

The doubtful virtue of splitting families

C. G. G. J. VAN STEENIS*

ABSTRACT

The criteria for splitting recognized families are discussed, with critical remarks on the arguments sometimes given for the splitting off of new families on the basis of features such as habit and homogeneity. Some examples are provided of the unnecessary creation of additional families.

RÉSUMÉ

VALEUR DOUTEUSE DE LA SCISSION DES FAMILLES

On discute les critères utilisés pour scinder des familles établies, avec des remarques critiques sur les arguments qui sont parfois donnés pour ériger des groupes en familles nouvelles sur la base des caractères tels que l'habitus et l'homogénéité. On donne quelques exemples de création inutile de nouvelles familles.

There is no rule in taxonomy about an acceptable size for plant families. Our judgement is based on comparative morphology and agreement about sets of intrinsic characters, supplemented if possible by data from systematic anatomy, karyology, palynology, chemotaxonomy, embryology, etc. Linnaeus has often been blamed for his rough, artificial handling of systematics and partly obscuring natural affinity in not introducing the family concept, leaving this to Adanson, de Jussieu, etc. I would like to emphasize that Linnaeus deliberately chose to offer a practical system of inventory. He was quite aware of natural affinities and of the family concept, but could not handle it practically in his system. For the rest, though much has been established in this line on a firm basis, we are, after two centuries of hard work, still not unanimous about family delimitation.

Hutchinson, in his *Families of Flowering Plants* (1926, 1934, 2nd ed. 1959), raised considerably the number of currently accepted families. The main virtue of this practice was, in his opinion, that pruning in large families of what he called "anomalous genera or tribes" would tend to make families "more natural by homogeneity". It is certainly true that such an action will lead to greater homogeneity, but the idea that families would then be more natural seems a doubtful philosophy. As I have emphasized before (1965), the lower echelons in any hierarchical system are obviously more homogeneous than the higher ones. It must be remembered, however, that homogeneity and naturalness are not equivalent and that, from a phylogenetic point of view, large families with great diversity may be just as natural as smaller or even monogeneric ones with less diversity and greater homogeneity.

This is admitted by Shaw (1973), but in favour of his splitting tendency he says that "the aim has been to secure a greater equivalence in morphological distinctness between family units and in the gaps separating them"—this can be likened to equal-sized pigeonholes. I want to dispute this idea, because it suggests the existence of some sort of mathematical regularity. It reminds one of the assumptions of Willis and De Vries, who postulated an origin of species and genera along all ancestral lines at fairly regular intervals. This sort of schematic-mathematical philosophy is defeated by the observation of both living and fossil development. One must accept that certain taxa had the potential for broad development (called 'radiation', in modern terms) in morphological diversity, as e.g. Leguminosae, Annonaceae, Rubiaceae, etc., while

others consisted of very few genera with many species (as Pandanaceae and Myristicaceae), or remained isolated as a single genus, as e.g. *Symplocos*. Examples show that such isolated genera can maintain themselves sometimes for a long time: *Symplocos* (Symptlocaceae) thrived as far back as the Eocene, *Nypa* (Palmae) and *Ctenolophon* (Linaceae) were clearly developed in the Upper Cretaceous; they obviously possessed a genome structure without the potential perspective for producing diversity until the present day.

That we should be able to judge the width of gaps by comparable morphological weight seems to me fictitious. Most gaps cannot be measured, but it is certain that they are uneven. Also, we have to accept that some "lumps of phylogeny" are larger than others if measured by their diversity and were more successful ecologically and genetically: in some cases demonstrable missing links have not become extinct, as e.g. in Leguminosae, or in Celastraceae, which must include Hippocrateaceae.

But there is not the slightest scientific gain and sense in splitting *Nypa* from the palms as a separate family, even if *Nypa* differs more from its congeners than the congeners differ among themselves. *Nypa* and the other palms remain one "lump of phylogeny", and even if raised to family rank, they remain close together, as in the case of the six segregate families of the Loganiaceae. The same holds, for example, for the bamboos where nothing scientific is gained by recognizing them as a separate family Bambusaceae distinct from Gramineae, from which their structure and phylogeny are inseparable. The same holds for the redundant Cuscutaceae, which should not become separate from Convolvulaceae; also the breaking up of Hamamelidaceae into three families, the Podostemonaceae into two, the Nymphaeaceae into two, etc., in all some 80 families as proposed by Shaw. It is most unfortunate that all these splittings have been accepted or were newly proposed in Willis's Dictionary with only brief explanation, as this dictionary, especially in small centres, with inadequate libraries is often regarded as a summarized source of up-to-date botanical information. The splitting of families is scientifically gainful only if it can be clearly shown that some genus or tribe is distinctly out of place and must on these grounds be removed and accommodated elsewhere in the system.

Sometimes the historical development of phyto-geographical knowledge must have also played a rôle, for example in the African genus *Panda*. Pierre accommodated *Panda* in a separate monotypic family Pandaceae of which the affinity was not very clear.

* c/o Rijksherbarium, Schelpenkade 6, Leiden, Netherlands.

Recently Forman (1966) found that the closest relatives of *Panda* are *Galearia* and *Microdesmis*, which have always been considered to belong, as a separate tribe, to Euphorbiaceae. They were not particularly aberrant in that family; herbarium botanists with sufficient form-knowledge would not hesitate to pre-identify unnamed material of these genera as belonging to the Euphorbiaceae. I believe that if Pierre had fathomed the affinity of *Panda* with Euphorbiaceae, Pandaceae would not have been raised to family rank so soon.

Another case in which history has worked against the reasonable merging of families is that of the African genus *Afrostyrax*. If Pierre had recognized this as belonging to the Sterculiaceae, the conversion of the tribe Huaceae into a segregate family of Sterculiaceae might never have been considered.

Another principle of Hutchinson's, which has led to the splitting of families, this time more serious, is his idea of a twofold phylogeny of flowering plants, viz. Lignosae and Herbaceae, an idea of the old Greeks. This phylogenetic approach has had peculiar consequences, for example for the Oxalidaceae which he kept in the restricted sense in the Geraniales, while he placed the segregate Avertroaceae in Rutales, and made a new family Lepidobotryaceae (with *Sarcotheca* and *Dapania*) in Malpighiales, although the difference between the two existing species of *Sarcotheca* is greater than that between the last two named genera. The most peculiar aspect of this principle is that Hutchinson stated that "trees and shrubs are older than climbers and herbs", without considering that this principle can also operate within a single family and that in the Oxalidaceae the woody genera could then be older than the climbers and herbs, which would have made a splitting of Oxalidaceae *sensu lato* unnecessary.

In a discussion I had with Shaw on Hutchinson's tendency to split families, Shaw added three other arguments which induced him to follow this policy, viz. (i) that what he called 'anomalous genera' would be otherwise forgotten or not sufficiently recognized or taken care of if they remain included in larger families, (ii) that sometimes such genera (or tribes) represented in his opinion a 'halfway-house' between other families, and (iii) that he was inclined to regard the habit of a plant as an important indication for phylogenetic affinity *q. d.* derivation.

The first argument does not seem valid; it hinges on the question of memory, especially with regard to identification. To memorize, involves knowledge of form and it makes no difference whether a plant belongs to a tribe of a larger family or to a separate, small or even monogeneric family. If raising to family rank does not change the systematic position (affinity), the segregation is redundant, similarly with the raising of tribes to family rank.

The second argument is, of course, a valid one: if one genus, or more, or a tribe is really anomalous in a larger family and shows distinct affinity of about equal value to one or more other families in other places in the system, or appears to have no clear affinities to any family, they must be recognized as families in their own right, representing a discrete "lump of phylogeny". But this argument has always been applied in the past and offers nothing new. So I can well understand that, for example, the genus *Sphenostemon* is out of place in Aquifoliaceae and stands so much alone that it deserves family status (Baas, 1975). And the same can be said of many other isolates like *Ancistrocladus*, *Corynocarpus*, *Paracryphia*, *Scyphostegia*, *Batis*, *Sarcosperma*, *Garrya*, etc.

The third argument is the evaluation of habit as an indication of affinity. This also weighed heavy with Hallier f. and with Hutchinson, and I agree that habit characters are frequently important tracers of identity, hence of affinity. They are a major tool even for sight or preliminary identification of unnamed specimens. Habit is often of great constancy in many groups, from which follows that it is systematically important; it is often more constant than floral details. It consists of a small set of easily observed characters, viz. woody/herbaceous, latex or resin (+ or -), phyllotaxis stipules (+ or -), leaf structure, external or internal glands (+ or -), place and structure of inflorescence, and indumentum.

But habit is a tricky feature and must be handled with caution and one cannot rely on one character only; it may give a distinct lead, but must be substantiated by further taxonomic characters, as similar habit is sometimes found in unrelated families through parallel development. I want to give a few examples of pitfalls caused through emphasis on habit.

Struck by the peculiar, dense, conical, terminal inflorescence of *Sphenoclea*, Shaw stated (1948) that there was no evidence to refer it to Campanulaceae in which it had always been accommodated. He said it resembled *Phytolacca* in habit, while other characters would also suggest affinity to Phytolaccaceae and Primulaceae, the latter observation possibly referring to a similar type of inflorescence of *Primula vialii*. After a careful study, Moeliono (1960) accepted that, though *Sphenoclea* does not have latex, its identity must fall within Campanulaceae, and that Lobeliaceae also belongs to the same ancestral complex. Earlier, Subramanyan (1950) had also shown that Shaw's suggestion of a presumed affinity of *Sphenoclea* with Phytolaccaceae must be rejected on the grounds of anatomical and embryological evidence. Recently Corner (1976) has shown that seed structure is similar to that of Campanulaceae. The one-character habit is obviously a superficial parallel.

Shaw also removed the genus *Pentaphragma* from the Campanulaceae, because it has no latex. This is certainly interesting, but such exceptions must be tolerated and accepted as, for example, in Acanthaceae, where the genus *Staurogyne* does not possess cystoliths (characteristic of this family), but is otherwise clearly Acanthaceous and not "exactly intermediate between Acanthaceae and Scrophulariaceae" as Shaw maintains.

Superficial resemblance in a habit character led Shaw to remove the Australian genus *Blepharocarya* from Anacardiaceae and create for it a separate family Blepharocaryaceae, which he admittedly retained close to Anacardiaceae. In this genus the inflorescence is peculiar in that the axes are fused, lobed and woody; Shaw considers this as "apparently almost exactly homologous to the cupule of Fagaceae". Although it is true that it is morphologically instructive to see how a cupule may have originated, this pseudo-cupule is, of course, no more than parallel development, not homologous, but analogous. The second argument based on its opposite, pinnate leaves does not hold, as pinnate leaves are not uncommon in the family, and *Bouea* also has opposite leaves. It is clear that here the one habit character is overweighted. There is more to it: Shaw should have considered that there is a minor trend or tendency in the development of Anacardiaceae, which has involved in peculiar ways both the axis of the inflorescence and the bracts; in *Anacardium* and *Semecarpus* the pedicel can swell to beyond the size of the fruit, in *Cotinus coccycgia* the stalks and pedicels of the infructescence lengthen considerably after flowering and the inflorescence may

serve as one diaspore, while in *Dobinea* and *Campylopetalum* (split off from Anacardiaceae as Podoaceae by Shaw), the bracts are frequently enlarged to a show-apparatus or the fruit is fused with the leafy enlarged bracteole.

In addition, I may now mention how surprised I was to observe during an excursion to Hermanus in the south-western Cape the monotypic South African Anacardiaceae genus *Laurophyllus** in which the contracted panicle consists of woody, coralloid, fused inflorescence axes and bracts which represents a similar, but not so perfected contraction as in *Blepharocarya*. These facts show that there is a clear trend in the family to 'achieve' something with inflorescent axes and bracts in its different taxa; this I find significant and characteristic of true affinity.

Blepharocaryaceae is redundant as a separate family and I wonder about Podoaceae: was the impetus to split this from Anacardiaceae induced primarily by its habit, because they are rhizomatous herbs or small shrubs?

Recently I had occasion to examine a specimen from Thailand, which I could not place, but which, after analysis of its characters, seemed to belong to the family Tiliaceae, although at first sight the superficial habit reminded me rather of *Combretum* or Malpighiaceae. Precise identification showed it to be *Plagiopteron* of the Tiliaceae. To my astonishment, I found that Shaw had raised it as a separate family, Plagiopteraceae. He claimed it to be an isolated taxon with a curious mixture of characters signalling affinity with several other families of which he mentioned Verbenaceae and Combretaceae (sic). Again, one gets the impression that Shaw was overwhelmed by habit, because of the opposite leaves, as *Plagiopteron* does not show any other taxonomic character which would remove it from Tiliaceae. Though I admit that in various groups phyllotaxis is an important taxonomic feature, in that it is constant for large assemblages, it is known to break down in several families, e.g. Bignoniaceae, or genera, e.g. *Ilex* in which colossal genus there is one species with exactly opposite leaves. Besides, in the Elaeocarpaceae, related to the Tiliaceae, there are some genera with decussate leaves e.g. *Aceratium* and *Sericolea*.

Habit also plays a part in those cases where a single rare feature is so conspicuous that an author assigns to it more significance than it warrants. For example, Hutchinson (1959) was so struck by the thick, disc-like style of *Siphonodon* that he removed this genus from Celastraceae to a new family Capusiaceae, which he placed next to Scyphostegiaceae, containing the single genus *Scyphostegia* in which a fleshy style

* Although I was very pleased to attend the magnificent Wild Flower Show at Hermanus, the highlight of my excursion there with Dr John Rourke and our generous host Dr Ion Williams was seeing *Laurophyllus*, a gem which aroused my systematic feelings; and, of course, the genus is now for the first time represented in the Rijksherbarium.

also occurs. Apart from this character, there is no affinity at all between *Siphonodon* and *Scyphostegia*, whereas the characters of the former fit nicely into Celastraceae.

In this discussion I have tried to emphasize that families should be considered as the highest reasonably discrete taxa representing coherent "lumps of phylogenetic affinity". These lumps may be small or large, homogeneous or diversified, but all equally natural phylogenetically. They are separated by gaps, which are usually unequal, and for which we have no standard of measuring.

Furthermore, I hope that I have demonstrated that in numerous cases the arguments for increasing the number of natural families were based on insufficient or redundant arguments (homogeneity, history, habit, etc.) and that segregating new families can only have scientific value if the segregate does not prove to be the closest ally to the family from which it is split.

In addition I sincerely hope that African botanists, in their effort to understand relationships, will not cursorily adopt these families, which are sometimes split off for inadequate reasons, but rather carefully consider all recent segregates on their intrinsic merit.

Nowadays too many authors of Floras and other botanical works simply follow, for the sake of convenience, the latest system of family classification or accept information provided in recent general dictionaries.

UITTREKSEL

Die maatstawe wat gebruik word wanneer erkende families verdeel word, word bespreek, met kritiese opmerkings oor die argumente vir die afsplitsing van nuwe families op die basis van faktore soos groeiwyse en homogeniteit. 'n Aantal voorbeelde word gegee van onnodige verdelings.

REFERENCES

- BAAS, P., 1975. Vegetative anatomy and the affinities of Aquifoliaceae, Sphenostemon, Phelline and Oncotheca. *Blumea* 22: 311-407.
- CORNER, E. J. H., 1976. *The seeds of dicotyledons*. Vol. 1, pp. 311 & atlas (especially p. 256). Cambridge University Press.
- FORMAN, L. L., 1966. The reinstatement of *Galearia* Z. & M. and *Microdesmis* Hook. f. in the Pandaceae. *Kew Bull.* 20: 309-321.
- HUTCHINSON, J., 1926, 1934, 1959. *Families of flowering plants*. Ed. 1 (2 vols., 1926, 1934); ed. 2 (2 vols., 1959).
- MOELIONO, B., 1960. Campanulaceae. In C. G. G. J. van Steenis (ed.), *Fl. Males.* 1, 6: 109.
- SHAW, H. K. AIRY, 1948. Sphenocleaceae. In C. G. G. J. van Steenis (ed.), *Fl. Males.* 1, 4: 27.
- SHAW, H. K. AIRY, 1973. *A dictionary of flowering plants and ferns* (J. C. Willis). Ed. 8. Cambridge University Press.
- SUBRAMANYAN, K., 1950. A contribution to our knowledge of the systematic position of the Sphenocleaceae. *Proc. Indian Acad. Sci.* 31, sect. 13, no. 1: 1-6.
- VAN STEENIS, C. G. G. J., 1965. Nature and purpose of botanical classification from the standpoint of the producer, particularly with respect to tropical plants. *Bull. Bot. Surv. India* 7: 8-14.