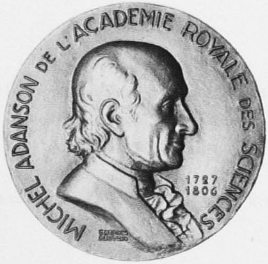
ADANSON

The Bicentennial of Michel Adanson's «Familles des plantes»

Part Two



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I INTRODUCTION: MATHEMATICS AND CLASSIFICATION

MATHEMATICS IN ADANSON'S DAY

In order to understand the views of Michel Adanson (1727-1806) on classification, we need also to understand the state of mathematics in his day. Adanson was at heart a follower of Descartes (1596-1650), and, like many of his contemporaries, probably had an inner conviction that all natural phenomena could be expressed in fairly simple mathematical terms—if we knew how to do so. The 18th century had the shining example of Newton (1642-1727), who had reduced the phenomena of mechanics and optics to a simple system. The mathematician of Adanson's day, with the logarithms of Napier (1550-1617) and Briggs (1556-1630), and the calculus of Newton and of Liebnitz (1646-1716), could manipulate with great facility and accuracy all manner of simple and invariant data. But one class of knowledge, in particular, was then (and still is) refractory to this treatment—the phenomena of biology. It may well be that we will in time greatly simplify it in the manner of Newton. Certain universal principles in biology are now emerging, such as natural selection, or the physicochemical basis of heredity.

Living organisms are characterized by possessing enormous amounts of "information" that may affect the characteristic that we are measuring; in other words, they are very complex. In contrast, the apparently complex revolutions of celestial bodies can be represented by a few parameters that can be measured rather exactly—mass, velocity, direction, gravitational constant, radiation pressure, etc. Adanson seems to have appreciated this, as shown by a comment on Copernicus in the *Familles des plantes* (1: xcv). There is in a sense "no more information" in the system as it is thus idealized. The physical properties of a star can be represented by some few parameters on its atomic composition, mass and age. In one sense this is "all there is" to this star. We could replace it by a star similar in these respects and expect to find precisely similar physical properties. The "recipe" for making a star is simple and concise. But this is not so in biology. Even a bacterium possesses an enormous amount of information required to make the exact details of its enzymes, perhaps in size equivalent to a printed book. In a single nucleus of higher organisms there may be a potential store of genetic information equivalent to that in a library (Sokal & Sneath, 1963). Now while the exact description of a star involves the description of position and velocity of all its particles (which formally requires a huge store of information), yet the details matter very little. It matters little if two atoms in a star are interchanged. Indeed, philosophers and physicists may debate whether this statement has any scientific meaning. But in an organism it may matter a great deal if two atoms are interchanged in its genetic information store: this may produce a lethal mutation, or one affecting any or many of the characters we study. The complexity is increased by the inevitable interactions with a variable, unstable environment.

The implication of this is that biological reactions are very varied and unpredictable. An organism may react to a stimulus in many different ways. We need only consider the outcomes of holding a match to a heap of gunpowder and to the nose of a tiger! The first may explode, but it cannot sneeze, roar, run away, or eat you. The great dependence of any biological characteristic on a host of other factors has led engineers and physicists to call them "soft" data, as opposed to their own "hard" data. Soft and hard refer to certainty of being true in all situations likely to occur in the work in hand. Softness, then, conceals complexity.

Yet there is one method of dealing with "soft" data, and that is through statistics. Instead of trying unsuccessfully to prevent variation, we make variation our ally. The *central tendency* (such as an average, or mean) and the *degree of variation* (such as the standard deviation) become our new parameters. Equally important is the idea that we can make composite qualities from a number of unrelated qualities. This had entered science through physics, where, for example, *inertia* was expressed in terms of force, distance and time. The idea that the union of a very large number of apparently unrelated qualities could yield a meaningful composite quality was only developed with multivariate statistics, and a sound logical basis for this is only now being elaborated. It is just such composite qualities that are needed for complex biological data. We see a groping toward such a concept in Adanson's idea of *the relations between all characters* of the plants that were being compared. The concept of composite qualities is, of course, very old, but we are only now beginning to realize how closely it is related to our own mental processes in philosophy, language, and everyday life.

The idea that an overall measure of similarity could be used in taxonomy was probably not due to Adanson alone, since many contemporaries also discuss it in some measure, in phrases such as "natural relations." Adanson first gave it careful consideration, but its use required first the development of multivariate statistics. In Adanson's day statistics was scarcely in existence, though the Normal Distribution was known to De Moivre (1667-1754), and Pascal (1623-1662) had laid the foundations of probability theory. Some statistical problems were studied by Jakob Bernouilli (1654-1705) and Laplace (1749-1827). It was not until the early 19th century that Quetelet (1796-1874) pioneered demographic statistics. Multivariate analysis, on which present-day numerical taxonomy is based, was still far in the future, and it was near the end of that century before Friedrich Heincke (1852-1929) made an early attempt to apply statistics to taxonomy (Heincke, 1898). The logical algebra of Boole (1815-1864) was similarly a late development, and one that was needed to develop the logical basis of taxonomy.

MATHEMATICS TODAY

What strikes the observer with some force is the way in which mathematics has today proliferated to an extent that makes communication between its branches difficult. Most applications to classification have been based on statistics (itself now almost too large for anyone to know it all). An occasional contribution has come from set theory and Boolean algebra (*e.g.*, Gregg, 1954) or from multidimensional geometry (*e.g.*, Sokal, 1961). Yet in higher branches of field theory and numerical analysis there may be techniques of great value for systematics. The other major advances in theoretical mathematics in the 19th and 20th centuries—*e.g.*, fields of Galois (1811-1832), the transfinite numbers of Cantor (1845-1918), the geometries of Riemann (1826-1866), topology, algebraic analysis, functions of Fourier (1768-1830), matrix analysis—have so far given little help to classification, possibly because they have not been explored for this. On the practical side, however, the development in recent years of electronic digital computers has been quite essential. Only by their aid can we handle the large amounts of information necessary to analyse biological data in the manner required.

A BRIEF DESCRIPTION OF MATHEMATICS IN PRESENT-DAY CLASSIFICATION

This section gives a non-technical account of mathematics in taxonomy, which we may for convenience divide into four logical steps, which must be carried out in order, and which we commonly do not distinguish when thinking about classification.

First, organisms and their characters must be chosen. Adanson is here important because he insisted that all parts of living creatures should be employed in taxonomy. We use mathematics here in deciding the number and kinds of organism and character that are needed.

Second, the organisms are compared with each other. The first attempt to use mathematical methods for this was probably that of Heincke (1898). He employed, in fishes, a measure of taxonomic distance based on many characters. Since then a number of *measures of similarity (resemblance,* or *affinity)* have been devised in different sciences.

Third, the taxa must be constructed (estimates of similarity alone do not group the creatures into taxonomic groups). This grouping process (commonly called *cluster analysis*) is a more recent development, and among early methods are those of Tryon (1939) and Sørensen (1948). The next step in the classificatory process is thus achieved.

Fourth, is the step of extracting from the data the characters best able to distinguish the groups found—and hence suitable for making diagnostic keys for identification of specimens. This has been developed mathematically by Fisher (1890-1962) in his classic paper on discriminant functions (Fisher, 1936). Fisher's method assumes in practice that discriminatory features have been selected from the large number of other features, a point that has had little attention even today, though the work of Lubischew (1962) is a step in this direction.

II PRESENT-DAY TAXONOMY

NUMERICAL TAXONOMY

In the last few years a unified approach to systematics has been developed, called *numerical taxonomy*. This attempts to evaluate numerically the affinity (or overall similarity) between taxonomic units, and to order the units into taxa on the basis of their affinities (Sneath & Sokal, 1962). It is a direct development from the ideas of Adanson on natural classifications, though often by people who were not aware of his work. A concise description is given by Sneath & Sokal (1962) and fuller treatment by Sokal & Sneath (1963). The procedure in the four logical steps given above is as follows:

(1) The organisms for study are chosen. *These are the operational taxonomic units,* or OTU's. A large number of their characters are recorded, taken from a wide range of different parts, organs and life-stages. These em-

brace, in addition to morphological characters, chemical, physiological, behavioral and other characters. The aim is to obtain a representative sample of the whole phenotype, and no distinction is made between the kind of character used, provided it is considered relevant to the construction of a taxonomy. The relations are therefore *phenetic* (Cain & Harrison, 1960), and not necessarily phylogenetic. Certain problems arise here: what characters are pertinent to taxonomy, should they be only those that faithfully reflect the genotype, and what is "the same" character in two organisms (the problem of homology in the wide sense)? These have been discussed at length elsewhere (Sokal & Sneath, 1963). Though of basic importance to any classification, they are not specially relevant to the ideas of Adanson, and are not discussed further here. The characters and OTU's must be sufficiently numerous to give statistically significant results, but we believe it is not necessary to know all of them, which answers one criticism of Adanson (see "The criticisms of his contemporaries" p. 491).

(2) The characters are now coded in a form suitable for numerical analysis, and each organism is compared in turn with every other with respect to these characters. This gives a table of overall similarities, based on all the characters used. For example, we might have two lilies showing a high figure for their mutual similarity, while a rose would show low similarity to either. A number of different statistics can be successfully used here, with relatively little difference in the end result. The simplest to understand is to define overall similarity as the *proportion of agreements* between the two organisms over the characters being studied.

(3) Using the table of similarities between the pairs of OTU's, the organisms are now sorted into groups by cluster analysis. As with the last step, several methods are available, giving much the same results with most data. These groups, or *phenons,* are taken as the taxonomic groups, and they can, if wished, be equated with the ordinary categories of rank, such as genus, family, order. However, such identifications are at present arbitrary, and there is as yet no general and objective method for deciding what is generic, familial, or ordinal rank. Nevertheless, if the ranks are employed simply as convenient summaries of the relative relationships, this does not greatly matter. And, of course, within any one study they may be standardized. In any event the relations between the phenons are not changed by naming them in this way.

(4) Having formed the taxonomic groups, the characters can be re-examined to find out which are most constant within the taxa thus formed.

These are suitable for use in keys for identification. It should be emphasized here that this step is logically subsequent to group-formation: we cannot say what best discriminates the groups before we have constructed them. It should also be noted, since this has given rise to many misunderstandings, that value in identification is deduced *a posteriori*, and this is an entirely different matter from the *a priori* value of characters in creating the groups in the first place.

THE RELATION OF NATURAL CLASSIFICATION TO ADANSONIAN PRINCIPLES

Taxonomy before Adanson was wholly dominated by the logic of Aristotle (384-322 B.C.), well described by Cain (1958). In this system the definition of a class implied a number of other properties that necessarily followed from it. The class of triangles, for example, was defined as the class of plane figures with three straight sides; as a consequence all triangles showed certain properties, *e.g.*, the sum of the internal angles is 180°. The class of triangles can then be subdivided (*e.g.*, into isosceles and scalene), and each subclass again possesses certain necessary properties as a consequence of the definition. This system is the *system of analysed entities*. It lends itself excellently to the hierarchic method known as the "Tree of Porphyry" (c.232-302 A.D.). But, as Cain points out, it is not applicable to biological classification, which requires the *system of unanalysed entities*, a problem never seriously attempted by the early philosophers. It is in practice impossible to define a cow, for example, in such a way that many necessary consequences follow from this definition without any exception.

We must now enquire more closely into the matter of natural classifications. It is evident that there are many exceptions to the alleged rules for setting up taxa. Why do we make these exceptions? If mammals are animals that secrete milk, why do we include "atypical" specimens that have no mammary glands, but exclude pigeons, which secrete "milk" from their crops? How do we define milk and mammary glands? Clearly they are not defined by their occurrence in mammals—this is a circular argument. It is evident that we ignore milk secretion *whenever it is not associated with the other characters of mammals*. Hence it is the *character associations* that form the true basis for the taxa. The taxa which we think of instinctively as "natural" are therefore based on the associations between characters. Such associations do not depend on *a priori* weighting. This is the essence of what Adanson taught, and it is considerations of this kind which have led numerical taxonomies to be called Adansonian.

Adansonian classifications are also "natural" classifications in the sense of Gilmour, as mentioned later. It is my belief that Gilmour's usage of the term "natural" does correspond to what we feel intuitively to be natural, and that "naturalness" is due to these Adansonian principles. The common definition of natural classifications is that they are based on phylogeny (or on idealistic typology). I believe this is incorrect, due to faulty attempts to rationalize our intuitive concept of "naturalness."

A further point relates to the principle by which groups are set up (see Simpson, 1961, p. 42; Sokal & Sneath, 1963). Taxa established on essential or invariant characters are called *monothetic,* and are not necessarily "natural" ones. If, however, we base the taxa on the most numerous correlations between characters, we find that the resultant groups, although "natural," do not necessarily show any character common to all members of the group. In other words, there may not be any "diagnostic characters." Such groups are called *polythetic.* Adansonian methods lead to polythetic taxonomies, in contradistinction to Aristotelian logic and also to the strict theory of the "subordination of parts" (see under "Equal weighting" pp. 478-481). We cannot insist on *a priori* grounds that any character is essential to membership in a natural taxonomic group.

Adanson (1757, *Coquillages,* p. xx) did not believe in *a priori* essential characters. He says:

Je n'assigne point de caractère particulier à chaque genre que j'établis, parceque ces caractères particuliers qui sont arbitraires varient quelquefois et deviennent souvent faux ou équivoques lorsqu'on vient à trouver de nouvelles espèces: j'y supplée par une exacte et entière description; elle tient lieu des meilleures caractères, puisqu'elle les rassemble tous, ceux qui sont arbitraires aussi-bien que ceux qui sont réels.

Reads, in free translation: Emphatically, I do not give any diagnostic character to the genera that I establish, because those diagnostic characters that are arbitrary vary a good deal and often become false or equivocal when new species are discovered. Instead I give an exact and full description; this takes the place of the better [diagnostic] characters, because it brings together all the characters, both those that are arbitrary and those that are real [natural or essential].

In *Familles des plantes* (1763, Vol. 1, Préface, p. xciv) we have a statement of the difference between what we now recognize as monothetic groups (which Adanson called *systems*) and polythetic groups (which he called *methods*). He did not define the latter except in negative terms, but it is clear that the polythetic concept was what he wished to express. He says (in free translation) . . .

. . . A method is any arrangement of objects or facts brought together by any congruities or resemblances, in such a way that one can express the method by a general idea applicable to all these objects, without meanwhile regarding this fundamental idea or basis *[principe]* as absolute, neither invariable nor so general that it could never show an exception. Thus, a method differs from a system only in the idea that the author attaches to the bases [for the groups], regarding them as variable in a method but as absolute in a system.

He then distinguishes natural from artificial methods, but only defines them tautologically as giving natural or unnatural groupings.

EQUAL WEIGHTING, OR *A PRIORI* VALUE OF CHARACTERS IN CONSTRUCTING TAXA

Adanson did not believe that one could decide *a priori* on the importance of different organs in determining natural relationships. Natural classifications are general classifications (Gilmour, 1937; note that Adanson uses "general" in another sense). They are based on the correlation between characters, and the natural groups should express the greatest number of relevant statements that can be made about their members. The groups, therefore, have a high "content of information." These points are quite distinct from their importance in other ways, and Gilmour made a valuable contribution by pointing this out.

If we cannot decide *a priori* on the weight of different characters for creating taxonomic groupings, this means that in practice they must be given equal weight. The difficulty here lies in finding any logical grounds for allocating greater weight to one character than another. We clearly cannot employ their constancy in the groups (as with features for identification), for the simple reason that *we do not yet know what these groups are.* This removes almost all our criteria. No argument involving the taxa is admissible. 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 in question.*

Other bases for weighting would be the importance of characters in phylogeny, or their importance to us in our own branch of biology. Yet the so-called facts of phylogeny are largely hypotheses based on classifications. They are again inadmissible as we do not have the classifications yet. Whether we are interested more in morphology or chemistry is irrelevant to the question of natural taxa; we can construct in this way morphological groups or chemical groups, but these are, by definition, not general classifications. No argument about the survival value of characters can assist us: survival value is constantly changing, as it depends on the ever-changing environment. Lack of fur would be lethal to the polar bear, but it is not to whales. Also, we are not asking for a classification based on survival. Lastly, how shall we give numerical values to weights? Is the presence of fur, five or ten, or a hundred, times more important than the presence of teeth?—in bears, in whales, in sloths?

The reason that Adanson's contemporaries could not accept his thesis is interesting. In essence it was that they employed his philosophy without admitting it; that is, they abandoned their own *a priori* principles wherever they clashed with the classifications which they apprehended intuitively. From these intuitive classifications they selected diagnostic characters and attributed great weight to these, rationalizing this on *a priori* principles. One of the special abilities of the human mind is the ability to make a swift and rough estimate of overall similarity from visible morphological detail: since it strains credulity that when considering numerous characters a logical weighting process is involved to any marked degree, it seems reasonable to assume that the intuitive process is predominantly "equally-weighted." His contemporaries were also, no doubt, confused (as probably Adanson was himself) by the fact that different characters may be of very different complexity. Many characters are character-complexes. In modern terms we would say that it is each piece of new information that has equal, or unit, weight, and therefore that complex characters are to be broken down into unit characters. These are the smallest logical subdivisions of characters, with certain provisos (see Sokal & Sneath, 1963). This breakdown into unit characters preserves the aggregate "weight" of complex characters, and the statistical methods of numerical taxonomy greatly even-up the weighting in practice.

Where Adanson, however, was right, was in rejecting the proposition that one could *a priori* decide what weights could be given (at least on the basis of any of the principles used by his contemporaries; see Adanson, 1847, p. 118), and in the implication that the character relations should be counted. The opposite view, briefly called the *law of subordination of parts,* is better referred to as the "*a priori* hierarchy of characters."

The hierarchy of characters is principally due to Antoine-Laurent de Jussieu (1748-1836) in botany (probably also, in some measure, to Bernard de Jussieu (1699-1777), and Stearn, 1961, p. xcii, notes that Tournefort had similar views), and in zoology to Geoffroy de Sainte-Hilaire (1772-1844) and Georges Cuvier (1769-1832). Very probably they felt in an intuitive fashion that a few characters, if they were the right ones, were sufficient for classification. In a certain sense this idea, again in the Descartes-Newton tradition, is correct, in that they would allow discrimination between taxa.

The problem is to find the right characters, and they claimed to have explicit rules for this. Cain, in his "Deductive and inductive methods..." (1959), has ably discussed this at some length; briefly their argument (though with disconcerting variation in details) was as follows:

(1) The characters of the most important organ systems—reproduction, respiration, circulation—should establish the highest taxonomic ranks.
(2) When this rule raised difficulties, the more constant characters were to be taken as more important.

The two parts of the rule are often mixed together in a curiously haphazard manner, which of itself shows that no explicit method had been evolved. For example, in the "Extrait des Registres," p. 7 in the introduction to A.-L. de Jussieu's *Genera plantarum* of 1789 (see also pp. xxxvii-xlvii), the primary characters are described as essential, always constant, uniform in all the orders, and drawn from the essential organs. The secondary characters are general, nearly uniform in all the orders with few exceptions, and taken from non-essential organs. The tertiary characters are sometimes uniform, sometimes variable, taken from organs sometimes essential, sometimes not essential. Since it is clear that we cannot tell their constancy at this stage of the classificatory process, the above rules are quite unworkable.

The earliest explicit description of the subordination of parts (A.-L. de Jussieu, 1773) is later than Adanson's books. Jean-Baptiste de Monet Lamarck (1744-1829) held very similar views, as Cain (1959, first entry) mentions. In the hands of Cuvier the *a priori* method was carried to absurd lengths. Overcome by the idealistic vision of animals as harmonious wholes, whose every part must fit in form and function, he was over-simplifying biology, as an astronomer might simplify stellar physics to a few basic factors. Cuvier did not realize how diverse were the ways in which parts could, and do, fit in living creatures. A single fossil bone was claimed to tell the enquirer most of the salient features of the rest of the animal. Alas, only too soon exceptions arose. The chalicotheres, with the teeth of herbivores and the feet of carnivores, are a well-known example. Simpson (1945, p. 256) comments; ". . . Only when remains were tardily found in unequivocal association could anyone believe that Cuvier's so-called law of correlation could be so utterly wrong in a particular case and that such an anomalous creature as a clawed ungulate could exist." He also says (Simpson, 1961, p. 44) that the belief that paleontologists can reconstruct a whole skeleton from a single bone is one that all paleontologists know, to their sorrow, to be untrue. We can now see that Cuvier's genius lay in his phenomenal memory and grasp of associations. Where a bone could be plausibly related to a previously known animal of close affinity, whether fossil or living (a relationship based, incidentally, on morphological similarity), he could predict, with surprising accuracy, what the missing portions would show. When, however, it was not closely related to a known form, this was much less certain even in outline, and it was impossible to predict details.

The Hierarchy of Characters is another form of unequal weighting. It is also foreign to modern evolutionary studies, since Sokal & Sneath (1963) point out that evolution could scarcely work in such a way that classes were distinguished only by certain kinds of characters, orders only by others, families only by others. It would also yield monothetic taxa, if strictly applied.

The method was probably used very seldom in its rigorous form. When the law of subordination of characters fitted the intuitive scheme, it was mentioned. When it did not, it was not referred to, and the exceptions were ignored. Exceptions are, of course, very common. If the exceptions are as numerous as the rule, it seems tendentious to call it a law. Biologists are silent on how one distinguishes in a given case the exceptions, unless by appealing to the principle of constancy within groups, which we have seen is inadmissible. As Cain (1962) says, the groups have to be first formed by intuitive methods, and these approximate to Adansonian ones.

THE USE OF ALL PARTS OF A PLANT IN SYSTEMATIC WORK

Another aspect of Adanson's work, now generally accepted, was his insistence that all parts of an organism should be studied and described before a classification was made. Speaking of molluscs, he says that the preoccupation of naturalists with beautiful shapes and colours of shells has drawn their attention away from the animals that make them (1757, *Coquillages,* pp. iv-vi). He was, indeed, the first conchologist to pay more than passing attention to the inhabitants! He employed a wide range of characters of the animals (but did not explore their internal structure: according to Hopwood, 1950, he was partly anticipated by Daubenton in 1743 and Gueltard in 1756). Similar arguments are found in his *Familles des plantes* (*e.g.*, Vol. 1, pp. xcv-ci).

Most systematists of his day appear to have felt that they could get along well enough by looking at a few parts of the organism. Linnaeus himself was less sanguine, but found himself compelled (by the enormous amount of new material then being discovered) to restrict his attention to rather few points to keep abreast of the flood. Augustin-Pyramus De Candolle (1778-1841) gave much attention to comparative morphology, as his book, *Théorie élémentaire de la botanique* (1813) shows. But many systematists must have been superficial in their studies, and Adanson's influence was probably quite important in correcting this.

Adanson does not seem to have had any clear awareness of the problem of non-specificity; that is, whether an acceptable and detailed classification based on one part of a group of organisms (or one stage of the life cycle) is in agreement with another such classification based on another part. But this is not surprising, as this problem is only just being examined (see Sokal & Sneath, 1963).

III THE NATURAL METHOD

SALIENT FEATURES OF LA MÉTHODE NATURELLE

What were the main features of Adanson's method? One cannot do better than quote from Cuvier's *Éloge* (Cuvier, 1807, pp. 13-14). He says that the most direct route would be to determine the value of each character and base the higher divisions on the most important. However, little was known of this in Adanson's day. Cuvier goes on (in free translation):

> He therefore had recourse to an inverse method, which may be called empirical, or based on experience: the effective comparison of species. For this he thought of an original procedure, which one must admit is very ingenious. Considering each organ by itself, he made a system of division based on its different modifications. He arranged in this system all the organisms known. Repeating the process for each of a great many organs, he constructed a number of such systems, all of them artificial and each based on a single organ chosen arbitrarily.
>
> It is evident that entities which are classed together in every one of these systems are exceedingly close to one another, since they resemble each other in all their organs. The relationship is less when the entities are placed in different classes by some of the systems. And finally, the most distant are those entities which are not grouped into the same class by any of these systems.
>
> This method thus gives a precise estimate of the degree of affinity between the organisms, independent of the state of knowledge on the function and importance of the different organs. But it has the defect of supposing prior knowledge of another kind—which in fact is no less difficult to acquire and understand—knowledge of all species and of all the organs of each. Neglect of only one of these could yield false relations, and Monsieur Adanson himself, despite the immense number of his observations, has furnished several examples of this.
>
> It is this which he called his *Universal Method*, and it is also the idea that dominates all his great works, published or in manuscript.

We see from this description that Adanson's Universal or Natural Method was remarkably similar to the first few steps of numerical taxonomy. He first chose the organisms and recorded many characters of each. He then prepared his arbitrary systems of division, and then apparently counted the number of times that a pair of entities fell together in the subdivisions. In effect this procedure counts the number of disagreements in the characters used to make the divisions, and if carried out systematically would have yielded a table of the comparisons between each organism and every other, which would have been, in effect, a similarity matrix. Whether Adanson ever proceeded in this systematic way is very doubtful: the number of pairwise comparisons between the 1615 genera in the *Familles des plantes* total over a million. It is more likely that he counted the disagreements for some of the comparisons only, but did enough to obtain a fair idea of the salient relations between the organisms.

Since the different characters used were simply counted, they were used without deliberate *a priori* weighting, as his contemporaries soon realized. The characters were not, however, explicitly broken down into unit characters, and therefore they did not make the best use of the data; nor were they precisely of equal weight in the modern sense. Effectively, however, it was an equal-weight method.

A further point, though by no means obvious at first sight, is that such a process would create polythetic groups, since it would be possible for clusters of organisms to be similar without all of them necessarily sharing any particular character, or having any distinctive single attribute. This marks off his views from those of A.-L. de Jussieu and Cuvier, whose theory of subordination of parts implies that certain important characters are necessarily constant in taxa, which are then monothetic—though this is a rule more properly honoured in the breach than in the observance, and in practice this consequence was evaded by ignoring the rule.

We therefore can see the salient features of Adanson's method; it employed: (1) many organisms, many characters from all parts of the organism (*i.e.*, the use of as much information as possible); (2) equal weighting of characters; and (3) overall similarity and polythetic divisions.

Adanson did not give precise directions for clustering the organisms into taxonomic groups, and this is not unexpected, since in those days there were few ideas in cluster analysis more advanced than the logical divisions of Aristotle, or the "Tree of Porphyry." This monothetic method must have occasioned much confusion when applied to a polythetic system which was only dimly understood. Nevertheless, he does say that one proceeds in a stepwise fashion, first making small families of very similar organisms, and then combining these families into groups of higher rank (1757, *Coquillages*, p. xi). This is "classification from below" in the synthetic, empirical and polythetic tradition (see Sneath, 1962) rather than the artificial monothetic method of "classification from above."

THE ORIGIN OF LA MÉTHODE NATURELLE

Our author is the first philosopher, who adventured to visit the torrid zone, for the propagation of knowledge; and who, in search of this valuable treasure, may be truly said, to have encountered more monsters, than those ancient heroes, represented in fabulous story to have gone in pursuit of the golden fleece (Pinkerton, 1808-1814, p. 598).

The multifarious and exuberant life of the tropics had an exhilarating effect on many naturalists when they first visited tropical countries. We may think of Charles Darwin (1809-1882) and the way in which his voyage on the *Beagle* set off the train of thoughts that later culminated in the theory of natural selection. Similarly, Alfred Russel Wallace (1823-1913) in the East Indies, while incapacitated by illness, independently hit upon the same theory. Like these, Michel Adanson had his great idea in the tropics, in Senegal. He tells us (in Adanson, & Payer, 1845, Vol. 1, p. 14) that there, as a young man without the counsel of others, and with only the works of Tournefort and Linnaeus (which seemed absolutely useless for the strange creatures he found), he was thrown on direct observation of nature; here he found the germ of his ideas.

Evidently he had thought out most of the details before leaving Senegal, for he says (1847, pp. 35-36) that he wrote to Bernard de Jussieu in 1750 giving a summary and two examples of his method. He points out (1757, *Coquillages*, pp. v-x) that no attention had been given to the animals that produce the shapely and gaily-coloured shells, that the shape of shell varies with age, that previous attempts at classification were superficial and confusing. He goes on (p. x):

C'est d'après cet examen et ces réflexions, que j'ai cru devoir travailler cet ouvrage sur un plan tout différent de celui qu'on suivi les anciens et les modernes. J'ai déjà dit que leurs méthodes, bien loin de donner aucune lumière sur la connoissance des Coquillages, tendoient au contraire à nous écarter de la vraie route qu'il faut suivre pour l'acquerir; et l'on verra par l'exposé que je vais faire de mon plan, que je ne dois rien aux uns et aux autres, puisque je n'ai pas emprunté la moindre de leurs idées.

Reads, in free translation: It was after these thoughts that I considered I should base this work on a plan that is entirely different from those of both early and modern writers. I have pointed out that their methods, far from shedding light on the knowledge of the molluscs, are more likely to obscure the true way to knowledge.

And it will be seen from the outline of my plan that I owe nothing to early or modern writers, because I have not used even the smallest part of their ideas.

In the *Familles des plantes* (1763, Vol. 1, Préface, p. c) he gives the main faults of artificial systems, as follows: they are founded on only a few characters, they do not agree with each other, and they misplace creatures that are obvious exceptions. We can see that by considering more characters he was able to reduce the number of such exceptions, and the disagreement between artificial systems becomes unimportant in a method that deliberately exploits such disagreements.

The forerunners of Adanson, besides Bernard de Jussieu (discussed below), were Cesalpino (1519-1603), Joachim Jung (1587-1657), John Ray (1627-1705), Pierre Magnol (1639-1715), Caspar Bauhin (1560-1624) and Linnaeus himself (1707-1778), all men of a quick intuition in taxonomic matters. It is difficult to understand why Joseph Pitton de Tournefort (1656-1708) is sometimes mentioned in this connection, as Adanson's theories were very different, though he himself considered Tournefort's taxa to be the best so far. Linnaeus recognized the need for a natural method, but admitted he was unable to discover it (see De Candolle, 1813, p. 60). Of these, the most noteworthy were probably Ray and Magnol. Ray first emphasized the importance of studying many parts of a plant, and had some idea of "naturalness" in taxonomy (see Sprague, 1950). Magnol was among the first to enunciate clearly the properties of a natural taxon, as Adanson (1763, Vol. 1, Préface, p. xxii) acknowledges. Magnol said: "There is a certain likeness and affinity in many plants which does not rest upon parts taken separately but in the total composition, which strikes the sense but which cannot be expressed in words" (translated from Magnol's *Prodromus Historiae generalis Plantarum* of 1689 and cited by Stearn, 1961, p. xci).

THE METHOD AS SEEN IN *Coquillages*

Adanson describes his method in 1757 as follows (Adanson, 1757, *Coquillages*, p. xj):

Je me contenterai de rapprocher les objets suivant le plus grand nombre de dégres de leurs rapports & le leurs ressemblances: les descriptions qui serviront à établir cette ressemblance, seront aussi les preuves les plus solides sur lesquelles seront appuyées les raisons que j'aurai eu de les rapprocher. Ces objets ainsi réunis, formeront plusieurs petites familles que je réunirai encore ensemble, afin d'en faire un tout dont les parties soiant unies & liées intimement.

In free translation reads: I will be content to bring together the objects according to the greatest number of their affinities and resemblances: the descriptions which serve to establish this resemblance will also be the most solid evidence on which to base the reasons that I have brought together. These objects brought together in this way will comprise many small families, which I will again bring together to make a whole, of which the parts are closely united and interconnected.

It was just two hundred years before this view was clearly appreciated (Sneath, 1957; Michener & Sokal, 1957).

He gives us a glimpse of his technique when he describes how the molluscs have been tabulated according to a number of different artificial systems (only eleven in number), so that one can compare them readily, but does not give the procedure for the comparison and rearrangement. This work also establishes the science of malacology, of which Adanson was the founder (Piette, 1942).

THE METHOD AS SEEN IN *FAMILLES DES PLANTES*

In *Familles des plantes* of 1763 the method has become more sophisticated in two ways. The first is that many more organs and characters are used; there were sixty-five systems, based on twenty-two organs, and no less than 1615 genera were considered in making the fifty-eight families. Secondly, taxonomic theory is discussed more thoroughly.

Adanson (Vol. 1, Préface, pp. xciii-xcv) distinguishes between *systems* based on a few invariant characters (*i.e.*, monothetic) from *methods* where characters are not necessarily invariant (*i.e.*, polythetic). Though all systems are perforce artificial, sometimes bringing together very different kinds of plant, yet methods can be artificial or natural. Artificial methods are easier to set up, as there are many alternatives which the author can choose between. In a natural group there are no characters that are essential. Nevertheless, the consideration of all parts should mean that the families should be stable, unlike artificial groups (*ibidem*, p. cxcii). In the 2nd edition (1847, p. 114), he adds a note that "a genus is an assemblage of species which resemble each other in the greatest number of their characters, not in all, nor in some kinds of character rather than others . . . but sometimes in certain kinds, sometimes in others, and in various number, depending on the genus and family in question."

The systematic categories, including species, exist only in our imaginations, since only individuals exist in nature, and it is no easy task even to define an individual (Préface, pp. clxiii-clxiv). Some gaps are due to our ignorance of forms that occupy them:

. . . il est aussi certain que plusiers de ces lignes de séparation qui sont les plus marquées, ont pour cause, soit l'ignorance ou nous somes des êtres intermediaires qui en font la liason, soit la perte même de ces Individus dans la succession des tems, & par les révolutions du globe terestre, come le témoignent les ossemans de monstreux Quadrupèdes, les skélètes ou impressions de Poissons & des Plantes, & un nombre prodijieux de Kokillages fossiles, si diférans de ceux qui vivent aujourd'hui dans les mers.

Reads, in free translation: It is also certain that in many of those lines of separation which are most marked, have, as their cause, either our ignorance of their intermediate forms which provide the connecting link, or else the extinction of such intermediate forms over periods of time and revolutions [catastrophies?] of the terrestrial globe—witness the bones of monstrous quadrupeds, the skeletons or impressions of fishes and plants, and a prodigious number of fossil shells, so different from those that live today in the seas.

Characters should be recorded fully, not, for example, omitting leaves in one plant, fruits in another (a common fault of botanical descriptions, he says). "Enfin ces Caracteres doivent toujours être comparatifs, & pris de la même partie, ou des mêmes parties dans toutes les plantes de la même Famille, ou qui se raprochent beaucoup . . ." (Préface, p. clxxi).

Adanson seems to have believed that plants formed a more or less continuous series, separated by gaps which defined the families, classes, etc. Adanson naturally had little idea of the multidimensional relationships of taxa, and (Vol. 1, Préface, pp. clxv, clxxxviii) speaks rather of a single dimension, with families and genera arrayed along it, though not evenly spaced. This bears some little affinity to the *scala naturae* of Charles Bonnet (1720-1793) and Lamarck (see Simpson, 1961, p. 60; Singer, 1959, p. 119), which contained the additional idea of perfection or progression from lower to higher forms and was believed to be quite continuous. The unevenness of the gaps suggested to Adanson that their size could indicate taxonomic ranks. He also believed that undiscovered families would fall in them, and perhaps this is what he means by saying that his method makes provision for all new plants. It does, of course, but because the relations are multidimensional with abundant space for new forms, and not simply for the reason Adanson gives. It was nevertheless a most perceptive comment.

However, he gives rather few instructions on the method itself (pp. cliv-clviii). He made complete descriptions of the plants, studying all the organs, and searched for the lines of separation between the natural groups. He again tells us that their affinity is obtained from comparison of all their characters. He noted down the differences of a new genus compared to related but well-known genera. The number of differences was an indication of the size of the gap between the organisms (p. clxv), and hence of taxonomic rank. Having made the sixty-five artificial systems he says (p. cclii): "... leur ensemble done tous les raports existans ou observés entre toutes les parties des plantes, raports d'où se sont formées nos 58 Familles." But he tells us little more about how to estimate the resemblances and make the natural groups. Cuvier (1807) explains in more detail (see "Salient features of la méthode naturelle" pp. 482-484), and De Candolle (1813, p. 70) gives a similar explanation: "Adanson pensa que les plantes qui se trouvaient les unes a côte des autres dans le plus grand nombre de ces systèmes, devaient être celles qui avaient entre'elles le plus de rapports, et qu'on devait le plus rapprocher dans l'ordre naturel."

In some respects Adanson's subsequent procedure is confusing. He placed at the head of each family the characters proper to it, and then in a few columns gave the characters that distinguished the genera of that family, though he apparently excluded some characters which experience had shown were unhelpful—a departure from the rigorous application of his theories. He then apparently revised the family, since he says some genera would need changing, while others had their place confirmed by their concordance on characters from other organs (pp. clxxxviii-cxci). He also underestimated the irregularity of nature, since he thought that the progression of genera was sufficiently continuous for two or three genera to embrace all the variations found in a family (p. cxciii).

Later in life Adanson seems to have lost interest in the methodology of classification. For example, in the posthumous *Cours d'histoire naturelle fait en 1772* (Adanson & Payer, 1845, p. 14), he refers to his classificatory method very briefly under the name of *caratère de l'ensemble,* but devotes much more space to the plan of his great work on all branches of knowledge, which he never completed.

DIFFICULTIES OF ADANSON'S METHOD

How feasible was Adanson's method in practice? Probably it was quite impossible to apply at the time, for reasons given in the next section, below. But it also contained a number of poorly-defined steps. Adanson gave little attention to the volume of data needed, despite his insistence on the use of all parts of a plant. We need at least fifty characters, preferably a hundred, to make worthwhile numerical taxonomic studies. He did not explain in any detail how one chose a character, except that it must afford separation of the plants under study. He did not reduce them to unit characters (hardly surprising, since no one had then considered how information could be measured). The problem of homology between characters was, of course, scarcely thought of then. In addition he does not give precise instructions for the construction of the most natural taxa from the many relationships between pairs of organisms. He did not apparently distinguish (as separate from a classification) the device of the artificial key for identification.

WHY IT WAS NOT SUCCESSFUL

Why was Adanson's method not successful? Stearn (1961, pp. xcv-xcvi) considers that the knowledge required for his procedure was then too limited, which is no doubt true. Two further points may be mentioned. Adanson did not in practice carry very far the hierarchy of ranks, and the lack of higher groupings probably militated against the use of his work. Also, the Adansonian method was far too laborious for general use before the advent of modern computing machines. Such machines were not even thought of in his day. It was Charles Babbage (1792-1871) who first conceived the idea of a data-processing machine, his famous "analytical engine" of 1833. This was not to be a simple calculator like his "differential engine," or similar machines of that period, since it was intended to store data, process it, and store the results, according to a program of pre-set instructions. It was to work upon the principle of the Jacquard lace-making machines, that used perforated cards. It is clear from Victorian lacework and weaving that such machines did store and process large amounts of data. It is no wonder the Victorians were so enthusiastic about the abilities of such machinery that they introduced florid ornamentation wherever they could. Babbage's analytical engine was abandoned—one of the greatest missed opportunities in science—because it was technically too difficult to make. Not until the Harvard Sequence Controlled Calculator in 1944 and the Eniac computer was his idea brought to fruition (Hammersley, 1950).

ADANSON'S FAMILIES

It is not within my competence to discuss the botanical value of the families described by Adanson. The foregoing paper by Stafleu does this admirably (*Adanson,* pt. I, pp. 123-264). Both Cuvier (1807, p. 18) and De Candolle (1813, pp. 71-72) admitted that Adanson's families of angiosperms were for the most part natural groups, and an improvement on previous taxonomy. But Stearn (1961, pp. xciv-xcv) has pointed out that this superiority was not very marked. Some of his families have since been split up, and we do not have a suitable standard for comparison (indeed we require here a numerical taxonomic study), so that evaluation is not very easy. Adanson's treatment of the pteridophytes is less satisfactory (Pichi-Sermolli, 1959). His belief in a *scala naturae* also caused him to misplace some genera in an effort to place them in a linear sequence.

ADANSON AND BERNARD DE JUSSIEU

The relation of Adanson's work to the unpublished work of Bernard de Jussieu (whom he regarded with great respect) has occasioned some doubt. The latter published little, and nowhere gave explicit rules about how to set about making a classification. Cuvier (1807, p. 17) points out that Adanson very probably got the ideas of the unity of certain families from the shrewd, intuitive classifications of Bernard de Jussieu, yet the principles of classification were Adanson's own. In fact it is clear that Antoine-Laurent de Jussieu, in fathering on Bernard de Jussieu the principal outlines of the classification used in his *Genera plantarum* of 1789, was at pains to distinguish between the classification itself and the principles of classification employed. Since A.-L. de Jussieu rejected Adanson's classificatory philosophy, he clearly would not claim this philosophy as the invention of his uncle and himself. In fact the classification used by the de Jussieus (though Adanson was much annoyed when his own classification was attributed to Bernard de Jussieu; see Chevalier, 1934, pp. 110-116) is beside the point. Adanson is the first to tell us how to classify. He was the expositor of the logic behind the intuition of men like the de Jussieus.

THE RELATION OF ADANSON'S IDEAS TO OTHERS OF HIS CONTEMPORARIES

An interesting point is Adanson's connection with the comparative morphology of Felix Vicq-d'Azyr (1748-1794). The latter expresses views that are clearly derived from Adanson, though without referring to him. For example, he says (Vicq-d'Azyr, 1792, p. xxv, in free translation): "A natural class results from the assemblage of a certain number of species which share a greater number of agreements than occur between any one species and the species of other classes. . . . In consequence a class can be very natural without there being a single character common to all the species composing it." Nothing is known of Adanson's influence on Vicq-d'Azyr (Hoeffer, 1855-1864).

ADANSON'S DISCIPLES

Adanson had few disciples. We have a comment by De Candolle (1813, p. 72) that his method served as the basis for the works of Buttner and Ruling. Vicq-d'Azyr is mentioned above. Whewell (1840, Vol. 1, pp. 449-523) took up some of Adanson's ideas in his interesting work on the philosophy of science. We owe to Henri Baillon (1827-1895) the first sympathetic recognition of Adanson (Stafleu, p. 245, 1963).

THE CRITICISMS OF HIS CONTEMPORARIES

Adanson's method of classification was criticised by Cuvier (1807, p. 14) on the following grounds. (1) It presupposed knowledge of all species and of all their structures. We now believe that we only need samples of both, provided they are reasonably large. (2) It may be misled by ignorance of important organs. However, we do not know what this importance is. Cuvier mentions some mistakes in Adanson's work on molluscs, due to ignorance of internal anatomy. He adds that the greater number of organs considered in the *Familles des plantes,* and the obscurity of their function, gives more justification to the empirical method in that work. He notes that of earlier botanists, the only one who had not given up the search for a natural system, and the only one who could even be considered as his master, was Bernard de Jussieu. Apparently Cuvier had a fairly open mind toward weighting, as he does not criticise this strongly, but rather as a less efficient process in taxonomy than the subordination of parts. He may have realized that they gave much the same result in practice, at least in those days.

The views of Augustin-Pyramus De Candolle are summed up in his influential textbook *Théorie élémentaire de la botanique* (1813, pp. 26-28, 70-72). Admirable discussions of this, and of Cuvier's views, are given by Cain (1959, both entries; 1962). Briefly, De Candolle points out that classifications can be *artificial* (based on some few obvious characters) or *natural* (ones that reflect the order of nature, though just what this consists of is obscure). The procedure of finding natural groupings can either be by *blind groping* (tâtonnement), as by Magnol, or by general comparison (Adanson's method), or else by the *subordination of parts,* founded by the de Jussieus and developed by Cuvier in zoology. As to Adanson's method, he says (p. 71):

> Cette idée est en effet séduisante au premier coup d'oeil par son exactitude apparante, mais elle ne peut soutenir un examen approfondi; en effet, 1° elle suppose que nous connaissons non seulement tous les organes des plantes, mais encore tous les points de vue sous lesquels il est possible de les considérer . . . en second lieu . . . l'idée fondamentale n'eu serait pas moins vicieuse en ceci, qu'elle suppose à tous les organes une égale importance . . .

Reads, in free translation: This idea is attractive at first glance because of its apparent exactitude, but it cannot survive deeper examination. In effect, first it presupposes that we know not only all the organs of plants, but also all the ways in which we could consider them. Second, the fundamental idea is no less viscious—that all organs are given equal importance.

He goes on by saying that some organs are more important for survival, and have a greater influence on the total organization, and that *consequently* they should be more important for classification (italics mine). He does not discuss the objections that a classification by *survival value* is not necessarily equivalent to a natural taxonomy, nor the obvious point that a character that influences many other characters will of necessity produce character correlations of the kind used by Adanson. Hence Adanson's method will, in this regard, give the same groupings as the method of subordination of parts. He concludes with the fallacious argument that the more constant characters are more important, when it is clear that at this stage we cannot tell which these are, since we have not yet formed any groupings.

VIEWS OF HISTORIANS ON ADANSON

Adanson has not attracted the merit he deserves from historians. Charles Singer (1959) and Pledge (1939) do not mention him. Julius von Sachs (1890, p. 116) completely misunderstood Adanson's work. After commenting, correctly, that his claim of priority over Bernard de Jussieu (with respect to the actual classes of plants) is unimportant, he says, incorrectly: "The natural system was not advanced by Adanson to any noticeable extent." Sachs continues: "How little he saw into its real nature and into the true method of research in this department of botany is sufficiently shown by the fact that he formed no less than sixty-five artificial systems founded on single marks, supposing that natural affinities would come out of themselves as an ultimate product—an effort all the more superfluous because a consideration of the systems proposed since Cesalpino's time would have been enough to show the uselessness of such a proceeding." Sachs overlooks the fact that it was precisely such a consideration which led to Adanson's method, from which natural affinities do come out. John Lindley (1799-1865), who first introduced "The Natural System" into Britain, does not mention Adanson in the Introduction to *The vegetable kingdom* (1846).

The natural method is most commonly attributed to the de Jussieus. It is true that they first achieved, intuitively, an adequate natural classification of higher plants. However, Stearn (1961, p. xci) comments: "... it was superiority of execution based on intimate knowledge of a wide range of plants rather than originality of basic concepts and design which gave Jussieu's *Genera plantarum* its great influence on the adoption of natural systems of classification." The basic concepts, rather, are due principally to Magnol and Adanson, and were used by others even when they denied their validity. George Gaylord Simpson (1961, pp. 41-46) gives Adanson credit for being a founder of empiricism in taxonomy, and says that most of his theoretical work was sound.

IV EVALUATION AND CONCLUSION

ADANSON'S CHARACTER

A good deal has been written about Adanson's character which need not be repeated here. He seems to have been a curious mixture of the kindly and the irascible. His outspoken criticism of others and his idiosyncracies of spelling did not help his cause. While undeniably a man of genius, and open to varied and novel ideas in his youth, he later became obsessed with the plan of his projected great encyclopaedia of all knowledge. The manuscript of this enormous work was never completed or published. He was not able to accede to the request of his colleagues to condense it and separate his own contributions from the mass of compilation from the works of others, and did in fact become out of touch with more recent work (Cuvier, 1807, pp. 20-21). It seems highly likely that he was temperamentally a man of the Descartian-Newtonian tradition, who cherished the hope that all knowledge could be explained and summarized in a few grand but simple laws, like Newtonian physics (*e.g.*, see his admiring comments in Adanson & Payer, 1845, p. 13, but see also his awareness of the danger, 1763, Vol. 1, Préface, p. cliv). Being also by temperament a "man of the cabinet," and fascinated by the diversity of nature, he may have believed that the accumulation of facts was both a necessary and a sufficient means to success. In the first he was correct—witness the wide reading of Darwin before he formulated his theory of evolution—but Adanson underestimated the difficulties of reducing biological data to simple principles. Though superficially one of the "encyclopedistes," he did not view his activities as a means for spreading social reform in the manner of Diderot (1713-1784) and d'Alembert (1717-1783). Adanson apparently did not share in the German school of idealistic typology as taught by the Naturphilosophen.

NOMENCLATURE

Adanson's system of nomenclature had little effect on taxonomy. He proposed two forms of mononomial nomenclature (1763, Vol. 1, Préface, pp. clxxxi-clxxxiii). His use of vernacular names was not compatible with Linnaean nomenclature. His rejection of Linnaean nomenclature was as displeasing to his contemporaries as his attempt to make French spelling completely phonetic, and both have directed attention away from his more solid achievements.

ADANSON'S VIEWS ON SPECIES AND EVOLUTION

Chevalier (1933, both entries) has noted that Adanson had views far in advance of his day upon the possibilities of transmutation of species. He also noted what we would now call microspecies. The attempt to attribute the germ of Lamarckism to Adanson seems to me less felicitous. However, his definition of a species in the sense of a distinct kind of organism (given in an annotation in Adanson's copy of Diderot and d'Alembert's *Encyclopédie, fide* Stafleu, 1963, p. 186) could hardly be bettered. He also first used the term "mutation," though not in a wholly modern sense (*see* Nicolas, 1963, p. 54).

Although before their time, Adanson's ideas were not very different from philosophic speculations by other biologists of the period. They are nevertheless significant in showing how Adanson was prepared to consider unlikely possibilities—surely one of the marks of an original mind.

ADANSON'S CONTRIBUTION TO SYSTEMATICS

What may we say, therefore, were Adanson's greatest contributions to systematics? They did not lie in nomenclature, nor evolution theory, nor principally in his classifications of molluscs and vascular plants. They were, rather, in the theory of classification, that is, taxonomy in the restricted sense. When he wrote, Linnaeus was at the height of his reputation, and so was the admittedly artificial system he had introduced. Adanson made the first and most important steps away from this artificiality, and in this he was much too far ahead of his day for his views to be appreciated. He was also one of the first to value the history of ideas in botany. His central idea could perhaps be described as a realization of the importance of the correlations between facts.

Adanson made the following important advances in taxonomy. His was the first attempt to define the complex quality of overall similarity, and to explain the polythetic method. He introduced the idea of the general or unweighted classification. He was among the first to insist that all parts of a plant should be used in classification, and to practice this. He showed how discrepancies between artificial systems could be resolved in a general classification. Lastly, Adanson was the earliest author to write at length about the classificatory process and to offer a set of explicit and partly workable rules.

We celebrate the bicentenary of his attempt at a natural classification of plants. A man of genius, born before his time, it is also fitting that we celebrate his most valuable theoretical work, volume one of his *Familles des plantes* published in March 1764, following publication in 1763 of the second volume of the *Familles des plantes.*

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