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## V. Our Changing Generic Concepts

W. H. CAMP

So far during this symposium there has been presented a most interesting discussion concerning the concept of the genus. Professor Bartlett, in his inimitable manner, has traced the early history of the concept of this unit of nomenclatural biology. Dr. Anderson, by means of his questionnaire, has collated and evaluated the thoughts and ideas of various of our modern taxonomic workers on the status of the genus. To this much-discussed problem, and based on patient study and much thought, Dr. Greenman has added his personal concepts. And Professor Sherff, using stability as the pillar around which he built his most excellent discussion, has presented a few of the many and valid arguments for the perpetuation of this stability.

It is therefore fitting to remember that the thing which we as taxonomists have been praying for—and even legislating for—is nomenclatural stability. It is the bright star toward which we have been steering; the goal we have been striving for; the haven of dreams come true—where there shall be no more changing of names.

But before I proceed with this discussion it might be well to make a public confession. Surrounded every day by herbarium cases in which repose specimens labeled with more than 150,000 different names, I am opposed to any changes which will necessitate the learning of new ones for the pitifully few of those I do know. At heart, therefore, I am a taxonomic conservative, a worshipper at the altar of nomenclatural stability. But even so, I trust you will permit me my brief moment of intellectual agnosticism while I depart from the broad path of fundamentalism; while I chance the difficult way of the transgressor along the stony road of the one whose assigned task on this program is to discuss, with sympathy, a most unwelcome topic—the splitting of genera.

What the name of an organism might be would make no difference, if it were a name and nothing more; but, under the present system in taxonomy, there is an implied consanguinity, an expression of relationship between species in so far as the generic name is concerned. We find today, therefore, that the genus is less a taxonomic catch-all and increasingly a unit expressive of close phyletic relationship.

Thus, among professional taxonomists, two schools of nomenclatural thinking in regard to generic delimitation have arisen and are now pursuing their own ways. At times during the development of the science these concepts were intertwined, often they ran parallel and, today, some

workers in the one group feel that the concepts of the other are so divergent from a fundamental convenience that they plead for legislative fiat to control their activity. An activity which those who pride themselves on being called conservatives consider as chaos, but which by the others is thought of as scientific progress.

Now, for a moment, let us consider certain of the backgrounds of one of these schools of thought in regard to biological nomenclature. At all times in the present discussion, we must remember that most of the genera around which the present controversy centers were described, and therefore delimited, during that period when biologists held as a basic principle the doctrine of Special Creation and its necessary corollary, the immutability of life forms. The philosophy of those who hold to a traditionally rigid concept of specific and generic delimitation was therefore founded on the basic assumption of a Special Creation. They will deny it, but the evidence is so obvious that they are in error if they try to rationalize their concepts in any other way. We may only hope that they really believe differently.

However, even in the early days, the facts, those insidious things which are continually raising their sinister heads, began staring the fathers of our science in the face and they soon began to be troubled with the knowledge that a nomenclatural unit was a concept and not a fact; that there were no hard and fast lines between the separate entities of each taxonomic category; that not only species, but genera and even families might intergrade. But these things came late in the making of the science and long after the definitions of the classic genera were laid down. I cannot emphasize it too strongly: those who are most intent upon the retention of the nomenclatural *status quo* are, today, confronted with the task of trying to rationalize a static system of immutability with the known facts.

Perhaps the central idea back of this should be expanded, not that all of us do not understand the situation, but merely to put it in a more concrete form. Briefly, our present system of nomenclature, in a general way, is organized on a basis of similarities, having as its fundamental principle a doctrine based upon the thesis that a community of similar morphological structures indicates relationship. With this criterion established, the beginning student of taxonomy soon learns that the species of a single genus have more characters in common than do the sister genera which constitute a family. But as his studies progress, it is not long until he discovers that there is no equality in the standard of delimitation; that in one group of plants, those characters which scarcely constitute specific differences, in another may be sufficient to separate the genera.

For example: in an attempt to rationalize the Vacciniaceae (if I may be permitted to speak of them as a family and not part of the Ericaceae)

I am confronted with the situation of finding, in the classic treatments, that such things as the articulation of the pedicel, which serve in part to separate the genera of the Thibaudieae, are in the Euvaccineae not considered as having sufficient weight to be included as characters separating even the species.

Let us now, for a while, consider another phase of this problem: certain of the goals of taxonomic research. As I have intimated, there are, at the present time, two rather definite schools of taxonomic revisionists. One of these includes those who, in their revisions, follow the already established generic lines, their work consisting in part of weeding out the synonymy that has, perhaps unavoidably, slipped into the literature of the group and also listing or describing the new material found since its last monographic treatment. The other school is not so much interested in the mere cataloging of known species as in a study of the origin, evolution and dispersal of a group of plants. It is this group to which the epithet of "splitter" is most often applied. It can only be regretted that some of the worst offenders in this matter were not so much monographers, but students of regional floras and, although much of their work undoubtedly will be permanent, it serves temporarily, at least, only to becloud the main issues. On the other hand, the honest monographer studying the group on which he is engaged from the standpoint of its total distribution, sees it as a group of plants which are the result of divergent lines that have proceeded out of the world's dim past into the present and knows that the plants in his hands, in themselves, do not constitute an orthogenetic series but are only the ends of a much-branched and often tangled system of descents. The monographer with such a viewpoint is likely to have a vastly different concept as to what constitutes a genus from the one who is merely cataloging the valid species of a group. I have not said that one method is better than the other, nor do I more than intimate that one is to be desired rather than the other. *They are intellectual activities of different sorts and, as a result, their end-products will be different.*

With this in mind and of myself knowing nothing about the mosses, I recently wrote to one of our well-known bryologists<sup>1</sup> for his opinion on what has been happening to the classic genera of bryophytes. I shall quote from his reply:

"There has been a tremendous change in the concept of the genus in mosses and hepatics in the last century. In the time of Linnaeus there were very few genera. Nearly all the acrocarpous mosses were members of *Bryum*, although the atypical and characteristic *Buxbaumia* and *Polytrichum* were, of course, recognized. Almost all pleurocarpous mosses were put into *Hypnum*, although, again, the absolutely unmistakable *Fontinalis* and *Neckera* were even then

<sup>1</sup> Dr. William Campbell Steere.

segregated. In the middle of the nineteenth century, however, a great splitting of the Linnaean and Hedwigian genera was effected in the epochal work of Bruch, Schimper and Gumbel (*Bryologia Europaea*) which was published between 1836 and 1855. The most important splits made here were the recognition in the old genus *Hypnum* of natural groups as new genera, such as *Brachythecium*, *Amblystegium*, *Plagiothecium*, *Thuidium*, *Pseudoleskea*, *Heterocladium*, and a dozen others.

"When the Musci of the whole world were evaluated as a group, rather than as an extension of the local flora of Europe and the United States, it was realized that mosses placed closely together in the same family, or even as congeners in the earlier systems, were really far separated. Through the work of Müller, and later Fleischer and Brotherus, whole new families and genera were erected for well known species. Whereas Linnaeus and, fifty years later, Hedwig, recognized a dozen or two genera, the list of valid genera in the last edition of Engler and Prantl (vol. 11: 1925) takes several pages. The tendency now is not so much to erect new genera, but toward a general recognition of splitting done since the turn of the century by Fleischer, Brotherus, Cardot, *et al.* However, I recall a paper by Dixon since 1930 in which he described ten new genera!

"It is therefore obvious that the breakdown in the Musci, insofar as the generic concept is concerned, is general. Now for examples. Perhaps the best are those from well known sources, and so I shall make a few comparisons between the old familiar Grout's *Mosses with Hand-lens and Microscope* (1903), and the newest and best, yet conservative work of Grout (as editor) *Moss Flora of North America, north of Mexico*. *Dicranum fulvellum* and *D. Starkei* are separated out into the genus *Arctoa*. *Dicranum longifolium* is now the type of *Paraleucobryum*. Although Grout does not segregate *Dicranum flagellare* and *D. montanum*, many American bryologists call them species of *Orthodicranum*. Still other segregates are recognized by Engler and Prantl. *Funaria* has been split, yielding the genus *Entosthodon*. The old genus *Amblystegium* has been broken up into *Amblystegium* (emend.), *Hygroamblystegium*, *Leptodietyum*, and *Sciaromium*. *Calliergon* yields *Calliergidium* and *Calliergoniella*. *Hylocomium* yields *Rhytidium*, *Rhytidiadelphus*, and *Rhytidiopsis*. Several other genera are split out by Fleischer, but not yet accepted by Grout. Even the much pared genus *Hypnum* still yields new genera, for example: *Brotherella*, *Heterophyllum*, *Ptilium*, and *Ctenidium*. *Plagiothecium* is subdivided into *Plagiothecium* (sensu stricto), *Taxiphyllum*, and *Isopterygium*. Grout considers these as subgenera, whereas Fleischer considers them as genera in different families! This very nicely illustrates the local viewpoint versus the cosmopolitan.

"I predict that I shall see all present subgenera become genera within my life-time. Splitting will continue almost anywhere in the mosses, perhaps most logically in the pleurocarpous groups. Hepaticae are in even more of a flux,

taxonomically. I am not unfavorably inclined or disposed toward these changes, for it is my conviction that most of the living forms are the tips of widely separated branches of the phylogenetic tree and are grouped together anyhow only because of man's passion for classification."

What I have just quoted from the above communication concerning the bryophytes is equally true of other forms. Let us, therefore, turn our attention to the flowering plants and for a little while consider the genus *Gaylussacia*, the huckleberries, with which many of us are familiar. The erection of the genera *Buxella*, *Decachaena*, and *Lasiococcus* to take care of our North American species of huckleberries has met with a great deal of opposition and I, too, have deplored the segregation.<sup>2</sup> But, fundamentally, it was sound, for the old classic genus is composed of four very definite groups of species: (1) The Buxifoliae (*Gaylussacia* H. B. K.), found mainly in the mountains of western South America, is composed of numerous species; (2) the Baccatae (*Decachaena* T. & G.), with its four species, is confined to eastern North America; (3) the Dumosae (*Lasiococcus* Small) ranges on the Coastal Plain from Newfoundland to Florida and Louisiana with two species, and occurs again with several additional in Brazil (a perfectly natural distribution); and (4) *Gaylussacia brachycera* (*Buxella* Small; this nomenclaturally a homonym) with an interesting distribution in small isolated areas from Tennessee to Pennsylvania and its morphological peculiarities, is plainly a relic out of the Early Tertiary and not closely related to the other huckleberries.

Had we been able to maintain the species with which we are most familiar in the genus *Gaylussacia* and erected new genera for those in South America, there would have been little protest. Apparently it is a common reaction among taxonomists—being human—that, so long as a genus is endemic in some remote part of the world it may be split as the student pleases, the splitting being hailed as a brilliant piece of research. But let one among us attempt, phyletically, to rearrange a genus with species in our own local areas—the rearrangement resulting in the necessity of learning new generic names—there is an immediate and loud protest. Even so, *Lasiococcus dumosus*, *Decachaena baccata* and *Buxella brachycera* are names with a strange and unfamiliar sound and I don't like them any more than you do. But, I have been asked, "Then why change them? We have known them as species of *Gaylussacia* for so many years." There is only one answer. If such an argument is to determine our criteria concerning the status of a generic name, then let us be purists and return these species to the genus *Vaccinium*, for they were known as *Vaccinium dumosum*, *Vaccinium resinosum*, and *Vaccinium brachycerum* for about a

<sup>2</sup> Bull. Torrey Club 62: 129-132. 1935.

half-century prior to their inclusion in the genus *Gaylussacia*. The point is, none among us remember the clamor that arose when the botanists of another day had to learn to think of them as belonging to "that new-fangled genus *Gaylussacia*." From the standpoint of phylogeny, there is no more reasonableness in retaining these species in *Gaylussacia* than in returning them to *Vaccinium*.

Let us now turn our attention, briefly, to the Compositae. There immediately comes to mind the present controversy concerning the status of *Euthamia*. Is it to be a genus or merely a section of *Solidago*? If, however, we are truthful with ourselves, we must admit that the characters which we use to separate *Euthamia* from *Solidago* are of no less magnitude than those by which the basic species of *Solidago* and *Aster* are differentiated. Or conversely, if we return *Euthamia* to *Solidago* then, to be consistent, *Solidago* and *Aster* should be united. Or, for another example, the genus *Senecio*. Here is an open field for the taxonomist who wishes merely, either to describe a considerable number of new species or, as is not unknown to some of us, the pleasant experience of throwing a vast number of names into synonymy. Actually, however, the real opportunity for study in this genus lies in unraveling the various migration routes used, and the evolutionary mechanisms resorted to, by this cosmopolitan, highly divergent and exceedingly complex group of plants.

I am anticipating the question which the so-called conservatives will ask at this point. "Is it necessary that we have a whole host of new genera foisted upon us; will not the sub-genus satisfy your desire to express phyletic segregation?" The answer, flatly, is "No." Do these same "conservatives" advocate returning *Marchantia* to the Algae, all the species of lichens to *Lichen*, many of the mosses to *Bryum* and *Hypnum*, and a host of orchids to *Orchis*? So far, our science has been progressing steadily toward a rationalization between taxonomic categories and phyletic units, and I see no valid reason why we should make our nomenclatural system so rigid and unyielding that it would no longer serve to express what it traditionally has: *the rank and degree of relationship between organisms*.

This concern over "stability of names" has always been a point of discussion among botanists, and if taxonomic priority in the Spermatophytes goes back to the "Species Plantarum" so does the present controversy, for in 1753 Peter Collinson (probably the father of "modern" nomenclatural conservatism!) wrote to Linnaeus as follows:<sup>3</sup>

"I have had the pleasure of reading your 'Species Plantarum,' a very laborious and useful work, but my dear friend, we that admire you are much concerned that you should perplex the delightful science of botany with

<sup>3</sup> Clute, Willard N. The Common Names of Plants, p. 13. 1931.

changing names that have been quite well received and adding new names quite unknown to us. Thus, botany which was a pleasant study and attainable by most men, is now become by alterations and new names, the study of a man's lifetime, and none now but real professors can pretend to attain it. As I love you, I tell you our sentiments. . . . If you will forever be making new names and altering good and old ones for such hard names that contain no idea of the plant, it will be impossible to attain a perfect knowledge of botany."

Being thus fortified with the knowledge that today's controversies are not a new thing and buoyed by the hope that the science of taxonomy has not become stagnant, I trust that we may look upon our present minor tempests with the same patient humor with which we view those of the past. Thus, looking into the not-too-distant future, we may envision the day when our standard texts will list not more than a half dozen species of *Vaccinium* in the Americas for, after all, the high-bush blueberries of our eastern states are much more closely related to the secure and well-established *Thibaudia* of South America than to *Vaccinium Myrtilus*, the type of its genus, the one which Linnaeus described first because he knew it best.

Perhaps I speak with unreasoning rashness, but in a science where every thinking morphologist and vascular anatomist knows that the "Pteridophyta" are not a phylum; that the "family" Polypodiaceae is not monophyletic but, in the main, a miscellany of the end-products of the evolution of other basic fern families; and where nearly every taxonomist admits that the Compositae are polyphyletic and not a natural family—and yet does nothing about these things—it is small wonder that the honest phyletic revisionist, too often confused with the unreasoning splitter of genera, is looked upon as a botanical outcast and pariah.

I tell you, and I am serious, we as taxonomists must face the issue. Either we must take our place with those who are attacking the fundamental problems of biology, or we will degenerate into mere namers of specimens. We must either confine ourselves to the grinding out of a few lines of miserably inadequate Latin with *sp. nov.* and our names hooked onto it, or be biologists. The bifurcation is clear. And if we are to take our place in the body biologic, it can only be as phylogenists—students of evolution in the broad sense—with the naming of plants a mere incidental. In doing so, we will find it necessary from time to time to reconsider our premises for, with additional information, it will be necessary to reorganize our concepts and lay our course into new channels of thought.

At present, our nomenclatural system indicates phyletic relationship. If we continue this system—and I see no need to change it—the results of our work must then be reflected in the names we use. Actually, owing to



fortuitous segregation of the past, *the number of changes necessary would be much less than one might suppose.*

If I am correctly informed, the first organized part of botany was taxonomy. Morphology and comparative anatomy have long ago forgotten the manner in which they were born, and cytology has bred a line who look upon their sire with pity and a little contempt. It is perhaps advisable that they again be brought in as integral parts of the family circle. Yet, in honesty and fairness, their defection was no fault of their own. It was ours. When they were born we tolerated their blattings as we do those of infants. In adolescence we were blind to their needs and gave them little guidance and less help, refusing to see that as adults they might have something of their own and something to contribute to our needs. It is therefore little wonder that morphology, comparative anatomy and cytology, pursuing their own ways with but little concern and less guidance from their parent, should be little troubled with the trials and tribulations which now confront their sire.

Casting aside simile, I say: it is high time that we as taxonomists make better use of the findings, and particularly bring into play the techniques of the modern morphologist, the comparative anatomist and the cytologist. Frankly, those of us who blat loudest "Back to the fundamental truth—back to Linnaeus," are those who have made little or no use of the wealth of material already made available to us by the students in these other fields.

The space is limited and I cannot, here, present my case with specific examples where such studies have been made and the conclusions derived from them but, in general, if we were to apply the techniques available and reconsider the problems confronting us from the standpoint of phylaxis, some of our existing genera would be combined and still others be broken down into their proper units. This, obviously, necessitates the change of some few names. But what of it? Should we, in deference to the non-taxonomists—a vociferous group who think of their branches of the science as being progressive—hesitate to modernize our science, even at the expense of a few changes of name? Do we as biologists hold that Aristotle taught only truth? In spite of their fad for "standardized plant names," do the horticulturists still use the nomenclature of Pliny? Do the physiologists feel the necessity of discussing their phenomena in the phraseology of Stephen Hales or Lavoisier? Do the ecologists use only the concepts of Warming? Do the anatomists describe their structures in the terminology of Marcello Malpigi or Nehemiah Grew? Do the cytologists consider Robert Brown the sole authority on nuclear phenomena? Are we, the taxonomists, then, to be stuck forever with concepts of the limits of genera as defined by Linnaeus, by Bentham, or even Asa Gray? If we are

honest with ourselves, we will admit that we have not felt any such necessity in the past. Nor do I see any present need of maintaining a stultitiously archaic *status quo* if, in holding to it, we impede the splendid progress already begun in a better understanding of fundamental plant relationships.

Perhaps we should adopt as our motto, not "Back to Linnaeus" but, "Forward to the truth." Perhaps, if we were not afraid of the puling croaking of certain of our confreres every time we broaden and particularize our concepts, we could put new life into old taxonomic bones, long interred in the musty vault of nomenclatural conservatism.

From an increasing number of laboratories there come rumblings of a rejuvenant taxonomy and I warn you, the workers in these institutions are not merely worms working in the corpse. When their further results come, as they recently have, there are those among us who may not like them, for a few plant families will be ripped apart and genera will be recast. Perhaps, with a regenerate and growing science—contributing more to botany than several additional lines to the latest supplement of the *Index Kewensis* when we revise a group; when we become a real part of biology—with emphasis on the true meaning of *βίος*—we then can move out of the top floors, the dusty attics and dim holes where they have pushed us and down where we belong—down on the first floor with the rest of those who, too, consider themselves botanists.

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