenous to Old World seashores, a wild plant altogether, and never under cultivation, and, in this wild condition not given to variation, but always one and the same thing. The second species is *Beta vulgaris*, one not known as a wild plant anywhere, but existing from immemorial ages in gardens and fields as a cultivated plant, and that under many marked varieties. Now the short and easy method of dealing with a genus like this—a method many an indifferent systematist would follow—would be to make the guess that, as only one wild species is known, all the cultivated things of that genus are but so many varieties of the one species. The whole tendency of Linnæus's mind was in this direction, that is, of reducing both genera and species to a minimum.

⁷ *Species Plantarum*, 2 Ed., p. 981.

But there was a difficulty here with these two members of the genus *Beta*, the simple and unvarying wild kind, and the extremely variable one of cultivation. The cultivated plant was hardy, often ran wild, as it were, by escape from cultivation; but these reverts never were found to be equivalent to *Beta maritima* or anywhere near it. The *Beta vulgaris* self-sown and run wild for years, and greatly altered from its cultivated condition, yet invariably retained a character of its own; so that no one would think of calling it *Beta maritima*; therefore, with Linnæus the collection of the varieties of cultivation must be admitted as forming a distinct species of which the native original was unknown, and probably long ages ago extinct. To this view of the case he was perhaps inclined; yet not so strongly as to preclude his offering, in a note, this very different suggestion: "Possibly born of *Beta maritima* in some foreign country."⁸ The force of this alternative proposition will be lost to any one who does not recall that, according to the Linnæan account of a variety, *Beta vulgaris* if it originated from seed of *Beta maritima* originated not as a variety but as a species; and such an origin as he thinks the cultivated beet may have had from the wild one would amount to nothing less than what is now called a mutation: one of those sudden leaps or transitions from one thing to another which we have been learning to take into account only lately.

A like instance confronted Linnæus under the genus *Cynara*, the type of which genus is the true artichoke, and has been cultivated from no one knows how far anterior to all written records. Under this old type species, *Cynara Scolymus*, Linnæus admits three marked varieties. Then he proceeds to name and define a second species, a very distinct one, but with a well authenticated history as having arisen and come into existence as a seedling of the other species. He intimates that he would have liked to be able to consider it a hybrid,⁹ but as its parentage as a hybrid could apparently lie nowhere but between two of the three varieties of the other species, the fact would remain that it was a species derived not from two parent species but from one alone. It was another of those abruptly derivative species in which Linnæus was disposed to believe despite those hard half-theologic definitions of his *Philosophia Botanica*.

⁸ *Species Plantarum*, 2 Ed., p. 322.

⁹ *Species Plantarum*, 2 Ed., p. 1159.

In the progress of these enquiries into the mind of Linnæus as to the origins of species nothing that I have come upon has more deeply interested me than his remark upon the two species of sundew common in northern Europe, *Drosera rotundifolia* and *D. longifolia*. They are very peculiar plants, uncommonly interesting from several points of view, and have in recent years profoundly engaged organographers and physiologists; but Linnæus was most interested in their ecology as bearing upon the problem of their genealogy. Both are bog plants, though far enough from being found in every northern bog. They seem to be particular about the kind of soil, the amount of moisture, the nature of the exposure, and also the plant associates amid which they will establish their habitation; and both species are at perfect agreement as to all special details of bog environment which they demand; for where one is found, there too is the other. They are much alike in size, mode of growth, degree of hairiness, form and color of flowers, etc., but the leaf blades in one are round, while in the other they are so much elongated as to be called narrowly oblong; and this one strong distinguishing mark is constant. There are no plants among them to show leaves intermediate between orbicular and oblong. They ought to be, and I think that by all botanists except Linnæus, both before his day and ever since, they have been held distinct; and even he did not positively affirm the contrary, but only expressed a doubt; and the sole reason he has for doubting the validity of *D. longifolia* is, that it and its mate species always occur under precisely the same conditions and together.¹⁰ It is such a reason as none but a confirmed evolutionist could give; the expression, perhaps unguarded, of a mind no longer very patient of the opinion that two species of the same genus can have the same native environment. A creative fiat could, of course, as readily make two species of a genus suited to certain conditions as one, and as easily twenty as two; and so no believer in the special creation of all species could have felt this doubt about the sundews to which Linnæus gave expression.

It has been thought that the mind of Linnæus as to the absolute fixity of species underwent a change between the years 1751 and 1762, though only in so far as to induce him to admit the origin of more

¹⁰Habitat ubique cum præcedente; an itaque satis diversa species? Species Plantarum, 1 Ed., p. 282; 2 Ed., p. 403.

recent species by hybridization.¹¹ My own impression is that few if any of the plants thought by Linnæus to be hybrids are at all of that origin, according to the views of modern botanists, but rather, for the most part at least perfectly distinct and genuine species. But what I have herein, I think, clearly shown is, not only that Linnæus accepted and admitted to his books, as species, forms he thought of as developed from other species, not by any crossing, but through mere environment—natural environment in some instances, artificial in others. And this bent of his mind was so strong that he could scarcely admit two members of a genus to be specifically distinct if found to occur always under the same physical conditions. Again: while it is generous to allow to the great nature student the eleven years between 1751 and 1762 in which to have changed his views a little as to the fixity of all species, the simple fact is that nowhere were the views set forth in the *Philosophia Botanica* of 1751 more squarely contradicted than in the *Species Plantarum* of 1753. There were two years intervening between the dates on the respective titles; but most likely he was engaged in writing the works, at least in part, simultaneously. But the great man was writing and publishing as other men of genius had done before him, under environment.

In a letter written by Linnæus as early as 1747, addressed to his most intimate and trusted friend, J. G. Gmelin, author of the *Flora Sibirica*, he gives confidential expression to the restraints under which he feels that he is obliged to write on matters that impinge upon the domain of theology; to his unwillingness to face the disapproval of the Lutheran and orthodox ecclesiastics who, in his day, ruled the destinies of all seats of learning in Sweden. He says to Gmelin:

You disapprove my having located Man among the Anthropomorphi. But man knows himself. Now we may, perhaps, give up those words. It matters little to me what name we use; but I demand of you, and of the whole world, that you show me a generic character—one that is according to generally accepted principles of classification—by which to distinguish between Man and Ape. I myself most assuredly know of none. I wish somebody would indicate one to me. But, if I had called man an ape, or vice versa, I should have fallen under the ban of all the ecclesiastics. It may be that as a naturalist I ought to have done so.¹²

¹¹ Osborn, *From the Greeks to Darwin*, p. 129.

¹² This, though written as we have said in 1747, was never published until 1861. The original Latin text of the letter occurs in "*Joannis Georgii Gme-*

The good orthodox Lutheran ecclesiastics that ruled the Swedish university in every department of it would be thoroughly content with the pronouncements of the *Philosophia Botanica*; and that was a book any scholar would read with pleasure and with profit; but nothing like that could be said of the *Species Plantarum*. Here, at least, in footnotes, or even in places more obscure, very briefly, veiled in figures of rhetoric, and even under the further protection of question marks, he could express his profounder convictions and feel secure. And he was secure, indeed.

lini, Reliquiæ quæ, supersunt commercii epistolici cum Carolo Linnæo Alberto Hallero Guilielmo Stellero et al., *Floram Gmelini Sibericam ejusque Iter sibericum potissimum concernentis, ex mandato et sumtibus Academiae scientiarum Cæsareæ Petropolitanae publicandas curavit Dr. Guil. Henr. Theodor Plieninger; Stuttgart, 1861,*" p. 55, and is as follows: "Non placet quod Hominem iter anthropomorpha collocaverim; sed homo noscit se ipsum. Removeamus vocabula, mihi perinde erit, quo nomine utamur; sed quæro a Te et Toto orbe differentiam genericam inter hominem et Simiam, quæ ex principiis Historiæ naturalis. Ego certissime nullam novi; utinam aliquis mihi unicum diceret. Si vocassem hominem simiam vel vice versa omnes in me conjecissem theologos. Debuissim forte ex lege artis."