TAXONOMY, SYSTEMATIC BOTANY AND BIOSYSTEMATICS

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Before me are copies of several works in which are used the terms "Taxonomy," "Systematic Botany," "Biosystematics," and "Experimental Taxonomy." They include works by such authors, among the botanists, as Linnaeus (1753), De Candolle (1813), Lindley (1830, 1853), Le Maout and Descaisne (1876), Radlkofer (1883) and his student Solereder (1899), Engler and Gilg (1924), Wettstein (1924), Hall and Clements (1923), Bremekamp (1939), Turrill (1942), Gilmour and Turrill (1941), Benson (1943), Clausen, Keck and Hiesey (1939, 1940), Camp and Gilly (1943), and among the zoologists such writers as Dobzhansky (1941) and Ernst Mayr (1942). I seek an understanding of the scope of Plant Taxonomy in terms of its organization, the sources of its materials, its research methods and its objectives. More particularly, I seek to understand whether Taxonomy, Systematic Botany and Biosystematics have any separate and independent standing or whether they are to be regarded as wholly or partially synonymous with each other. For the most part, I am not searching for definitions: I am interpreting usage, oftentimes over and above, or in spite of definition, for it is usage and the history of usage that ultimately molds the meanings of our words and terms. I am familiar with the current concepts of plant taxonomy and how its scope and its methodology have grown with the advancement of the science of Botany. I know the confusion in the literature, and in the minds of botanists, between the terms "Taxonomy" and "Systematic Botany." And I have grown up along-side of Biosystematics and have shared the enthusiasm of its workers, am aware of its values, and know something of its limitations. Let me make it clear that I write not as an authority who would seek to impose what may seem to be his somewhat arbitrary views upon the uninformed but rather as one who has given the subject long and serious thought and who now wishes to present his tentative conclusions to open forum for discussion.

The word "taxonomy" was given us by the elder De Candolle. He used it as a heading for a part of "Theorie Elementaire" with the subheading "Theorie de Classification," a phrase which he presented in the text as a definition of taxonomy. However, he oriented the principles of taxonomy toward seeking a basis of resemblance (symmetry) among plants in order to explain their "relationship." Relationship was thought of solely in terms of the community of characteristics that accounted for resemblance. His principles, although not entirely acceptable today, were largely those of a systematic approach to comparative morphol-

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ogy, chiefly organography. In later chapters, he discussed natural arrangement, the concept of the taxonomic categories, and phytography, which has been termed the art of description, and included in it what we today term nomenclature and synonymy. Thus the classical taxonomy was concerned with classification and the establishment of relationship and used as its major tools comparative (systematic) morphology as the source of fact, a system of taxonomic categories, a system of nomenclature, and precise description.

There is no doubt but that the tangible product of the labors of such men as De Candolle was a system of classification, but in view of the principles outlined by him and the logic of their arrangement, it is also perfectly clear that the system of classification did not stand as the end and objective of taxonomy but rather through its arrangement into a system of inclusive categories, and its nomenclature it served as the vehicle of expression of the relationship that the taxonomist sought to depict. Thus the system of classification, rather than solely an end, is also a tool of the taxonomist. It is his working hypothesis. It seems clear then that from the beginning of taxonomy, the stage was set for a clear differentiation between the overall objectives of taxonomy and the tools it fashioned and established to achieve its ends. These tools included a source of, and a method of arriving at facts, and a method of presentation or arrangement of these facts to express the relationships among plants. Thus from the beginning Taxonomy was a synthesis of facts into an expression of botanical interrelationships.

I have not as yet established just where and with whom the term "Systematic Botany" originated. From its early use, it must have followed very closely upon the term "taxonomy" and might possibly have even preceded it. Lindley used it in 1830 but seems not to have mentioned it in the 1853 edition of "The Vegetable Kingdom" in which he gave an elaborate survey of the history of taxonomy. Certainly the term "systematic" pertained to the "System" and presumably the "Natural System." Its early use seems to have been as a casual self-evident term, but later definitions associated it with seeking the evidence of relationship and this must mean the relationship that is associated with community of characters rather than the phylogenetic relationship which followed the theory of evolution. It was not until the effective application of the methods of the plant anatomists to taxonomy under the impetus of Radlkofer that we find the term used with enthusiasm. Here, quite definitely, it was used in the sense of a "new Taxonomy" with Systematic Anatomy as its major tool. Taxonomy, however, is ageless; there is no old and no new Taxonomy. There are, however, new methods of arriving at facts and new integrations possible from time to time and it is these new methods that constitute the new Taxonomy of succeeding generations of botanists. Radlkofer and his students defined "Systematic Botany" much in the sense we have outlined Taxonomy as established though usage by De Candolle and his contemporaries. These plant anatomists, however, emphasized arriving at relationships and left for Taxonomy only classification and nomenclature. They glorified the anatomical method as the true method of determining relationship yet, in effect, many but not all of them accepted the system of the early taxonomist and in an orderly manner built up an enormous volume of fact about each systematic group of plants without at the same time utilizing these facts to establish relationship. The type example is the magnificent work of Solereder, "The Systematic Anatomy of the Dicotyledons." Here the anatomical method is not used to build a system of classification, as one would suppose from Solereder's introduction, but rather the working hypothesis of Bentham and Hooker was accepted as a method of approach to the subject of Systematic Anatomy. Solereder's work stands in its own right as a solid and valuable contribution to Plant Anatomy and to Taxonomy. If we accept the usage of Solereder, this is his Systematic Botany in spite of his definition, and it can be put to any use to which the facts of Comparative Anatomy may be utilized. Such a Systematic Botany is enormously valuable to Taxonomy by virtue of the accumulation of facts along a systematic pattern. It is "systematic" primarily by virtue of canvassing, however thoroughly, the anatomical features of the system. It is a method of arriving at comparable facts and is indispensable to the taxonomist in testing his hypotheses. Whereas it can and must be utilized to the fullest by the taxonomist, it certainly is not synonymous with Taxonomy.

Because of the confusion in the literature relative to the terms Systematic Botany and Taxonomy, it may seem now to be a matter of arbitrary choice as to which we shall accept for the classical Taxonomy. I would choose the older term "Taxonomy" for this role for, if we accept the term Taxonomy in the sense of the usage of those who use "Systematic Botany" in the broader sense, then what is here considered under the heading of Systematic Botany will either have to be considered under Taxonomy, in a restricted sense, or a new term created for it. It is desirable to keep it separate because its conclusions are wholly objective and fully documented in contrast to those aspects here considered under the Taxonomic System, which are almost wholly subjective and involve interpretation in terms of concepts of taxonomic categories and of phylogenetic series of morphological characters. Systematic Botany, as I see it, involves research techniques and a research point of view that uncovers facts which may also be utilized independently of any taxonomic usage. I have found no evidence that Taxonomy ever has been used in this particular restricted sense while certainly Systematic Botany was so used even though the authors who so used it defined it in a broader sense, a sense they did not adhere to in their own work.

With the development of the various branches of Botany to the point that a systematic approach to their problems will yield fruitful results, there will be an increasing need for the term "Systematic Botany," used in the precise sense in which Solereder em-

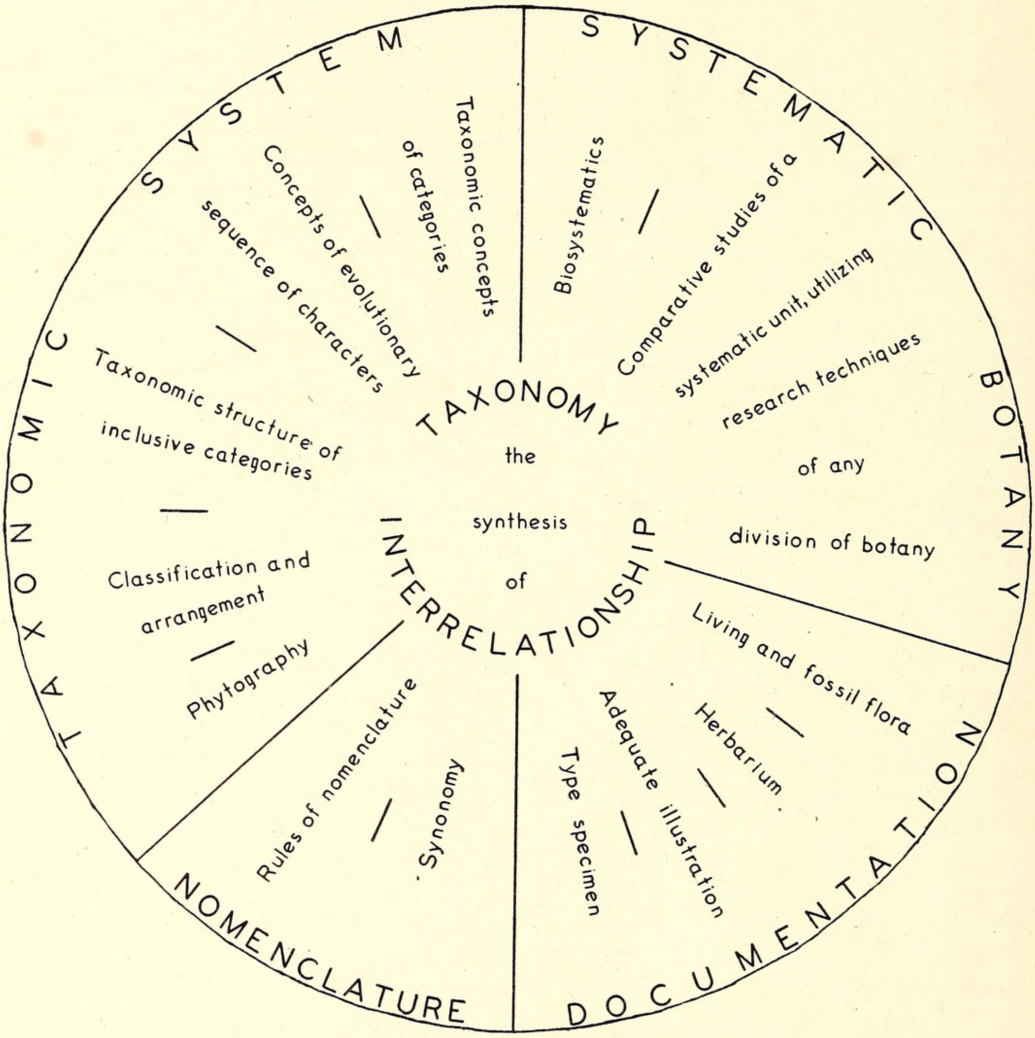

FIG. 1. The organization of the science of Taxonomy. Four fields are represented. The field of research and investigation (the fact-finding processes) are centered in Systematic Botany. The facts so determined are assembled and classified according to concepts of the evolutionary sequence of characters and organized into the series of inclusive categories. These categories are then arranged in the taxonomic structure in such a way as to give expression to the pattern of relationship. A system of nomenclature is employed according to rules, and superfluous names are relegated to synonymy. The botanical facts and the nomenclature are documented by 1) the flora of living and fossil plants, 2) the herbarium, 3) the type specimen or, 3a) by adequate illustration. Each segment of the diagram constitutes a significant tool of Taxonomy and plays a part in this synthesis of interrelationship.

ployed it. Already we see evidence of a rapidly formulating Systematic Cytology and a Systematic Genetics. Some significant beginnings have been made in a Systematic Biochemistry. "Comparative Botany" (Comparative Anatomy and Comparative Morphology, etc.) is not adequate to the needs of Taxonomy unless it presents a systematic comparison. The comparison, to be significant, must be throughout a systematic group. The connotation of a "systematic group" is significant to the concept of Systematic Botany and of Taxonomy because of the implication of relationship. It is an implication that the systematic approach to a basic discipline is designed to test. The systematic approach to each of the divisions of Systematic Botany will yield documented facts that may be of use to the taxonomist in his synthesis of interrelationships. They constitute the materials for a method of pitting the facts of one discipline against those of another to test hypotheses of relationship. We may define Systematic Botany as the comparative study of any related (systematic) group of plants utilizing the research techniques of any of the divisions of botany. In contrast, Taxonomy is the synthesis of all of the facts about plants into a concept and expression of the interrelationships of plants. Systematic Botany is its major source of botanical fact.

At the time of De Candolle, only Comparative Organography had accumulated a sufficient body of fact and methodology about it to enable systematic studies to be fruitful. Later, Comparative Anatomy reached a point where certain additional but limited systematic approaches were available. It was not until near the end of the first quarter of the current century that Cytology and Genetics jointly found themselves in a position to approach their field from the point of view of systematics, and Taxonomy finds the results of this work of very great significance. Further advances in the investigations of the vascular anatomy of flowers and of the anatomy of stems and leaves have provided new facts and methods of value to Taxonomy. It is significant that the theory of evolution completely revolutionized the point of view in Taxonomy and gave direction and clear meaning to the concept of relationship but it did not immediately provide additional facts. It resulted only in the reconsideration of those facts already established. Since community of characteristics serves as the evidence of relationship under the evolutionary concept as it did under the natural systems, certain putatively phylogenetic systems differ little from the older natural systems, and all are strikingly alike in many of their details. The chief differences between modern systems result from differences in point of view in interpreting developmental trends in the evolution of morphological characters.

It should be pointed out that Taxonomy can advance only to the extent that the basic fields of research in botany upon which it rests have accumulated documented, comparable facts of utility to the taxonomist. Should these fields go to sleep or fail to awaken, Taxonomy must also sleep. During such times, Taxonomy may be brought up to date but it cannot advance.

There has recently come into prominence a new term, namely, Biosystematics, and it has been hailed as the "New Taxonomy," as appears to have been the case with the term "Systematic Botany" that preceded it. This over-emphasis arises out of the enthusiasm of its workers for the impetus that it has provided to Taxonomy. And like the early anatomists, some of the biosystematists are convinced that this is Taxonomy and that there is no other adequate approach to Taxonomy. Thus, some would take unto Biosystematics the classical objectives of Taxonomy and again leave only classification and nomenclature to Taxonomy and to Systematic Botany. I think that it is now clear that Biosystematics is a valid and very important aspect of Systematic Botany as here outlined and, as such, a significant tool of Taxonomy. It, however, is on the plane of integration as well as of comparison. It utilizes Comparative Cytology and Comparative Genetics. It seeks to determine relative crossability and relative intersterility and seeks an analysis of the genetic constitution of natural and potential interbreeding populations. It attempts to classify the genetic units of such populations in terms of the cytogenetic phenomena that initiate them, the isolating mechanisms under whose sanction they develop and the nature of the environmental forces that permit their survival. To this end, it uses such terms as ecospecies, ecotypes, ecoclines and cytotypes, though this terminology is as yet a long way from being settled as to its precise role in Biosystematics. Since these same genetic and cytological phenomena are responsible for the morphological and physiological character of the individuals that make up natural populations, Biosystematics is sometimes fittingly spoken of as "studies in the nature of species" and its dynamics are often spoken of as "speciation." It approaches the ideal Taxonomy when it seeks to establish phylogenetic relationship among the members of a genus. It is our only means of establishing true genetic relationship.

This brings up the point as to the difference between phylogenetic relationship of the taxonomist, and genetic relationship as construed by the geneticist. Phylogenetic relationship is the system of genetic lineages that have brought us our species populations as they exist today. Its interpretations are drawn from the irretrievable past. It is the field of relationship that is of major concern to the taxonomist and he would like nothing more than to be able to reconstruct the pattern of genetic lineage that has been traversed. The phrase "genetic relationship" etymologically means precisely the same thing as phylogenetic relationship. As this phrase is used by geneticists, however, it means more than that. It includes also, in effect, the entire genetic potential of related species populations, the known facts of which are arrived at through manipulation to determine relative crossability and the relative intersterility of progeny. It is concerned with the sum total of genic materials capable of being exchanged and the methods by which this may be accomplished.

The difference is entirely comparable to the difference between genetic relationship and genealogy in any randomly selected group of people or between the races of man. It would require only a relatively few generations of carefully supervised mating to mix thoroughly the gene pattern available. It would require an enormous number of generations back through time to reach a point of common genealogical origin for the individuals or the groups. Obviously such groups are more closely related genetically than they are genealogically. Similarly is it possible for certain species, subspecies or populations to be more closely related through genetic potential than they are related phylogenetically. The taxonomist cannot operate in the field of potential situations. He must therefore draw a careful line between the dynamics of the past and those of the future. He can use genetic facts of the present where they shed light upon the dynamics of the past but he must leave the future to the elements of chance that may build natural populations under the sanction of a selective natural environment. When these are accomplished, he will have additional material for his synthesis. A case in point is evidence indicating the close genetic but distant phylogenetic relationship of *Platanus orientalis* L. and *P. occidentalis* L. through their fertile hybrid, *P. acerifolia* Willd. Here the old and new world world parents have presumably been separated at least since Miocene time if not since Cretaceous time and have diverged significantly through genetic processes both morphologically and physiologically so that the taxonomist has recognized in them two species. When brought together artificially, they cross with the production of a fertile hybrid. Some geneticists and biosystematists argue that this is evidence that they should be classed as subspecies of a single species, however remote the phylogenetic ancestry. It is important here to realize that both the morphological and the physiological divergence of these two species are each the result of genetic change, resulting, presumably, from the accumulation of enormous numbers of successive gene mutations or other chromosomal changes. Somehow these did not also involve a sterility barrier, or if they did, a reverse mutation or a compensating mutation that wiped it out may have occurred later. In any event, no one has demonstrated that the progeny can or will establish themselves in a natural stand and serve as a bridge for the persistent exchange of genes between the two parents. This is an interesting case but let it be emphasized that the taxonomist cannot deal in futures. It is obvious that not all of the total genic complement involved in potential genetic situations goes into every lineage. There is no way of predicting how much or what part of the genic complement will be involved in any particular case and whether or not the resulting combination will survive environmental selection. The taxonomist can operate effectively in such cases primarily on morphological, physiological and palaeontological evidence. On this evidence these divergent populations have reached a level of differentiation which permits them to be regarded as taxonomically different and yet there is sufficient evidence of relationship such as permits them to be included in the same genus. Whether they be classed as species or subspecies will depend upon the judgment of the classifier in assessing the role of the two entities in the taxonomic system of the genus. We do not as yet know enough about the nature of sterility barriers or of retained fertility to assess them phylogenetically. We can assess them only in terms of present day genetic potential and genetic opportunity.

It is likewise possible that two entities may be more closely related phylogenetically than the degree of possible gene exchange between them would indicate. Through any of several cytogenetic mechanisms a sterility barrier may be erected isolating one population from another so that no further gene exchange between them is possible. Such a population in its inception may be only one generation removed from the other phylogenetically, yet the genetic barrier between them may be complete. When polyploidy is involved as the cytogenetic mechanism, morphological and physiological differences in the progeny sometimes are achieved that are of such magnitude as to warrant taxonomic recognition. Here, again, the taxonomist is interested not only in the taxonomic rank of the entity but also in the phylogenetic lineage involved over and above the genetic evidence derived from manipulation.

On the other hand, where natural introgression can be demonstrated, it is obvious that we are dealing with the accomplished junction of two lineages in a reticulate pattern of phylogeny. The taxonomist must consider such cases. It seems therefore important for the taxonomist, in adjudging the findings of the biosystematists, carefully to consider in each case whether it is evidence of an accomplished natural fact or of a potential situation whose unfulfilled actuality lies in the nebulous future.

It is possible to work biosystematically only with plants between which genetic manipulation is both possible and practical. This of course limits the sphere of its usefulness enormously. Its usefulness is chiefly on the subspecific level. Often, however, it yields facts that prove eminently useful to the taxonomist in formulating his species concepts. Biosystematics then furnishes another source of facts organized systematically to stand independently in their own right, or some of which may also effectively serve the taxonomist in his synthesis.

Some biosystematists prefer to speak of their field of research as experimental taxonomy. If we can assume that experimentation and manipulation are synonymous, such a term as Experimental Taxonomy might be regarded as validly applied. However, I think most workers regard the results of experiments as strictly objective since experimentation presumably establishes proof or disproof. We may establish proof of genetic relationship in the limited sphere where this is possible. If we can then demonstrate that the genetic relationship is precisely what the phylogenetic relationship has been the term Experimental Taxonomy is a valid one. In the overwhelming majority of cases, however, it is necessary to adjudge the situation in terms of our concepts of phylogeny before assigning taxonomic rank and position. In the postulation of such phylogenetic relationship, we strive toward an ideal through the exercise of judgment. It is doubtful whether judgment, apart from the facts upon which it rests, is subject to experiment. Because of this, most genetic manipulation in Taxonomy falls short of being an experiment.

Another aspect of Taxonomy is what has been termed the taxonomic system. There are many botanists who would regard the system as the ultimate goal of Taxonomy since it provides the basis of classification. There is, however, confusion of concepts evident in this point of view since classification is the basis of building the taxonomic system, rather than the taxonomic system the basis of classification. The taxonomic system may serve as a tool in identification or may serve as a tool of expression of relationship.

It is important to point out that today orders and families are placed in the system not to indicate a lineal sequence of phylogenetic relationship from order to order or from family to family. Rather, these names stand in lieu of combinations of characters, and their arrangement expresses successive modifications of characters thought to have been followed in the evolutionary history of the orders and families. The plant groups as we know them today are obviously the end points in many such phylogenetic sequences built upon a divaricate pattern. What lies before us now is a cross section of the phylogenetic lineages that have resulted in the orders and families as we know them today. How far back in the lineage a given genus or a family or an order may have diverged is now wholly conjectural. It would indeed be hazardous to assume that of two orders arranged in sequence the higher originated in the lower. The utility of the phylogenetic system rests in the fact that by knowing the characters of the orders and families, the botanist can interpret the system and can find an expression of the interrelationship that is construed to exist.

The taxonomic system involves the arrangement of plant groups in a series of hierarchical categories, which I shall refer to as the taxonomic structure. This at once introduces a dual concept as to the meaning and use of taxonomic categories. They may be spoken of in relation to the taxonomic structure, wherein they constitute an arrangement of empty abstract categories, or they may be spoken of in relation to the taxonomic system, wherein we deal with actual plants and animals organized in terms of the categories of the taxonomic structure. When we use the term "species" without reference to any group of organisms under consideration, we are speaking of the abstract category in the taxonomic structure. When we speak of the species of the genus *Pinus,* we are referring to organized entities of the taxonomic system. Because of difficulties involved in attempts at definition of taxonomic categories, it is important that this distinction be kept in mind. The categories of the taxonomic structure involve a series of inclusive groups of different value as one descends from the kingdom through phylum, class, order, family, genus, and species. The wisdom of past experience has dictated that the taxonomist purposely refrain from defining these categories in any way that will impose restrictions on the freedom with which he may express the interrelationships that he construes to exist. However, the inclusive sequence or relative position of the categories with respect to one another is important and is fixed by international agreement (Briquet, 1935). This constitutes a basis for the relative evaluation of the categories. A relationship among plants that one taxonomist may wish to express in terms of three families in one order may be regarded by another taxonomist as better expressed in terms of three separate orders. This constitutes a legitimate difference of opinion wholly consistent with the nature of the facts and with the objectives of taxonomy. The facts necessary for an objective Taxonomy are not at present available except possibly on an exceedingly limited scale. However, in order to organize our materials into a workable system, it has been necessary to bridge the gaps in our knowledge with hypotheses. For this reason, the systems of classification are largely if not wholly subjective. They constitute the working hypotheses of the taxonomist. The hypotheses, however, are not nebulous; they have been based usually upon comparative morphology interpreted in terms of current concepts of what constitutes evidence of relationship and are subject to modification as new evidence from any source may develop. The expressed relationships are mostly incapable of proof, but are subject to personal acceptance or personal rejection by taxonomists. The "empty" categories in the taxonomic structure are incapable of inclusive and exclusive definition in any way that is phylogenetically or taxonomically significant. It therefore seems evident that there can be no absolute system of classification arranged on a structure of clearly defined and precisely evaluated categories. Under these circumstances, the taxonomic structure will better serve the needs of the taxonomist if its categories remain elastic and relative. An illustrative case is presented by *Phlox gracilis* Greene of the Polemoniaceae. During its taxonomic history, it has found a place in *Phlox, Gilia, Collomia, Microsteris,* and *Navarretia.* Professor Wherry, a student of the Polemoniaceae and especially of the genus *Phlox,* sees in this species characters which he deems sufficient to exclude it from *Phlox.* Being informed also on the other genera in the family, he knows that it is not at home in any of these, so he follows the latter decision of Greene and elevates it to the category of a separate genus, *Microsteris.* So long as Professor Wherry construes evidence of relationship as he does, he is absolutely correct in his disposition of this species. I, who also am a student of the Polemoniaceae, see in this species evidence of such close relationship to *Phlox* that I believe that the objectives of taxonomy are better served by including it in *Phlox,* even though it demands subgeneric status in that genus. So long as I construe relationship as I do I am equally correct in placing it in *Phlox.* I think Professor Wherry and I understand each other in this matter. There are no definitions of the genus or of the species, as such, that compel us to unify our treatment. The important point about this example is not what happens to nomenclature and not that two experts disagree. It is rather that each of us is free to express relationships as we see and interpret them in terms of a system of classification and a nomenclature that may be judged by those botanists who may wish to use them. The resultant differences in nomenclature and synonymy serve as tools in the taxonomic system and reflect two concepts of relationship, or five concepts of relationship if we consider the entire synonymy. Taxonomy is obligated to a stable nomenclature only to the point of consistency with its objectives. When synonymies are once worked out and properly cited, nomenclature is stable to anyone who understands the objectives of taxonomy, the use of indices, and the methods of citation. This is little enough to expect of all informed botanists. To many other branches of learning, synonymy is a welcome symptom of the richness of the language. To the taxonomist, it is a tool of expression for his concepts which he hopes will not become unwieldy. The non-taxonomist, it seems, looks upon synonymy as a symptom of the *rigor mortis* of taxonomy.

There have been many attempts by taxonomists and others to define the categories of the taxonomic structure. Many of these attempts rest on an assumption that the category possesses objective reality. And most of these have attempted to define the species category. Their authors, unfortunately, have largely confused concepts involved in definition with concepts involved in characterization as well as concepts of phylogenetic relationship as construed by the taxonomist with those of genetic relationship as construed by the geneticist. The concept of definition involves the circumscription of limits; a definition must be both inclusive and exclusive. I have seen no putative definition of a taxonomic category so worded as to be incapable of application either to the next higher or the next lower category of the taxonomic structure. That which is a species to one taxonomist may be a subspecies to another, and that which is a family to one may be an order to another. This difference of opinion is wholly consistent with the nature of the known facts and does no harm to the objectives of Taxonomy. The empty category of the taxonomic structure has no foundation in reality and obviously cannot be objectively defined. If the category in the taxonomic system has reality, it may be defined only in terms of the particular plants comprising it. It is my personal belief that there is no significant definition possible that can be applied categorically in the taxonomic structure or in the taxonomic system to the total exclusion of the next higher or the next lower category. Taxonomic categories possess only relative values insofar as we are now in a position to understand them. Until they can be made objective, it is best that they remain relative and elastic in their application. The sphere of relativity is solely with respect to the next higher and the next lower category within the immediate sphere of taxonomic relationship. An interpretation as to what constitutes a species of pine has little meaning relative to an interpretation as to what constitutes a species of *Rosa* even though the specific category is used for both. However, within the genus *Pinus* one is more nearly able to develop concepts of comparative evaluation without at the same time being able to fix these values at their precise level in the taxonomic structure to the satisfaction of all. In such a sphere of relationship, it is strictly within the province of sound logic and observable fact for a taxonomist to hold concepts of species which may cause him to be regarded as a "splitter" with one segment of a group and a "lumper" with another. At its worst, lumping is the tool of the superficial and splitting the tool of the uncritical. There are, however, depending upon the nature of the problem, intelligent uses for each procedure that are both necessary and desirable. In most cases, relationship can be expressed only in terms of relative nearness of relationship, and within the plant kingdom all degrees of relationship exist. This situation can be reduced to a system of rigidly defined categories only by arbitrary decisions which result in artificial categories. *Pinus radiata* is a species of a relatively large genus. Because of certain common characteristics which are construed to indicate relationship, this and other pines are aggregated into the genus *Pinus*. *Pinus* differs in several characters from *Abies* and *Tsuga,* but because of certain other common characters, these three genera, along with several others, are aggregated into the family Pinaceae. In a like manner, Pinaceae, Taxodiaceae, and Cupressaceae differ from one another but are grouped with other families under the order Coniferales. Thus the species *Pinus radiata* is only a small part of the genus *Pinus,* the family Pinaceae, and the order Coniferales. *Ginkgo biloba,* on the other hand, is not only the only living species, but is also the living genus, the family, and the order. Whereas, in *Pinus radiata,* we can enumerate characters that indicate in turn the species, the genus, the family, and the order, in *Ginkgo biloba* there are no characters of structure or function significant to any conceivable definition of these taxonomic categories that can be utilized to designate each in turn in the living member. The position of *Ginkgo biloba* in the taxonomic system is significant only as an order in the class Gymnospermae and coordinate with the orders Coniferales, Taxales, and Cycadales. In this case, there is no foundation for evaluation in definitive terms in any of the categories below the order. Obviously, the living *Ginkgo biloba,* the species, is precisely the same genetically as the living *Ginkgo,* the genus, Ginkgoaceae, the family, and Ginkgoales, the order. There is no way of defining the species on a genetic basis in this case that will exclude the genus, the family, or the order, without setting up some arbitrary criteria. A concept of species is significant only in a genus of more than one entity. I cite these cases to point out that taxonomic categories are relative and that the sphere of relativity rests among the species of a genus, the genera of a family, and the families of an order. It rests in closeness of relationship and not across the entire taxonomic structure.

Taxonomy is one of the few sciences that documents its results by preservation of actual plant materials that have served as a basis for its concepts. Through the citation of specimen vouchers and their permanent preservation in herbaria, the literature of Taxonomy is forever current and always in demand. The Species Plantarum (1753) of Linnaeaus is in continued demand through its documentation in the specimens preserved in the Linnaean herbarium, however archaic the system of classificaiton may be that was utilized in it. Were it not for the Linnaean herbarium, the Species Plantarum and its nomenclature would long since have passed to the limbo of literary novelties, much as the ancient herbals, and serve only to tantalize the student as to what the author might possibly have had in mind when he described, imperfectly or incompletely, this or that species of plant. It could not possibly have been utilized as the boundary of priority of nomenclature. Similar documentation of the results of research should be urged upon the other divisions of botany.

Taxonomy can only bring itself up to date. It cannot advance independently of the basic disciplines upon which it rests. It is important to all of the basic disciplines, however, that Taxonomy keep itself up to date with the state of development of each of them. Knowledge of the interrelationships of plants is the key to many problems that arise in botany, the applied sciences, industry, and agriculture. One discouragement the taxonomist has had to face throughout this past century is that although systematic morphology clearly provided the basis for Taxonomy and the foundation for the natural system and pointed clearly through the theory of evolution to the phylogenetic systems, detailed advances in Morphology and Anatomy beyond this point were for a long time largely inadequate to help him solve his problems. The Morphology upon which classical Taxonomy rests is little more than simple organography involving position and number of parts, adhesion, cohesion, abortion, and modification of floral organs. This method has centered around the concept of Wolff (Samassa, 1896), championed by Goethe (1790) that the flower was a modified shoot and that its organs were homologous with leaves. It has accepted in its phylogenetic approach DeCandolle's idea that in the natural system anything which tended to obscure basic symmetry was a mark of advance in the system. The fact of the matter is that we are not as yet agreed as to what a flower is in terms of its homologies. We are in almost complete ignorance of the phylogenetic sequences in the development of the parts of the flower. There is much disagreement among morphologists and anatomists as to the meaning of vasculation in these organs in terms of arriving at homologies. These and many other unsolved problems of systematic morphology are vital to a sound phylogenetic Taxonomy. Research in Taxonomy has largely skirted these problems, with the result that there has been no sound advance in this aspect of the science. Instead, research in Taxonomy has centered chiefly in organizing the genus and species and ordering the nomenclature.

If Taxonomy is to fulfill its function in the botanical sciences, it must shift its emphasis from purely organizational techniques to include active research in some of the basic disciplines upon which Taxonomy rests. The taxonomist can be of little service to the objectives of his science by the pursuit of organizational monographs executed without contemplation of the basic problems upon which the science rests. The taxonomist must become a morphologist, a cytologist, a geneticist, and we hope, in time also a physiologist, and a biochemist. There is no field of specialization in taxonomy apart from these disciplines that is in any way adequate to the problems that confront the taxonomist. It makes little difference what area of the taxonomic structure or the taxonomic system he is investigating, because, with the exception of the riddles of nomenclature, the problems upon which he passes judgment are all basic botanical problems. In the higher categories, his problems thus far have been concerned chiefly with the evaluation of the structures of the flower as he seeks evidence concerning phylogenetic sequence of its structures. Some new lines of approach are now available from the field of Anatomy. In order that his judgment may be sound, he must master the research techniques of Comparative Morphology and Comparative Anatomy and Histology and apply them to his problem. He must be in a position to judge for himself the controversial matter in these fields as it applies to his problems. If his researches are on the level of species and genera, he must, in addition, master the research techniques of the cytologist and the geneticist, so that he may utilize the tools developed in these sciences for the interpretation and evaluation of variation and intergradation. It is only from these points of view that he will be able to assess his problems in terms of the expression of the interrelationships that are his objectives. The taxonomist must first of all be a botanist with the broadest of training. Only then will he be capable of being a competent taxonomist.

Summary

In the various works dealing with the general field of Taxonomy, we may arrive at many different concepts as to the meaning of the terms "Taxonomy" and "Systematic Botany." Most definitions, however, have regarded the two terms as synonymous even though usage in the same work differed sharply from the definition stated. Since there is need for a term applicable in the sense in which Solereder used Systematic Anatomy, and since little is to be gained by invoking a new term, it now seems necessary to make an arbitrary choice between these definitions and usages.

Through usage, over and above definition, De Candolle supplied the classical Taxonomy with a method and an objective that served to establish the term "Taxonomy" in the inclusive sense we here advocate. His usage involved the methodology and tools concerned with taxonomic research, classification, nomenclature, the taxonomic system and the determination of relationship. Since De Candolle, we have added little to the classical concept of taxonomy except additional and improved method and additional botanical fact. The objective remains the same, namely the classification of plants into a system that expresses their interrelationships. All that the theory of evolution accomplished immediately for taxonomy was to make clear the meaning of the selfsame "relationships" that De Candolle sought, namely the relationship based upon resemblance or community of characters which now becomes the evidence of relationship through common evolutionary descent. Being first associated with the natural systems, the term "Taxonomy" was at its inception inescapably linked with the motive of establishing relationship among plants.

With the adoption of this broad concept of Taxonomy as herein advocated, there is no need today for the term "Systematic Botany" used in a sense that is synonymous with Taxonomy. There is, however, an important use for the term "Systematic Botany." This is its application to the systematic approach to any of the basic divisions of Botany, much as Solereder used the

term "Systematic Anatomy." Its objective will be the establishment of botanical facts upon which concepts of relationship are based rather than simply the establishment of relationship. This is the field of botanical research upon which Taxonomy rests.

With such a concept of Taxonomy and Systematic Botany, Biosystematics fits naturally into Systematic Botany on the plane of integration. It seeks to establish facts about a systematic group in systematic order so that these may be used in the synthesis of taxonomic relationships.

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