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The Concept of the Genus: IV. The Delimitations of Genera from the Conservative Point of View

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## IV. The Delimitations of Genera from the Conservative Point of View

EARL EDWARD SHERFF

It may seem presumptuous to attempt to represent within twenty minutes, even in small part, the conservative school of thought in plant taxonomy. In fact the term conservative itself has been sadly abused and one doubts if its definition for botanists generally is not largely subjective and dependent mainly upon who does the defining. By one writer conservatives have been characterized as those seeking relationships and hesitant to describe new species, while radicals are characterized as being impressed by diversification and anxious to record their findings. But with many of us the distinction seems akin to that made long ago between orthodoxy and heterodoxy, namely, that orthodoxy is my "doxy" and heterodoxy is yours. Certain it is that some taxonomists have professed a conservative viewpoint for generic delimitations and then, by utter disregard of nomenclatural rules or taxonomic precedent, or both, proceeded to make wholesale changes or innovations of nomenclature in other respects, sometimes going farther than even a self-respecting radical or liberal would feel warranted in doing. Thus, for example, what amounted almost to an obsession with one of our late American workers in taxonomy was the designation and naming or renaming of subspecies, by which he meant commonly nothing more or less than the conventional varieties as they were understood by Linnaeus, Augustin DeCandolle, Willdenow, Gray, and a long line of other workers. If we are to accept the principle of a binomial nomenclature at all, it would seem self-evident that we should not only abide by the rules adopted by our international congresses but also, wherever an arbitrary choice is to be made, defer to the carefully reasoned practices and matured judgments of taxonomy's founders, whenever these practices and judgments do not conflict with present-day rules.

Both radicals and conservatives must use the binomial system of nomenclature. An essential feature of this system is, of course, that the binomial for any species derives its first part from the generic name. This feature has been lamented as a fundamental weakness of the binomial system, since a change in our conceptions of genera and species eventuates frequently in a change of the scientific names. L. H. Bailey even states that "we should have gained much in simplicity of literature, in clarity and in popular usage, if we had had a mononymy or other arrangement instead of a taxonomic dionymy." Even admitting the truth of this statement, should we not have lost immeasurably had nomenclature failed to asso-

ciate for us, as it attempts to do under the binomial system, related specific entities under one generic name? In any event, as long as the binomial system of nomenclature is officially used by all of us, truly conservative botanists will be reluctant to recast generic concepts or limits except upon the most convincing evidence.

Conservatives, generally speaking, attempt to delimit genera with approximately the scope employed by Tournefort and later by Linnaeus. To sneer at our inability to define categorically what is meant by the Linnaean concept of genera is beside the point. True, there has been inconsistency, but it likewise is true that an unbiased study of Linnaean genera usually imparts a *genus sense* which is not far off the middle road of taxonomic opinion. Apparently much of the mischief done heretofore in carelessly juggling generic limits must be blamed upon certain viewpoints and procedures which the conservatives must condemn if they are not to condone the mischief itself. May we mention a few of these very briefly.

First there is the loose talk heard in some quarters about cumbersome trinomials and quadrinomials. In case a cosmopolitan or at least polydemic species exhibits several varieties and forms or *formae*, we are in effect told to elevate each to specific rank and thus simplify our nomenclature. A logical outcome of such a course, however, is sure to be the warping of our *specific* concepts far past the limits understood for species by Ray or by Linnaeus. In short, we shall have a degradation of the original species concept in numerous cases but its retention undisturbed in the others. Some of our so-called radicals, having committed themselves to this way of doing, have awakened to find too many species in some of the genera. They have then made generic segregations to ease the fancied tension from within which they themselves had helped to create. If we are truly enamoured of conservatism and genuinely believe in a logical delimitation of genera, we must not neglect our *species* concepts.

Another matter that must engage our attention is the provincialism that has flavored all too much of the work on the manufacture of new genera. The entire earth must be taken as the source-book of our generic concepts. The writers of some of our manuals and local floras have overlooked this fact. Many times they have erected so-called new genera largely or solely upon the basis of the species within their own geographic range. A classic instance is that of *Astragalus*, where the author of a manual covering part of western North America decided to employ eighteen distinct genera instead of one. But, as Skottsberg, a distinguished representative of conservative opinion abroad, points out, *Astragalus* is not exclusively or even mainly a North American genus. "Is it likely," he asks, "that the eighteen United States genera will be left untouched and natural after

the 1,000 non-American species have been taken into due consideration?" Skottsberg sets forth additional examples, one of them that of *Vaccinium* as treated in American manuals. For the American species, the keys to the sections *Vaccinium* proper, *Cyanococcus*, and *Vitis-Idaea* are sufficiently diagnostic. When, however, a half-dozen mutually close Hawaiian Island species are compared with the same keys, they are found to run to different sections, or indeed to possess overlapping characters. A believer in small genera might here be inclined to put *Cyanococcus* and *Vitis-Idaea* as separate genera and erect one or two additional but tiny genera to take care of the Hawaiian misfits. A conservative course would doubtless be, on the other hand, the continued maintenance of the genus *Vaccinium* in its broader sense, coupled with a redefinition of the component sections. A point to be emphasized, however, is that the author of a local flora or a manual for a restricted range is many times unfamiliar with a considerable percentage of species in the genera treated. In most cases the presumption of evidence will be against him. The least he can do and at the same time show respect for other workers who must use his book is to refrain from altering the status of any genus unless he has a comprehensive monographic knowledge of it for whatever parts of the earth it may inhabit.

A third matter, one closely related to the second, is the need for greater emphasis upon monographic research. It may be true, as some able workers assert, that various large genera like *Opuntia* and *Euphorbia* need breaking down into smaller units if we are to have a genus concept such as Linnaeus would have formulated could he have had all the information that we possess today. But only extended and painstaking monographic research will be of much value in helping us to make the appraisals or evaluations needed for settling these cases. May I inject here my own personal conviction, intensified during several years' monographic research upon the genus *Euphorbia* as it occurs in the Hawaiian Islands? I recognize of course that a large genus may embrace species more diverse morphologically than species of many admittedly distinct Linnaean genera. Such a genus is apt, however, to display within itself such a profusion of intergrading and overlapping characters as to make clean-cut generic segregations, at least within our present geological era or epoch, quite impossible. Conservatives are stigmatized as inconsistent if they move slowly in accepting some of the proposed segregations. But of what use is it, we may well ask, to reach for imagined increase in consistency if in so doing we throw the species into such confusion that no honest student can successfully fit to them our binomial system of nomenclature.

The conservative's preference in a general way for stability in nomenclature is sometimes criticized as making for stagnation of taxonomic

progress. One botanist has rightly termed the hope for complete stability a "will-o'-the-wisp calling us to the swamp of unattainment." But surely no conservative hopes for or expects complete stability. New forms continue to be discovered and, with their study, limits of certain genera may have to be changed. Old genera that have received their present taxonomic identity largely by piecemeal accretions from the pens of numerous authors must needs be restudied monographically. Much has been written about polyphyletic, or the origin of a genus or other group at different places or times by convergence of two or more lines of descent. Little has been written about the *pseudo-polyphyletic* that has arisen sometimes in literature when two or more authors with diverse points of view have referred generically different forms to the same genus. Conservatives, however, should be and doubtless will be as prompt as any others to welcome a re-examination of the morphological and phylogenetic bases on which each such genus rests. They will insist none the less that major nomenclatural changes be made only after extended and detailed research and not as the result of personal whim, or caprice of fancy, or mere love for something new. Probably our present era exceeds all past eras in the tendency to mistake mere change for genuine advancement. The plain duty of taxonomists, whether of the conservative or radical persuasion it matters little, is to shun all change made merely for the sake of change. They must seek an atmosphere of the utmost objectivity for their researches. It would be false to say that our concepts, generic or otherwise, are never in part subjective, but the degree of subjectivity should decrease as the comprehensiveness and thoroughness of our work increase.

A word should next be said against the arbitrary separation of genera, as is still often done, solely upon the presence or absence of one or more supposedly diagnostic characters. Under the theory of special creation this may have seemed justified. But we cannot hope to reconcile our presently held evolutionary theory of phylogeny at all points in the plant kingdom with such a practice. The genus *Cosmos* may be taken as an illustration. If we insist upon the presence of a rostrate achene, as was once done, several undisputed species of *Cosmos* automatically fall out of the genus, among them *Cosmos calvus*. If we insist upon wingless achenes, then *Cosmos Blakei* is excluded. If we demand slender roots, the entire section *Discopoda*, characterized in part by having fascicled, tuberous roots, must be dropped. Yet *Cosmos*, whether we assume for it a monophyletic or a polyphyletic origin, is so natural a genus that it was not even divided taxonomically into sections until 1932. The presence or absence of retrorsely barbed achenial aristae in the separation of the genus *Bidens* from the genus *Coreopsis* offers another illustration. Linnaeus, Augustin

De Candolle, and a host of other workers separated the two genera primarily by this one character. When Asa Gray found a herbarium specimen of the so-called *Coreopsis aristosa* possessing retrorsely barbed aristae instead of the antrorsely barbed ones customary in that species, he designated it "*Coreopsis aristosa* transformed into a *Bidens*." Later he treated this and similar forms in his *Synoptical Flora of North America* as hybrids between *Coreopsis* and *Bidens*. But with the advance of knowledge that came during the decade following the appearance of Gray's *Synoptical Flora*, it became evident that these forms were not hybrids. On the contrary, they were recognized as definite varieties. We then had the anomalous situation in which *Coreopsis aristosa*, *Coreopsis involucrata*, and *Coreopsis trichosperma*—to use the names then accepted for these species—were assigned to *Coreopsis*, while their varieties with retrorsely barbed aristae were to be referred to another genus, *Bidens*, if the traditional basis of distinction were to be observed. N. L. Britton promptly sensed the utter inconsistency and indefensibility of insisting further upon the following of tradition—and here we digress to remark that Britton would rank with most of us as a liberal or radical, not a conservative. Yet the course that he adopted in this and many other instances, when contrasted with that previously followed by some who were professedly conservative, should remind us that not all taxonomic progress has been accomplished or even initiated by the conservatives. With this thought in mind, may I confess to almost an outright fear of doing violence to the interests of plant taxonomy by appearing to divide its devotees for even twenty minutes into two distinct schools of thought? In actual experience there are more than two schools and each school has several grades. Moreover, the enrollment is frequently shifting and sometimes even switching schools. But to return to the case in point. Britton at once referred the so-called *Coreopsis* species exhibiting ambiversalism in their arisal barbs to the genus *Bidens*. In so doing, he was guided not by a single arbitrary or artificial character but by the sum total of characters manifested in each group studied. Such a course, it would seem, conservatives must ever stand ready to adopt if our taxonomy is to take even the slightest cognizance of evolutionary phylogeny.

This brings us to the often heard criticism that considerations of phylogeny will forever upset nomenclature. In the multitudinous cases like those mentioned, however, it will usually be only one or a few of the borderline species that will require shifting and consequently a change in name. The genera themselves will stand largely intact. But even in cases where the supposed phylogenetic record would appear to dictate radical rebuilding of generic concepts or widespread shifting of generic limits, it should be remembered that phylogenetic preachments vary highly

with the one uttering them. They may reflect a complex of emotional, nutritional, economic, and, someone has been cruel enough to add, pathological factors, a complex that has been known more than once to express itself in distinctly different phylogenetic explanations by the same individual within successive periods of time. Here, may I say, all true conservatives should welcome carefully thought out contributions from the standpoint of phylogeny, but we can have little patience with ever-recurrent, petty tampering in generic limits. Especially must this be so if we are led to suspect that a fortnight's sojourn at the seashore or a different brand of breakfast food would have crystallized into a different phylogenetic scheme of relationships. A recent writer has pleaded for a freer use of sub-generic sections to avoid the needless multiplication of genera and consequent alteration of numerous binomials. And indeed it would seem that there is much to commend such a plea, especially for the many cases where equally competent and equally well-informed authorities disagree.

Passing over several additional considerations which are germane to the subject of generic delimitations but which must be omitted here for lack of time, I shall conclude by discussing for a moment the urge made upon us, that we turn to experimental taxonomy, especially in its ecological and genetical aspects. As was pointed out by De Wildeman some years ago and also by Wiegand, the data offered by experimental taxonomy are not usually of practical value to the general taxonomist, even though they are very desirable and often capable of throwing great light upon the significance of morphological characters. To quote Wiegand verbatim, "such data are often impossible to obtain, sometimes because of the unavailability of the living material, sometimes because, as in the case of woody species, the time required to grow the plants is too great, but often also because of the large number of plants concerned." Personally, I would be the last to discourage monographers anywhere from supplementary cultural studies. But if it be admitted that generic characters as a rule are especially well ingrained into the evolutionary fiber of plant species, it would seem that limits of genera, as apart from limits of smaller units, will not soon need alteration because of experimentally adduced evidence. It appears not unlikely that far into the future, as already in the past, we must perforce heed the counsels of morphology and oftentimes of geographic distribution in the delimitation of genera for all unless some of the very lowest plants.

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