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THE GENERIC CONCEPT IN THE CLASSIFICATION OF THE FLOWERING PLANTS.¹

THAT many of the most useful scientific terms defy accurate definition is a fact which can not be denied. Indeed, the recent progress of science, at least in biology, has been away from, rather than towards, any dogmatic form of statement. Many terms, which fifty years ago were smugly defined in the text-books and manuals of the time, are now, when viewed in the light of the developmental theory and from the diverse points of view of modern investigation, either well-nigh obsolete or have, through a gradual accretion of varying meanings, come to express only the vaguest generalities. This change is by no means a matter to be deplored. It is, in fact, an evidence of advancing thought. Loose and general terms are giving place to more technical and specific ones at about the same rate that the older and vaguer concepts are being supplanted by the more refined distinctions of modern science.

In some cases, however, the wide usefulness and general familiarity of terms have made them, notwithstanding some vagueness, far too valuable to discard and difficult to replace; and in these cases it is a matter of great importance that scientists should from time to time examine such terms theoretically in order that they may be applied with reasonable uniformity. An excellent example in point is the word

¹Address of the vice-president and chairman of Section G—Botany—at the New Orleans meeting of the American Association for the Advancement of Science.

genus, so well known yet so variously used. It is in no wise my purpose in the course of these remarks to attempt to redefine the generic concept, but merely to consider the present divergence in the limitation of genera and see if there is any practical basis for a greater harmony in this matter.

Plants are classified according to their degrees of likeness or dissimilarity to each other. In likeness we see evidence that certain plants are related, and by related we mean that they are sprung from a comparatively recent common ancestry. Likeness, it is true, no matter how close it may be, is not an absolute proof of near relationship; but, since in most cases it is impossible to trace conclusively the ancestry of existing species of plants, their degree of similarity furnishes almost the only evidence available in regard to their relationship. I need scarcely say that by likeness is here meant not mere habital resemblance, but the sum of all the morphological, anatomical and physiological similarities between the plants compared. Bearing these matters in mind, we may roughly describe a genus (when pluritypic) as a group of species which from likeness appear to be more nearly related to each other than they are to other species. But so varying are the degrees of similarity and so diverse is human judgment regarding them, that such a definition offers only an exceedingly vague basis for a uniform classification.

Indeed, the practise of different botanists in the interpretation of genera has been so multifarious that many persons have become sceptical as to the real existence of such groups in nature and are accordingly inclined to treat the whole subject of generic classification as a mere matter of utility, a sort of division of the plant world into sections of convenient size by confessedly artificial lines analogous to the parallels of latitude and meridians of longi-

tude. But much may be said for the objective reality of genera, at least in certain families. Who will wish to contend, for instance, that such tolerably uniform groups as *Lupinus*, *Aquilegia*, *Delphinium* or *Carex* are not far more distinct natural categories than are many of the relatively vague and ill-defined species of which they are composed. There can be no doubt whatever that in many families clearly definable genera exist and have been duly recognized. In other families or parts of families, however, the living species do not fall into sharply limitable groups, but are by character either somewhat isolated or more often exhibit highly complex cross affinities rendering any simple or convincing classification into genera impossible. It is in such groups that uniformity of classification is most difficult to obtain, for it is in them that individual judgment has the widest play.

There are two very different methods of treating genera. One is to lay much stress on the idea of a generic type, that is to say, a species of the genus in question, which is supposed to fix the generic character and pass as a sort of sample or gauge, by comparison with which other nearly related species are to be judged. If they appear to agree with this type-species in the essential characters, namely in those points which in the particular family concerned have come through accumulated experience to be considered of generic value, the species are considered congeneric with the type and receive the same generic name. The other mode of treating a genus is to endeavor not so much to group the species about some historic type as to indicate the precise circumscription of the genus by pointing out as clearly as possible just how its species as a whole differ from those of related genera. A species has sometimes been not unaptly referred to as an island in a sea of death. Carrying out this simile

as I believe Mr. Cook has done, we may for purposes of illustration consider a genus as a sort of archipelago of such islands, the members of the group being separated by rather shallow channels, while the archipelago is itself separated from other similar groups of islands by deeper channels. It has been urged that a type-species, when accurately defined, furnishes a sort of latitude and longitude of one island in the archipelago and thus gives one of the best means of locating the group as a whole. To a certain point this is undoubtedly true, yet the information, however important, is by no means sufficient for purposes of classification, for it is obvious that to know the precise position of an island in an archipelago gives no clue to the shape and extent of the group. To acquire this latter knowledge we must measure and sound the intervening channels. Let us carry out our figure a little further and agree that the breadth of a channel indicates the extent of actual difference between the plants in question, while the depth of the channel indicates the fixity of this difference. It is obvious that islands may be far apart and yet be divided only by very shallow water, the space between them being from its inconsiderable depth very likely to be more or less filled with spits, shoals or sand bars. Similarly, as we all know, two species may in their typical forms be very unlike, yet exhibit such affinity that they are more or less connected by frequent intermediates, hybrids or atavistic forms. On the other hand, two islands may be very near together yet separated by an exceedingly deep channel, in which any intermediate bars, shoals or islets are quite impossible; and in like manner two species may exhibit close habitual similarity and yet maintain their technical distinctions with perfect fidelity.

It is, of course, these deeper channels, these natural intervals, no matter how nar-

row and difficult to find, that should be diligently sought as yielding the most satisfactory limits of a genus. It is by the relative or complete absence of intergradation that we must recognize differences of high antiquity and profound classificatory significance. It is not by the visible extent of the differences, but by their constancy that their importance is to be measured. This is, of course, no new thesis. It has been reiterated, time and again, in one form or another, by many distinguished biologists. Yet it is a principle habitually disregarded by many systematists, and to its neglect is due a large part of the annoying diversity in current classification. Large genera are daily being divided and new ones artificially created on the basis of differences which are regarded as important solely from their magnitude or conspicuous character and with scarcely a thought as to their constancy. But it is easy to see that botanists, who confound differences of degree with those of kind, or lay great stress on the wide divergence of certain type species and fail to take into account the species which are intermediate, are like reckless mariners who estimate the depth of a channel solely by its breadth.

The great majority of new generic propositions rest upon the examination of a comparatively small number of species; indeed, a considerable part of them are brought out in works relating to some geographically restricted flora. In such cases, it is a practise all too common to treat the newly proposed generic segregates as though they were made up solely of their representatives which chance to inhabit the particular region studied. In many instances this enables the writer to define his genera with a specious definiteness, which may appear very convincing to those whose botanical activities are restricted to the same local flora, but which, when viewed in the light of a broader knowledge of re-

lated extra-limital species, is quickly seen to be the merest artificiality.

This introduces a subject which I would earnestly emphasize, namely, that those who attempt to alter generic lines should always take a broad, a cosmopolitan, view of the group concerned. To do so, may, it is true, involve great difficulty. It is a relatively easy matter to divide into groups two or three dozen species of any one of the great genera like *Euphorbia*, *Solanum*, *Cyperus*, *Eupatorium*, *Polygonum* or *As-tragalus*. It is a very different task to examine all known species of any of these huge genera and show that they may be divided into definable and mutually exclusive groups; but it should be perfectly evident that nothing short of a definite disposition of all the component species of a genus can be construed as a satisfactory and scholarly generic segregation. It would probably be within the bounds of truth to say that wide-reaching divisions of large genera have within the last decade been frequently undertaken without the examination of more than a quarter or in some instances a tenth part of the species involved. Such attempts may be compared if we return a moment to our marine simile, to the guesswork of a negligent explorer, who, finding an archipelago entered by a deep inlet or bay, should, after sailing a short distance into it, conclude that the channel continued at the same depth and in the same direction through the rest of the group, and who should, therefore, record such a clear channel on his chart, leaving to future mariners who attempt to sail by his map the unenviable task of discovering by sad experience the real course and depth of the channel. Neither such a chartographer nor such a botanist is likely to enjoy long a reputation for accuracy or scholarship.

On the other hand, it may be urged, with some plausibility, that many large and tra-

ditional genera contain very unlike elements and that those writers who clearly perceive this should not be obliged to maintain the heterogeneous aggregate, even though they may not be in a position to go into the great task of examining all its foreign representatives and deciding in which component group each should be placed. It may be argued, further, that to impose such a burden of work as a prerequisite to every more sweeping generic change would be to retard very greatly the progress of classification. These, however, are mere excuses for hasty and superficial work. In fact, just such expressions as 'composed of very unlike elements' are exceedingly apt to be based on differences of magnitude rather than those of constancy. Persons examining our indigenous species of *Polygonum* might very naturally suppose the sections *Persicaria* and *Avicularia* distinct genera. It requires a knowledge of the Old World species to perceive how untrustworthy are the distinctions by which these subordinate groups are separated. The caryophyllaceous groups *Alsine* of Wahlenberg and *Melandrium* of Röhlings are commonly maintained as genera by writers of central Europe and if only European species are considered these might seem fully worthy of generic rank. It is after a study of Asiatic and American species that the alleged generic distinctions are seen to be weak and inconsistent. It is an easy matter to separate sharply our few species of *Oxalis* into groups on the basis of homogony and heterogony, on the color of the petals, and on the nature of the root-stock. It is a very different task to arrange clearly in these proposed genera the species of Africa, in which yellow petals are sometimes associated with a bulbous base and purple petals with elongated leafy stems, in which heterogony bears no definite relation to color of petals, and in which

sealy rootstocks and bulbous base are actually found in the same individual.

The genus *Hexastylis* of Rafinesque has been recently restored and separated from *Asarum* on the ground that the former has a superior ovary, distinct styles, and filaments shorter than the anthers, while in the latter it is said that the ovary is inferior, the styles united, and the filaments are longer than the anthers. This might seem to be an excellent generic division were it not for Asiatic species of *Asarum* in which an inferior ovary is combined with distinct styles and nearly sessile anthers. Examples of this sort might be almost indefinitely multiplied.

In all these cases the generic segregation is weak, ineffective and unscholarly because it rests on incomplete observation. It is, moreover, distressing to see what a strong prejudice there is on the part of many investigators against taking a broader and more cosmopolitan view of plant classification. If genera appear to be good in a particular region, that is regarded as quite justifying their maintenance in floras treating of that part of the world. This tendency toward insularity is to some extent perceptible in the work of different European nations. It is far more noticeable in the growing separation of the taxonomic work of the eastern and western continents. It is obviously the outcome of the narrowing influence of specialization and should be vigorously combated, for it is seriously threatening not only the uniformity and harmony, but the normal progress and dignity, of our subject as a whole. For it is quite evident that were it to be once admitted that generic limits rest not upon species of the world but are to be fixed in each flora merely according to their chance representatives in that flora, it would introduce a chaos into classification as ridiculous as it would be unnatural. Two genera might be justifiable in the limited flora

of Connecticut, which would completely merge were the other New England species considered; or two genera might present marked differences in New England, which would intergrade in the middle or southern states. It is obvious that if we are to obtain any measure of uniformity or stability, genera must be founded not on local, but on a cosmopolitan basis.

It has often been suggested that excessive splitting of genera has been due to personal vanity of authors who enjoy creating the new binomials involved. However this may be, I much doubt whether any such unworthy motive has played great part in the matter, but infer that many authors share the view of Rafinesque that large genera are unwieldy things; that it is a very difficult matter to prepare a good key for such large numbers of species and that classification is rendered simpler and clearer if the genus can be broken up into fragments of convenient size. In regard to this it may be said that the subdivision of the genus into subgenera or sections accomplishes precisely the same end with no confusion of generic nomenclature. Different minds may work in unlike manner when confronted by the difficulties of identifying plants. Personally, I should very much prefer to have the difficulty at one point rather than at two; that is to say, I should rather have generic lines drawn so widely that genera would be pretty definite and readily recognized, in the manner, let us say, of *Cyperus*, *Astragalus* or *Euphorbia* in the broader and long traditional sense. The recognition of such genera requires little or no mental effort on the part of a botanist of any training. The attention is left free for the specific identification and this may be undertaken with a happy confidence that all the species likely to come into question will be found in the same group and under the same generic name. These species may be inconvenient-

ly numerous, but at least one is not disturbed by any lurking doubt whether, after all, he has got the right genus. I can see no real simplification in having *Mariscus* and sundry other vaguely marked sections of *Cyperus* treated as separate genera, so that in identification one must first struggle with the generic key and then, haunted by some misgivings as to his success in this matter, proceed to the specific key. As I have said, minds may act quite differently in this regard, but clarity of classification is after all a very artificial consideration and it is dearly bought if secured at the expense of misleading statements. Therefore, I can not believe that a writer when dealing with a limited flora is justified in stating that two genera are distinct if he is wilfully or carelessly neglecting their more or less complete intergradation in some other flora.

It may be thought that the sort of world-wide perspective which is here advocated is a thing well nigh impossible; that a writer who attempts a flora, for instance, of the United States will have his hands quite full with the difficulties of his already herculean task and that his progress would be slow indeed were he forced to estimate each generic difference by the large standards of the world's flora. Here I must use a much-dreaded word, quite a bug-bear of many a self-reliant scientist, namely, *authority*. It is indeed quite impossible for any one of us to repeat the accumulated work of many decades which has led to the recognition of hundreds and thousands of genera. We must accept at least a large part of them on authority. The absolute necessity of this is so self-evident that it needs no justification. I am not advocating any slavish submission to the opinion of others, nor minimizing the importance of verifying and in every possible way correcting previous work, but merely urging a wholesome respect for the opinion of

those whose intensive monographic work or broad cosmopolitan outlook has given them an exceptional opportunity to see just where generic limits may be most naturally and profitably drawn.

Fortunately, the genera of flowering plants have been comprehensively treated in two or three works of high excellence. These sources of information are sufficiently complete and recent to be of the greatest service to writers on restricted floras. In addition to such general guides there exist for many families very detailed monographs embodying the expert opinions which are the result of long and critical study of all members of the particular affinity concerned. It is not to be denied that such monumental works exhibit a considerable measure of diversity in the interpretation of genera, but this is not wholly a misfortune since it permits a certain elasticity in classification and enables a certain selection according to varying judgments. Furthermore, however great the divergence may be in these large monographic and cosmopolitan works, it is relative uniformity compared with the state which would soon obtain were genera to be defined in each flora solely according to its local representatives.

One of the most unhappy tendencies observable in modern classification is a gradual letting down of standards, a feeling that if a few ill-defined genera are to be found in a particular family, the others should in the interests of a sort of specious symmetry be cut up until all are about of the same degree of vagueness and uncertainty. When an author who tends to excess in dividing genera feels called upon to assign a ground for his action, it is in nearly all cases that the segregates he is making are quite as good genera as many which already exist.

This process of taking the poorest existing work of others for a guide or as a

sample of what is permissible might obviously be carried on forever. In the great majority of cases this type of classification is the work of authors who have quite lost sight of the fact that it is not the magnitude but the constancy of the differences which is of real significance in classification. There is, for instance, no more dangerous theory than that because a particular trait, let us say the presence of a corona, forms an excellent generic distinction in one family it must necessarily be of generic significance in another.

The frequent occurrence in botanical literature of such expressions as 'a generic difference,' 'distinctions of generic rank,' etc., seems to indicate a more or less widespread feeling that differences of a particular kind or magnitude, or relating to special plant-structures, are in some way recognizable as diagnostic characteristics of generic value as opposed to those which can be used merely for the separation of species or varieties. There is a common idea, for instance, that a difference of floral structure, or especially one of fruit or seed, is almost infallibly trustworthy in the separation of genera. This belief has certainly the justification that distinctions in the essential parts of the flower or in the fruit are much more apt to be fixed, are at least far more constant, than those of habit, foliage or pubescence, since the latter appear much more subject to modification with varying environment. Nevertheless, this theory, as a rule of classification, may easily be carried too far. When carefully examined, very few differences in the structure of flower or fruit will be found to be invariably of generic value. Nearly all appear at some point in the plant system to be in a fluctuating state. For example, the position of the cotyledons, although possessing a high value in separating many cruciferous genera, completely breaks down in the genus *Lepidium*, where

in plants of the closest habital similarity accumbent and incumbent cotyledons are found. In the Rosaceæ the unlike adnation and connation of the floral parts furnish in some portions of the great family excellent generic and even tribal distinctions. In other rosaceous genera, however, the same diversity of adnation and connation occurs in species which are obviously of such close relationship and so connected by intermediates that their generic separation is by no means convincing. Many other instances of this kind might be cited to show that a difference having great classificatory significance in one place may be almost valueless in another.

It is fair to inquire how we know this. If, for example, genera of the Cruciferae can be readily separated by the position of the cotyledons, why is it not after all the most logical course to treat *Lepidium virginicum*, with its accumbent cotyledons, as constituting a genus distinct from the other species of *Lepidium* in which the cotyledons are incumbent? There are two pretty obvious reasons against this. One is that in *Lepidium* there are other species which have transitional cotyledons, exhibiting various degrees of obliquity. In the second place, the accumbent cotyledons of *Lepidium virginicum*, although a striking character, are unaccompanied by any other difference of moment. From this fact we may reasonably infer that in this particular group the difference in the position of the cotyledons is of relatively recent origin, for it has not had time to become correlated with any other trait of significance or constancy. This brings us to a matter of great practical as well as theoretical importance in classification, namely, that few, if any, genera carry conviction as natural groups, or, to be more precise, naturally delimitable groups, unless they can be separated by more than one feature. The ideal genus is certainly one in which several distin-

guishing traits are constantly associated. When limits are properly drawn it is certainly true that a very large number of such ideal genera exist. Unfortunately for the peace of mind of the systematist, however, there are considerable series of species in certain families, which quite defy classification into genera of this sort. They are groups in which we are forced into accepting a far less satisfactory type of generic division, and in some cases it is necessary to make the most of a single character. Of this fact, the genera *Arenaria* and *Stellaria* furnish an excellent example. So far as I am aware there is no technical distinction between these genera except in the petals, which are entire in *Arenaria* and bifid in *Stellaria*, and this difference becomes very weak in certain species in which the petals are merely emarginate, yet I doubt if any systematist at present wishes to unite these two large and traditionally maintained genera.

It is thus clear that by common consent we are willing to preserve for the sake of practical convenience certain familiar genera, even though precise technical grounds are lacking and a few intermediates occur. In other words, there is an historic element which will obtrude itself into our classification. Its influence is obviously opposed to a truly natural and symmetrical system; nevertheless, much as it may be deplored on theoretical grounds, it is an undeniable fact, and as such demands careful scrutiny and consideration.

Perhaps no large family shows the importance of the historic element in generic classification better than the Compositæ. The lack of sharp boundaries between such genera as *Aster*, *Erigeron* and *Conyza*, *Solidago*, *Aplopappus* and *Bigelovia*, is patent to all whose botanical experience extends beyond the limits of some local flora, yet so far as I am aware no botanist has been willing to accept a recent proposi-

tion to merge several of these genera into one. The genera have through long recognition become a practical aid in classification. They are groups into which at least a very high percentage of the component species may be naturally associated, those of intermediate character being, notwithstanding their undeniable existence, relatively little in evidence.

But it may be asked whether the recognition of such somewhat ill-defined genera is not quite at variance with our fine theories that genera should be groups capable of clear and mutually exclusive definition, archipelagoes separated by deep and safely navigable channels. Certainly this is quite true, but I must beg you to notice that I am in no wise urging any one to recognize weak or poorly limited genera; I am merely stating the undeniable fact that certain groups of this sort are now so widely recognized as genera that we have no choice in the matter. *Aster*, *Erigeron*, *Conyza* and the like have become established facts in classification. They fail to reach our ideals, but this does not of necessity mean that we must let down our standards elsewhere. Attention may be called to the fact that when they were first proposed these genera were, so far as they were then known, quite as distinct as could be desired; their present vague and merging state being due chiefly to intermediates discovered long after the founding of the genera. With the increase of knowledge these genera which once seemed distinct have grown together, the process has been a gradual one and at no time has it seemed desirable to abandon the idea of these genera, although it has become increasingly evident that it is to a great extent artificial. This is one way in which the historic element has entered into our classification. We may readily admit that the authors establishing these old and now merging genera were in most instances justified in doing so, because

according to the knowledge of their time the groups seemed pretty sharply separable, yet that does not mean that we should take those merging genera as a model for our own work. It is, for instance, a very different matter at this time to pronounce the great and long united genus *Astragalus* a complex of distinct genera, for with our present knowledge of the gradual transitions between these alleged segregates of *Astragalus*, their treatment as separate genera involves a sort of suppression of facts.

It is for this reason that writers who divide large genera on the ground that the component parts are just as good genera as many which are traditionally maintained are doomed to the disappointment of seeing their propositions neglected. We may readily pardon the older writer who in his limited knowledge of species made some incorrect inferences as to generic lines, who pointed out the probable channels about his archipelago on the basis of a conscientious but necessarily restricted exploration, made, let us say, with a still ineffective sounding apparatus. It may even, for purposes of geographic orientation, be worth while to let his hypothetical channels remain on our charts, their shallowness and danger being duly indicated. It is quite another matter when we are directed by modern writers to record such channels on the chart at places where we know that they do not in reality exist. But it may be asked why, if it is sometimes desirable to leave the record of an old error, may not the very similar new proposals be equally useful for purposes of geographic orientation. If it is worth while to let old and now merging genera stand, why not make new ones of the same kind? But this end can be accomplished equally well by the use of subgenera or sections, and that too without our seeming to indicate that differences are more constant than they really

are, without our being obliged to record a navigable channel where none exists.

The division of large genera into several smaller ones is commonly accompanied by a certain loss about which little has been said, the obscuring, namely, of the existence of the larger affinity which had been indicated by the old and more comprehensive genus. However diverse the elements of *Oenothera* may be, it can not be doubted that they form a recognizable whole, of which the constituent species are more nearly related to one another than they are to *Epilobiums*, *Gauras* or to species of other onagraceous genera. In other words, *Oenothera* in its comprehensive sense is a natural, although perhaps rather loose group. If we show the narrower affinities, by the use of subgenera, we still have the word *Oenothera* left to cover the larger relationship. This is a matter of importance, for it gives the student at once the information that the affinities of the parts of the genus are closer than those of the genus to other genera. If we give up the old genus *Oenothera* and substitute a group of small component genera, we lose sight of the larger affinity and our classification is accordingly less clear and rich in its statement. It has suffered a distinct loss in the abandonment of the genus *Oenothera* in the larger sense. An added disadvantage is that we are making the constituent groups, although they are of a nearer affinity and clearly belong to a subordinate rank in classification, appear as if they were co-ordinate with other genera which are still treated in the larger way. It is clear, therefore, that the division of a loose but more or less natural genus is attended by some disadvantages quite unconnected with any considerations of sentiment or temporary inconvenience.

We have seen that the difficulties of classifying plants in a really natural and logical way are somewhat increased by the

involuntary and well nigh necessary admission of a certain historic element into our systems. There is another source of this artificiality, besides the temptation to allow poor genera to stand, on the ground of long usage. The relation of a genus to its name is a matter which exerts no small influence in this regard. The attempt to determine which of several names is to be retained for a given genus constantly forces us to consider the historic basis on which the genus rests and to attach its name to some species or group of species to which it was first applied, to determine, in other words, what was the type of the genus, and to maintain the genus in such a way that it may always be true to its type. While sympathizing to a considerable extent with those botanists who desire to place our nomenclature upon a more secure basis by attaching the names to recognized types, I feel that the methods employed will have to be very cautiously applied or they will tend greatly to increase the artificial element in our system. The historic type is not a natural thing; it is merely that particular form of plant life which was, often quite by accident, first discovered and, therefore, first received the name which it bears. Later discoveries often show that this first species of a genus is by no means of a typical, or, as one may say, central character. It is often quite peripheral, perhaps even an aberrant or outlying member of the group to which it belongs. However important the historic type may be in nomenclature, it is obvious that it is of no particular significance in classification, and any employment of the type method in the determination of proper names must not on any account be permitted to exercise any influence in classification. The word type itself is decidedly unfortunate as thus applied to what is often very far from being typical. In this as in some other phases of taxonomy it is of the greatest

importance to keep it clearly in mind that nomenclature, although very necessary to classification, is a thing wholly apart from the classification itself. It is, furthermore, quite evident that nomenclature should be subservient to classification and that the clearness and accuracy of classification should never be sacrificed in order to give beauty or symmetry to any system of nomenclature.

I have now stated my premises and perhaps you are looking for some conclusions, or possibly some practical suggestions as to the best way of obtaining a greater harmony in the matter of generic classification. The difficulties of the problem are quite apparent. The limitation of genera has always in the past rested on individual judgment and it must continue to do so in the future. There is no way of making all people think alike on a subject so intricate and I am by no means certain that complete unity is really needful or desirable. The fact remains, however, that, although the genera of the flowering plants have now been scientifically studied for about two centuries, there is at present in America, at least, a degree of diversity in their interpretation which is rather discouraging. It is disheartening because it is impossible to see in it any real progress toward a well-rounded and satisfying system, which will win the confidence of the professional botanist, give uniform training to the student and command the respect of our colleagues in other branches of science. From this, I think that it is perfectly clear that botanical systematists have certain imperative duties in regard to this subject. These duties are, in the first place, great caution in making changes, and in the second place, a feeling of obligation, when these changes seem necessary, to state the reasons for them so clearly and forcibly that they will appeal to all thoughtful and discriminating workers in the same field. The burden of

proof should always rest upon the writer suggesting the change. It is rather surprising to notice how lightly this matter is taken by some, who attempt sweeping changes. It is by no means rare to see a few habitually similar species of a large genus split off and set up as a new genus with scarcely any attempt to give accurate definition to the new group or tell just what traits are of diagnostic value in separating it. The authors of such work indolently and carelessly shift the burden of proof upon others. Their statements regarding genera are scarcely more than hypotheses, but unhappily they are expressed not as theories or mere conjectures, but as facts. This, however, is not the worst type of publication on this subject, for it is even more misleading to assign generic characters as in the case of the segregates of *Oxalis*, for instance, which do not hold good. Do not understand me to say that authors have been intentionally misleading, for I do not believe that to be the case. I merely mean to say that some writers who have made rather free changes in generic classification have taken so lightly their responsibilities in doing so and have felt so little the obligation to present any complete or scholarly proof of their dogmatically stated conclusions, that they have been tempted into rash and hurried assertions, which are in many cases decidedly misleading.

It may perhaps be thought by some that this is unduly hard on writers who have intentionally adopted a lower grade of generic classification than the somewhat ideal one described a few moments ago, who believe in the practicality of treating as genera minor groups of allied species, although it is by no means maintained that these are sharply defined or non-intergrading. To this it may be said that these minor groups, being of a different classificatory rank from the larger long-established

genera, such as *Cyperus*, *Oenothera*, *Astragalus*, *Ranunculus* and the like, should not in any natural or well-devised system be treated as coordinate with them and bear the name genus, this name having been applied to groups of a superior rank. This seems to me a very important matter. For if it were considered proper to apply the term genus to smaller and smaller groups with more and more vague definition, there is surely no end in sight and no real progress toward a definite and reliable system. To draw an illustration from another science, it may be noted that each generation sees an added perfection and accuracy in apparatus for physical measurements of space, time and gravity, but the physicist does not feel it necessary on this account to shorten the meter scale, make his clock run faster, or file down the weights of his balance. His old constants remain as treasured acquisitions and it is by careful reference to them that his progress is made.

What we need in botanical classification is a series of such constants, a number of graded categories which can be generally endorsed and properly respected. Standards as definite as those of the physicist are, of course, quite unattainable in dealing with the variable and often intergrading groups of organic creation. But where absolute accuracy and uniformity are impossible, we should the more diligently seek to preserve such standards as exist. As has been pointed out there are but few families of flowering plants which have not been comprehensively treated by monographers who so far as their particular group was concerned have been in a position to see pretty clearly where it was best to draw generic lines. While it must be admitted that there are many minor differences in the generic concepts exhibited in the scholarly and monumental works to which I here refer, yet they establish a good usage, which on the whole has a consider-

able measure of uniformity and which goes far to establish the rank of such categories as genus, species and variety. This fact is clearly shown by the contrasting work of those free-lances who, armed with the less effective weapons of a more restricted knowledge, have in doing independent battle upon the difficulties of generic classification followed other tactics and set up new standards. I doubt if they have realized how quickly and fully the personal equation is recognized in regard to their work, or how generally even the amateur and layman grasp the fact that their generic and specific propositions are not up to the standard. No one can change the temperature by making the degrees of his thermometer smaller. Least of all is it possible to make people believe that the shortened degree is as important as the longer one. Time spent in this mere letting down of standards and shifting of ranks is worse than wasted. The process is annoying and confusing, for to the natural difficulties of generic classification plus a certain inevitable historic element of artificiality, it superimposes the most awkward and irritating difficulty of all, namely, the personal equation.

Let us get something done and not spend our time in endless and profitless strife about first principles, thereby bringing confusion into what may be regarded as fairly well established already. There are limitless fields for further profitable work in the finer classification of the flowering plants without perpetual tampering with the boundaries of important and long-studied genera—a type of activity very prone to sink to the level of a mere juggling with names. Having said so much against generic changes of a superficial character, I fear some of my hearers may get the impression that I am opposed to generic changes in general and perhaps even to the further investigation of generic limits;

but this is in no wise the case. There is certainly great opportunity for further and very profitable study of generic classification. The genera of several families, as for instance the Cruciferae, are in many cases pretty artificial groups. We need much further knowledge of the relationships of the species concerned. Let those who wish to be of real service in this matter give us what we so much desire, namely, additional light upon the ontogeny, embryology or finer anatomy of these species, sources of information sure to yield data of high classificatory importance.

In closing let me urge that, while we remit no effort to secure further light on this subject, there should be a general agreement to treat the accepted and traditional interpretation of large and important genera as sacred and binding until we can furnish definite and convincing evidence that change is needful, and that for the welfare and dignity of our science, all should unite in opposing changes of the artificial sort, which consist merely in the shifting of ranks and modification of standards.

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*INVESTIGATIONS AND COMMERCIAL TESTS
IN CONNECTION WITH THE WORK OF
AN ENGINEERING COLLEGE.¹*

IN any school it is necessary, in securing the best efficiency in instruction, that the professors shall be able to speak with authority on the subjects which they teach. In technical schools those who teach the practical engineering subjects can not speak with authority unless they have had practical experience. Investigations and commercial tests may serve to give them this practical experience, and the question naturally arises—is it a good policy for pro-

¹ Address of the vice-president and chairman of Section D—Mechanical Science and Engineering, at New Orleans, December 29, 1905.