ADANSON AND MODERN TAXONOMY

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THIS article started as a review of the second part of the Hunt Library "Adanson",* of which the first part has already been briefly noted (Notes R.B.G. Edinb. 26:26, 1964). If it has somewhat outgrown that aim, it is nonetheless still centred on the contribution which these two volumes make towards the study of a very noteworthy biologist, and the relevance of his ideas to taxonomy today. The essays in part two are: Adanson et le Mouvement Colonial (J. P. Nicholas); Les Dessinateurs d'Histoire naturelle en France au XVIII siècle (G. Duprat); Mathematics and Classification from Adanson to the Present (P. H. A. Sneath); L'Oeuvre zoologique d'Adanson (T. Monod); Adanson's Sources, References and Abbreviations (F. A. Stafleu); and, finally, there are the speeches made at the presentation of the Adanson medals as well as an evening discourse on Franco-American activities in botany by Roger de Vilmorin. Together with the essays in part one, these comprise a very thorough and admirably executed survey of Adanson, the man, his time and his work.

The neglect of Adanson by his successors is mentioned in several of the contributions; let me add to the record, therefore, one note of praise that is not quoted here. This is from none other than Lamarck, who wrote (Discours préliminaire, *Encycl. Méth. Bot.* p. xxxv, Paris, 1783; ed. nouv. p. xxxvii, Padua, 1784): "M. Adanson . . . aura toujours la gloire d'avoir publié le premier des familles de Plantes. . . ." Lamarck discusses appreciatively various aspects of Adanson's work, but detects one of the inconveniences: that Adanson's fine long family descriptions must not, as a matter of principle, be reduced to shorter, more practical, diagnostic characters. Lamarck is also puzzled by Adanson's subdivisions of his families. He notes that in 'Liliacées' they are named as though they were families within families, "ou des sous-familles, si l'on peut s'exprimer ainsi", whereas the 'Personées', Lamarck says, are divided by simple characters into true sections, that is to say artificial divisions, to which no names are given. "Y a-t-il, donc, indépendamment des divisions artificielles que l'on peut établir, plusieurs sortes des familes naturelles?" Adanson did indeed build genera first into smaller and then into larger families and he sometimes used the word 'family' in rather different senses. Monod (pt. 2, p. 512) warns us of this and explains some of Adanson's usages in Mollusca. Though the above extracts show that Adanson's work provided Lamarck with food for thought (and if this was so at the level of the family, it can scarcely have been otherwise for species), I agree with Sneath (pt. 2, p. 494) that Chevalier probably overran the evidence in attributing to Adanson the germ of Lamarck's evolutionary ideas.

Sneath himself, however, reiterates the claim, almost equally extravagant that Adanson is the father of the methods of numerical taxonomy. Stafleu

* Adanson. The Bicentennial of Michel Adanson's "Familles des Plantes". Edited by G. H. M. Lawrence, Pittsburgh, Pennsylvania, The Hunt Botanical Library, Carnegie Institute of Technology. 2 volumes: part one, 1963; part two, 1965. 637 p., 48 figs. (together Hunt Monograph Series no. 1). Price 15 dollars.

combats this,* and indeed tells us (pt. 1, p. 244) where the legend of Adanson's arithmetical methods started, in an article by Adrien de Jussieu in 1848. There is another legend, that Adanson individually classified every one of the 1615 genera that he recognized in each of his 65 artificial systems. This is found in Sneath's earlier papers, is repeated in his essay (pt. 2, p. 483) and has been endorsed by Davis & Heywood (*Principles of Angiosperm Taxonomy*, p. 23: 1964), who speak of Adanson's "single-character systems in which the position of one genus relative to another varied greatly . . ." and say " . . . he believed that the genera which were found side by side in the greatest number of systems should be classified together."

I can find no evidence that Adanson ever used his single-character systems at the generic level. They are analyses of his families.

What we do know of Adanson's methods of working. He tells us (*Fam. Pl.* p. clviii):-†

"Je faisois d'abord une description entière de chaque Plante, en metant dans autant d'articles separés, chacune de ses parties, dans tous ses détails; & à mesure qu'il se présentoit de nouveles Espèces qui avoient du raport a celles déja décrites, je les décrivois à côté, en suprimant toutes les ressemblances, & en notant seulement leurs diférences. Ce fut par l'ensemble de ses descriptions comparées, que je m'aperçus que les Plantes se ranjoient naturelement d'elles-mêmes sous des Classes ou Familles . . ."

"*Que je m'aperçus* . . ." These are the key words. Genera and families were arrived at by *perception* not by any counting up of resemblances or differences (cf. Sneath, pt. 2, p. 483). Many recent authors refer to this perceptive working as *intuitive* classification; I have done so myself on occasion. The term fits nicely with the conception of taxonomy as an art rather than a science, and of taxonomists as dependent on an inborn flair rather than scientific study. *Intuitive* is a bad word in this context. The beginner, even one with a flair for taxonomy, who classifies by intuition is deceived time and again by superficial resemblances. Only by training and experience is natural ability developed to the state when some of the reasoning processes in classification become so rapid as to be almost subconscious. It is desirable to replace intuitive in this context by some other word; I tentatively suggest *perceptive*, showing that the classification depends on the perceptions of the trained mind rather than on the deliberately reasoned analysis of the data.

The view that taxonomy is an art rather than a science is false, because art and science are not antithetic. All science contains a proportion of art, in almost any sense of the word art. That point does not concern these essays on Adanson, but my final disagreement with current terminology in the theory of taxonomy does. What is *a priori* weighting? The advance of Adanson's methods over those of his predecessors is praised by both Stafleu and Sneath. He insisted that as many characters as possible should be taken

* See also his review of *Phenetic and Phylogenetic Classification* (ed. Heywood & McNeill) in Taxon, 14: 136 (Apr. 1965).

† Freely translated: "First I make a complete description of each plant, putting each part, with all its details, under a separate heading: and as new species, having resemblance to those already described, come to hand one by one I describe them alongside, suppressing all the resemblances and noting only the points of difference. It was by the overall view of these comparative descriptions that I preceived that plants range themselves naturally into classes or families . . ."

into consideration, rather than allowing importance to be attached only to those concerned with reproduction, as did some of his predecessors and contemporaries. Adanson applied a much-needed corrective in this. But it is difficult to define the weighting of characters that was current at the time as being *a priori*, despite Stafleu's arguments (see more particularly his introduction to A.- L. de Jussieu, *Genera Plantarum*, reimp. 1964, p. xxi). Especial importance was allotted to reproductive characters because the resultant classifications were so much more satisfactory than those based on medicinal properties or vegetative features. It is true that limited experience led to generalizations that were far too sweeping (but which were quietly ignored when inconvenient in practice); also that the real importance of reproductive organs easily accepted the graft of an "Aristotelian" view of their "essential" value; but the weighting of characters against which Adanson reacted so strongly was not in the philosophical sense *a priori*. The only system that strictly merits this term is that in which it is decided, *a priori*, that all characters shall be treated as equal.

So called *a priori* weighting was really the attachment of a value to characters in one group from experience of their reliability in another. This might be better called *extrinsic* weighting. By contrast characters evaluated from studies within the group concerned could be called *intrinsically* weighted or, as Sneath and others say, weighted *a posteriori*. I have elsewhere (in *Phenetic & Phylogenetic Classification*, ed. McNeill & Heywood, p. 15: 1964) suggested that characters treated as equal should be termed *isocratic*. These three terms, isocratic, extrinsically or intrinsically weighted (or evaluated), would cover the whole range of the way in which characters are used.

Adanson himself was keenly interested in the evaluation of characters. In fact it is in the assessment of such values that he had his only recourse to arithmetic. He gave (*Fam. Pl.* p. cccxi-cccxii) a summary table of his 65 artificial systems which were each based on a single character. The column he headed "classes" are what we should now call character-states: the number of alternative categories in which a family might be placed in respect of the character under study. Naturally many of his families possess the character in more than one state, and were therefore entered in more than one place in that system. The column headed "sections" indicates the number of entries (families or parts of families) in the system concerned. The final column headed "natural sections" indicates the number of families which, being constant for one character-state, did not have to be split into sections. To this column Adanson added a fractional figure of natural sections/sections, which was in effect a measure of the constancy of the character in his families. For instance the number of styles had a constancy figure of about $\frac{1}{4}$, whereas that for the number of stamens was only 1/36. Clearly a constancy figure of unity would be obtained if each family possessed the character in only a single state. It does not occur.

I have considered Adanson's little sums in detail because the fractional figure is omitted in Stafleu's reproduction of the table (pt. 1, pp. 199–200) and neither he nor Sneath comment on this, the only numerical assessment in the book. It must be mentioned that the figures given by Adanson in his summary table do not always agree exactly with those to be derived from the systems themselves, though they are always of the right order. Perhaps this

merely proves that Adanson was not a numerical taxonomist. Certainly the claims that he actually counted character differences (e.g. Sneath, pt. 2, p. 478) cannot be upheld.

It is difficult to see the force of one criticism of Adanson's methods made by later authors, that they required a knowledge of all plants and all their characters. Perfection requires complete knowledge whatever the method and all methods fall short of perfection in its absence. Numerical taxonomy may need only a sample of characters (Sneath, pt. 2, p. 491—though the number he demands, 50 or better 100, sets the labour of their acquisition high), but to say that it needs only a sample of the species is to enter more dangerous ground. In studying the generic groupings of the Old World Gesneriaceae I am concerned with perhaps 1000–1200 species: the real generic difficulties probably stem from some 100–200 of these. A numerical classification based on a 50% sample is likely to have lost half my problems at the start.

Experiments in numerical taxonomy will assuredly produce interesting information; it may well be that there are points in the plant kingdom where major problems will be solved. The plain fact remains however that the chief impediment to good classification is sheer ignorance of the plants on a world-wide basis. Next in importance is the difficulty of measuring, or classifying into satisfactory character-states, the more complex characters, such as the corolla-shape of zygomorphic Gamopetalae. Numerical taxonomy does not avoid this: indeed the effort necessary to obtain suitable data may well yield important contributions to taxonomy.

The real gulf between traditional and computer-dependent taxonomists can be seen from some remarks Sneath makes (pt. 2, p. 478) when discussing the weighting of characters:

"For example, parallel venation is not an important character in constructing the natural group of monocotyledons: to assume this is to assume the existence of a natural group, the monocotyledons. We do not know about this, since at this stage of the study the *naturalness of the monocotyledons is precisely what we are calling into question.*"

Sneath approaches taxonomy with a clean slate on which can be written a new, isocratic, classification. The groups of traditional taxonomy have been built step by step by a method of successive adjustments; organisms have been added or subtracted, and characters revised and refined. The taxonomist's argument (as in the interplay of the character *parallel venation* and the group *monocotyledons*) may sometimes seem to approach the circular, but J. B. Conant once remarked (in Proc. Amer. Acad. Arts & Sci. 80:12, 1951) that Pasteur would not have advanced biochemistry as he did if he had not argued almost in a circle. There can be little doubt that biology lacks an adequate theoretical bio-logic to sustain some of its practices. The classifying of living organisms is not necessarily to be ruled (though it may be aided) by the logic appropriate to the classification of inanimate objects (cf. Rollins in Taxon, 14: 1–6, 1965).

Give a traditional taxonomist a pile of 5000 specimens to arrange in a classification. He will sort and re-sort them by eye, check and re-check, dissect each to confirm or revise his sortings. The groups he makes will grow and be arranged under his hand: he will know their strengths and weaknesses. The computerly-minded will make a wide survey of the material in order to decide on 50 or more characters suitable for use, and he will

settle the states for which each character will be scored. Then he will make the necessary quarter of a million observations for the computer to sort and correlate. He will know nothing of the groupings till they come out of the computer.*

This essential difference between numerical and traditional taxonomy will stand, even though the latter may now incorporate statistical techniques such as multivariate analysis and the use of discriminant functions (for a survey of some possibilities see Sokal in Biological Reviews, 40:337–391, 1965). Numerical taxonomy uses statistical methods to form groups whereas traditional taxonomy only uses them to discriminate more precisely between groups already perceived. If it becomes increasingly apparent that there is a fundamental divergence here, let us remember Whitehead's dictum, that a clash of doctrines is not a disaster—it is an opportunity. The stimulus of discord must be made to yield clearer thoughts on the reasoning that underlies taxonomic practice.

Adanson would enjoy the claims put forward on his behalf, and he would enjoy the argument they provoke. We are mightily in debt to Dr. G. H. M. Lawrence and the Hunt Library for bringing a fine botanist back into the limelight. If we now quietly bury the terms Adansonian and neo-Adansonian, which we certainly should do before they cause more confusion, there is no danger that Adanson himself will again be forgotten. Yet, one other thing is needed. Adanson may now be more understandable to us in his principles, but in particular detail he remains nearly incomprehensible. His use of Linnaean generic names in non-Linnaean, usually pre-Linnaean, senses and the large number of 'barbarous' names that he himself introduced, make the details of his work almost inaccessible. Stafleu has, in his article *Adanson's Sources References and Abbreviations*, provided the first indispensable tool which will enable someone to compile a full account of Adanson's genera. Stafleu himself has pointed out the need (pt. 1, p. 219) and has done a sample of the work in elucidating the genera of Malvae (pt. 1, pp. 221–228). Who will carry on?

* In passing it may be noted that he *will* have some foreknowledge of the best way to key out the groups when they are obtained. For this will depend only in part on the constancy of characters in the groups (cf. Sneath, pt. 2, pp. 475–476): the best characters for a dichotomous key are bound to be those that have been chosen and scored in such a way that their possible states fall into two clearly distinguishable groups.

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