COVERT FRAGMENTA AND THE ORIGINS OF THE BOTANICAL FAMILY

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The following is an attempt to elucidate a problem which folk understanding of the living world posed for the classical naturalists of seventeenth- and eighteenth-century Europe. Family-level botanical groupings are recognised in folk societies, though seldom named. Unlike the named folkbotanical groupings of generics and life-forms, these covert fragments do not appear to constitute a properly taxonomic series, that is, a mutually exclusive, exhaustive and relational partitioning of the local flora. As European naturalists began to transcend the bounds of the local environment, however, groupings based on a holistic appreciation of local flora became increasingly untenable. By the eighteenth century attention shifted to the family as a context-free basis for organising flora world-wide in a single taxonomic system. Henceforth, the problem of natural history became one of finding an exhaustive series of families, on the intuitive model of the local covert fragmenta, which could cover any and all local environments. This effort came to be known as the Natural Method; its failure played a significant part in the birth of biology. The analysis of this effort and its failure is meant to illuminate the conceptual link between our ordinary, everyday understanding of the phenomenal world and scientific understanding. My aim thereby is to suggest that an ethnobiological appreciation of the scope and limits of common sense is indispensable to a properly scientific awareness of living nature.

Common-sense nature

Ever since Buffon, or more justly ever since Sachs, there has been a mistaken tendency among historians of biology to treat the development of natural history in terms of a tendentious dichotomy—a dichotomy which has, unfortunately, transmigrated into ethnobiological discourse. There is, on the one hand, the 'artificial' approach of the 'system' which presumably imposes a deductively rigid, hierarchical order on basic, empirical groupings of organisms. On the other hand, there is the 'natural' approach of the 'method' which allegedly attempts inductively to ascertain higher-order clusters of basic-level groupings by looking 'to nature herself' rather than obeying the arbitrary dictates of our 'feeble imagination' (Buffon 1749: 1–37). As Sachs puts it, 'These two elements of systematic investigation were entirely incommensurable; it was not possible by the use of arbitrary principles of classification which satisfied the understanding to do justice at the same time to the instinctive feeling for natural affinity which could not be argued away' (1890: 7).

Commentators agree that both approaches presupposed a thoroughly empirical basis which consisted of an exhaustive partitioning of nature in terms of genera and/or species. The difference supposedly consisted in the manner of articulating higher-order groupings. According to Linnaeus, for example, this could only be done in accordance with rationalist principles, that is, by the

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reduction of habitual structures to characteristic formulae. This required placing each and every family within a mathematically satisfying sequence of essential features. Such a procedure was only conceivable with reference to the characters of fructification, argued Linnaeus; for the intuitively-given families of the lay naturalist were usually associated with distinct fructifications, and most fructifications could adequately be represented as a mathematical combination of their readily identifiable parts (for example, the number, magnitude, geometrical form and topological disposition of the stamens and carpels). Thus, on the basis of the rationalist principle of sufficient reason, Linnaeus believed that there must be a naturally unique index of fructification features which could be computed for all families, known and as yet unknown.

Implicit in this approach is the Cartesian credo that if God has undeceivingly allowed us to see a part of nature's plan, then he has given us reason by which to anticipate the whole. Accordingly, Linnaeus explicitly adopted Leibniz's axiom that 'Nature makes no leaps', thereby ensuring, *a priori*, that families still to be discovered already had a fixed place within a pre-established series. The choice of fructification was further justified on metaphysical grounds, possible mathematical variations on the pattern of fructification supposedly representing the potentially viable modifications on the theme of life itself. However, this programme was to prove increasingly unacceptable on empirical grounds. Not only did exploration fail to provide data by which to justify disuniting local family-level fragments which occasionally did not overlap in fructification patterns, but reports of 'exotic' plants always seemed to violate the partial sequences so laboriously worked out by the systematists.

For most commentators, it was the Method which finally managed to shear natural history of its scholastic and rationalist vestiges and thereby inaugurate it into the realm of empirical science. According to Stafleu (1963), it was Buffon and Adanson, rather than Cesalpino[1] and Linnaeus, who set natural history on the path of scientific 'positivism'. Mayr contends that Buffon's 'criteria for *recognition* of higher taxa were entirely different from those which Linnaeus professed to use (total habitus *v.* single characters revealing the essence)' (1982: 182, my emphasis).

Now Linnaeus never claimed that recognition of higher-order groups should be based on single characters; rather, he always maintained that higher-order groupings be first intuited out of a state of nature in the same way as genera and species, that is, in terms of the *habitus*. Intuition of habitus, which represented the external facies of the organism (its readily visible overall morphological aspect: *primo intuitu ex facie externa*), was the basis for the recognition of all natural groupings. For Linnaeus, the habitus was simply the necessary, but not sufficient, condition for the classification of higher-order groupings: 'Although the habitual characters are not sufficient, nevertheless they most often give knowledge of the plant at first glance' (1751: sec. 168). Although rational means must be added to the System in order to produce a deductive classification, it is nonetheless the case that

> *C. Bauhin*[2] and the *Ancients* had marvellously devined by the Habitus of plants their respective affinities . . . often the Systematists have deviated, while the Habitus would have indicated the true way (1751: sec. 163).

In this respect, Linnaeus's system of 'Natural Fragments' (*Fragmenta*, 1751: sec. 77) differed in no significant way from Adanson's method likewise based on the habitus of the plant: *Facies seu habitus Plantae* (1763–1764: **1**, clxx). Elsewhere, Adanson describes the matter as follows:

> Natural history must thus be studied in nature herself; it must be taught verbally and without recourse to books, if one wishes to make rapid and vast progress. It wishes to be considered not from one side, not in terms of one part, as in all modern systems, but by all the mutually combined parts; it is there that the *character of the ensemble* arises, which one commonly calls the habitus (*port*), or that knowledge which indicates the profound naturalist, knowledge by which he classes at first glance a being which he has never seen as easily as he classes those he knows best. It is the character of the ensemble, which has never been used in any method, except in our own . . . Let not this word *character of the ensemble* frighten by its novelty: it signifies nothing more than the result of *the relations which constitute the essence of a being, in such a manner that, at first glance (au premier coup d'oeil), one affirms, without knowing why, that this being is different from all others* [at the required level of generality]. (1847: 14–15, my emphasis.)

According to Cain (1959: 237), Adanson thus provided the first, and clearest, elaboration of a method for constructing higher-order taxa. Yet, despite Adanson's claim that the habitus was never used in any method before him, it appears to constitute a fundamental aspect of all living-kind classifications, in all times and places. Moreover, as Buffon himself observed, the habitus could never provide a *principled* means to consistent higher-order groupings since intuitions about such groupings may vary from person to person, or at least from culture to culture. Either, therefore, some principle must be introduced in addition to the method of the habitus, or classification will remain forever biased towards the observer and hence as inherently arbitrary as any system. It is for this reason that Buffon felt that *no* truly natural classification of supraspecific morphological similarities was possible.

Nevertheless, for such self-proclaimed neo-Adansonians as Sokal and Sneath (1963), Adanson's method is the only way towards a 'natural classification' in Gilmour's sense as that 'which enables the greatest number of inductive statements to be made regarding its constituent groups' (Gilmour 1937: 1042).[3] From this empiricist perspective, the systematic higher-order classifications of Linnaeus and his predecessors are to be considered 'artificial' in the sense of arbitrarily favouring one character over another; *a priori*, in the sense that the character is chosen for the purposes of deducing groupings in an intellectually convenient manner; and 'special purpose' in that the use of deduction presumably depends on its convenience for the taxonomist, and not to its intrinsic value in generating knowledge of the nature and diversity of properties which attach to living kinds as such.

In fact, for the neo-Adansonians, pre-Classical taxonomies are also of this sort. Before the advent of methodical induction *all* classification—indeed, all mental activity—was presumably of this kind; that is, it was initially subject to the purposeful dictates of symbolic passions and economic needs and, in the last instance, to reason itself. Admittedly, post-Renaissance systems were less culturally parochial than folk classification, but they were still anthropocentric: concern with reason alone may very well have been a necessary pre-condition for the emergence of an abstract method for understanding the nature of objects *per se*; however, in this respect the system was still artificially biased towards human concerns.

It is this point of view which has primarily informed ethnobiologists' appreciations of folkbiological classification. According to Berlin, Breedlove and Raven (1966) progress in the development of classification is marked by the move from culturally parochial 'special purpose' orderings to those increasingly transcultural and general purpose. In Bulmer's (1979) view, cultural parochialism is most apparent in higher-order groupings, which are the most 'artificially' constituted. For Hunn (1977), progress in classification is characterised by a gradual abandonment of higher-order 'deductions' in favour of an extension of the kinds of intuitive inductions which produce basic-level groupings. Although some arbitrary 'rounding off' of inductive clusters is always required in order to render their boundaries intelligible, contends Ellen (1979), the development of taxonomy has generally tended towards less artificial cuts in nature.

For the most part, ethnobiological commentary on the status of higher-order groupings in relation to the development of our classificatory knowledge of the living world has centred on named suprageneric folk taxa, that is, life-forms such as tree and bug. Since modern systematists do not recognise the biological validity of such groupings, it seems plausible to argue that their apparently universal acceptance by common folk owes to some strongly anthropocentric imperative. Yet, it is one thing to acknowledge a universal anthropocentric bias, and quite another to claim that this bias is purposeful in the sense of being inextricably bound to some culturally specific world view. For my part, while I admit the first point, I deny the second; why this is, the following pages are intended to show.

In their everyday lives, human beings do not have the concerns of scientists; hence, they need not, and generally do not, occupy themselves with physical and biological microstructures, nor with problems related to astronomical and evolutionary dimensions. But when the bounds of the everyday observer are transcended, as they were in post-Renaissance Europe, the intuitive categories of common sense no longer appear to suffice. It is not that common sense offers an objectively false or fiduciary interpretation of the world which has no transcultural expression; rather, unaided by studied elaboration, it simply does not have the wherewithal to deal with nonphenomenal problems. Nevertheless, inasmuch as it is only through common sense that we come to recognise a problem at all, it is only 'natural' that our initial means of tackling such problems be drawn from those resources which common sense itself has to offer, though the appraisal of these resources is less intuitively valid and more consciously discriminating and fallible. In the present case, the initial solution involved the suppression of one sort of common-sense structure—the life-form—and the elaboration of another, namely the incipient family-level fragment of the folk naturalist; however, this attempted solution had little, if anything, to do with problems of induction, general purpose or de-emphasising the artificial.

Unnamed family-level fragments

Perhaps the first modern author to notice the relationship between covert folk fragmenta and the family concept in biological taxonomy was Bartlett. Reflecting on the folkbotanical classification of the Batak of Sumatra, he notes:

> As to plants in general, there is a partial classification, going to genera or species in hundreds of instances, but leaving many plants unclassified, regarding which all the native botanist will say is that there are trees, herbs, vines, ferns, or mosses. Any very slender sedge is *si marithe-tihe*, 'the one who passes for *tihe*' or 'the *tihe*-like one' (*tihe* being a particular kind of sedge) and many other designations of this are very broadly but discriminatingly classificatory. Here we have an inkling of the family concept and a name which is linguistically a reflection of the same kind of thinking that gave us the botanical family names in current scientific use (1940: 354).

Often, however, 'it is recognised that there are very similar kinds, but nobody bothers to give them names' (1940: 354).

The covert nature of many implicitly recognised plant groupings is mentioned by Conklin for the Hanunóo of the Philippines: 'midgroupings were made, of course, but not according to a structured terminologically-identifiable system' (1954: 97). Ethnobiologists were motivated to seek out evidence for such unnamed mid-level groupings because it seemed quite unlikely that native classifiers, who were able to distinguish hundreds of biologically valid genera, species and varieties, would content themselves with simply collecting these numerous named groupings in lists under a few marked life-forms. It was hardly conceivable that the keen eye of the folk naturalist would miss the more obvious suprageneric groupings entirely.

Berlin and his associates were the first to develop a method explicitly aimed at 'searching for possible subgroupings within contrast sets of large numbers', that is, within life-forms.

> The names of the immediately included taxa of each major [life-form] class name, written on slips of paper, were presented to informants with instructions to read through the lists and place in separate piles those names which applied to plants that were judged to be similar to one another (Berlin *et al.* 1968: 292).

They were able to show that the Tzeltal of Mexico implicitly recognise numerous unnamed groupings which appear to be intermediate between generics and life-forms.

Brown questioned the significance of these results. He argued that, in fact, 'many unlabelled groupings are in reality not covert at all, i.e., that their taxa are cross-indexed under some non-biological labelled categories' (1974: 327). Such groupings only appeared to be covert because they were actually nontaxonomic. For example, Friedberg (1970) had shown in a study of Bunaq (Timor) classification that certain plant groupings which apparently cut across life-forms such as 'tree', 'herb' and 'vine', pertain more to cosmology than to strictly taxonomic concerns. Since they cut across the named taxonomic groupings, one would not expect them to be consistently named under any one such life-form. If they were, then the named grouping would be essentially incomplete (since the intermediate grouping was recognised to cut across life-forms); conversely, if the complete intermediate-level grouping were named, then the fact that the grouping cut across life-forms would contradict the logic of taxonomic relations (such as definite life-form boundaries and total inclusion of subordinate taxa, transitivity, etc.). One such set of groupings refers to plants related by their magical and medicinal qualities. These implicitly recognised *kaluk* groupings cut across the major life-forms; 'nevertheless the systematics of the composition of the *kaluks* is not apparent and does not seem to be directly linked to plant classification. In effect, one finds [such *kaluk* groupings of] plants to be variable in number and to belong to wholly different [named taxonomic] groupings' (Friedberg 1970: 1115). In other words, *kaluk* groupings reflect non-taxonomic (non-morphological, non-ecological) affinities between plants.

Thus, provided that the informant is clear about what kind of information he is supposed to give the ethnobiologist, he may indeed name these ostensibly 'covert' categories; if, for example, the informant shifts from morpho-ecological affinities between plants to more general criteria of usefulness or cosmology (which are not necessarily restricted to living kinds) he may be compelled to provide names for groupings of generics which are related by such criteria. Accordingly, for the Brou of Cambodia:

> In addition to these large categories [i.e. life-forms] that can be qualified as descriptive (it is in effect the aspect of the plant, its habitus and its mode of growth that permits one to classify [a plant] among the trees, herbs or vines, or to designate it as a mushroom), there exist certain others which cover a lesser number of species, but which are important insofar as it is most often a question of useful plants (Matras & Martin 1972: 7).

In his rejoinder to Brown, Berlin (1974) allowed for the possibility of groupings organised on the basis of non-taxonomic criteria, but which nevertheless included taxonomic groupings; however, such non-taxonomic groupings, whether named or unnamed, were *not* the intermediate fragments in question. Such fragments were not organised on the basis of non-taxonomic concerns of usefulness or cosmology; rather, they reflected affinities of habitus only: 'the covert taxa discovered in Tzeltal are formed exclusively on the recognition of gross, visually recognized, morphological similarities and do not represent classes formed on functional considerations' (Berlin 1974: 329; cf. 1976: 395).

Earlier Berlin had argued that mid-level groupings occasionally might be named, especially if they were initially formed to accommodate rare plants or newly introduced plants of foreign origin; these names would presumably disappear once the unfamiliar plant had become familiar enough to have its own distinctive facies.

> The intermediate taxa which have arisen are unstable as the new generics continue to be used over time. In the case of introduced grains, it was suggested that the new forms become conceptually 'a single unsegmented pattern' [recognisable at a glance without reference to the more familiar generic with which it had been initially associated] . . . The final result will be the ultimate loss of the intermediate taxa as *named* categories, although conceptually they will continue to remain (1972: 78).

After considering Shoshoni bird classification, however, Hage and Miller (1976) argue that covert categories may be far from unstable when they assume names. Not only may these names be retained in the subsequent historical development of the folk taxonomy, but such named categories may even provide the source of new life-forms. For Numic speakers such as the Shoshoni, a typically small bird (*huittsuu*) or typically large bird (*kwinna*) may come to be associated with other birds of similar dimensions. The term for the typical bird may, at first, simply bring to mind the associated species, or generics, but over time the name of the typical bird itself becomes a suprageneric label for all birds of that kind. The birds are now formed into two named groupings: the large birds (*kwinna*) and the small birds (*huittsuu*). In the course of time, one or the other grouping may come to represent all the birds (Hage and Miller cite examples of Numic languages where *huittsuu* predominates, and others where it is *kwinna*). The name *not* chosen to represent all the birds may, indeed, disappear from the ethnobiological lexicon; however, those intermediate categories which eventually do provide the life-form names, and which 'are simply covert categories . . . which have became labelled' endure to become arguably the most stable classificatory groups of all (Hage & Miller 1976: 484).

How do we reconcile the conflicting claims of Berlin and Hage and Miller? It seems to me that the situation described by Berlin has little, if anything, to do with that described by Hage and Miller. The conflict resolves once it is realised that the 'intermediate categories' discussed by Berlin (1972) actually cover three distinct kinds of conceptual processes: the incorporation of unfamiliar groupings into the taxonomic system, the emergence of life-forms, and the formation of covert fragmenta properly so-called.

In the first case a foreign species is initially incorporated into the taxonomic system by associating it with the name of a more familiar generic. Once the foreign species acquires its own distinctive habitus it becomes a generic in its own right and is designated by its own distinctive label. The name of the more familiar generic is no longer attached to it. The conceptual affiliation between old and new may indeed persist 'covertly', but I fail to see that this process necessarily provides *the* source of the covert family-level complexes discussed by Berlin and others. Such occasional covert attachments might or might not be integrated into pre-existing family-level complexes. There is no evidence, however, that family-level complexes initially emerge as such covert attachments, that is, by first being named and then having the name disappear. Covert family-level complexes seem to emerge without ever having been named, and in ways which are not necessarily connected to the process of incorporating unfamiliar species into the taxonomic system. The incorporation of foreign species into the taxonomic system could conceivably affect the development of a family-level complex, but such a process of incorporation seems to be neither a necessary nor sufficient condition for the emergence of such a complex.

The second situation described by Hage and Miller appears to have little to do either with the incorporation of unfamiliar species or with the emergence of family-level complexes. What they describe is the development of life-form taxa from their typical generics, such that the typical generic label may eventually come to mark the whole life-form which includes other generics more or less related to the typical generic by aspects of habitus and habits of life. Berlin (1972) describes a process whereby the name of the typical generic lying near the centre of the life-form space becomes polysemous with the life-form; Hage and Miller suggest that the 'typical' generic may be only the typical representative of one pole of the size dimension which structures the life-form space. That the life-form space is structured in this way is indicated by Brown's data (1977; 1979); and the process whereby the label of the typical representative of a part or whole of the life-form space eventually attaches to other generics in its 'sphere of influence' is suggested by Bright and Bright (1965: 253) for the botanical domain. But, again, this process seems not to be necessarily related to the incorporation of only rare or foreign genera into the taxonomic system, nor to the emergence of family-level complexes.

The confusion between the three processes owes partly to the techniques employed by Berlin *et al.* (1968) in their elicitation of covert categories. They presented informants only with the names of generics and specifics *included under* a given life-form. Thus, the method of elicitation may have unduly restricted recognition of covert complexes only to those which happened to fall entirely within the range of a given life-form. Given this rather arbitrary restriction, there would be no reason to suspect that the development of such a complex involves processes which do not exclusively operate upon generics *within* a life-form; conversely, it would then seem only natural that the development of the life-form itself should be related to processes which operate exclusively upon its constituent genera, including processes related to the formation of covert complexes within the life-form. But if, as appears to be the case, covert complexes have little, if anything, to do with the internal structure of animal life-forms, and if, as also appears to be the case, such complexes may cut across botanical life-forms, then the putative connexion between life-form development and the development of covert complexes becomes highly problematical.

One indication that covert complexes may cut across botanical life-forms can be found in Hays's (1976) study of Ndumba (Highland New Guinea) ethnobotany. Hays developed a technique for eliciting covert complexes from non-literate informants. He simply asked informants to enumerate verbally those plants which came to mind when the name of a given generic was invoked. The names thus enumerated were, in their turn, invoked and informants were again asked to specify the plants' names that readily came to mind. For each target generic invoked, a set of associated generics was elicited. Comparing such sets, Hays was able to confirm fairly consistent overlappings: that is, each generic found in the overlap tended to coincide with the other generics in the overlap regardless of which generic in the co-occurring group was used as the target. Informants formed covert complexes on the basis of habitus relations which seem to accord with aspects of the facies characteristic of modern botanical families.

Since biological families cut across folk life-forms, it may well be that covert complexes also do—if it is the case that such complexes actually provide the common-sense ground for the emergence of the family concept in natural history.

> Briefly summarizing the eleven sets of naming responses of *Riedalia* spp., we find that in seven of the sets the co-occurring names form a group of eight which precisely matches the group of taxa which are included in the folk genus *faa'nresa*. . . . Converting the naming responses in the other 4 sets to their appropriate folk generic names, we find co-occurring names: *faahifaan'daura*, *faa'nresa*, *heng'gunru*, and *roro'mmunra*. This grouping, however, does not appear in the 'shared' folk

taxonomic model; in fact, one of the names, *faahifaan'daura*, labels a taxon which is included in a different life form taxon (*sa'tari* ['trees and shrubs']) than are the other three (all being folkgeneric within *mauna* ['herbaceous plants']). Thus, while some plant name co-occurrences are understandable with reference to the folk taxonomic system, the others seem problematic (Hays 1976: 500).

Since, however, only one informant mentioned *faahifaan'daura* on only one set, Hays asks if the cross-cutting may not be due to 'informant error'. In any event, Hays acknowledges that his data are insufficient to enable one to decide one way or the other.

Nevertheless, Friedberg (1970) does provide additional evidence that cross-cutting of life-forms by covert complexes need not be due to informant error. For example, the unnamed complex which Friedberg (1970: 1117) refers to as *haricots* ('beans') contains several named species of plants, some of which belong to 'trees and shrubs', others to 'vines', and still others to 'herbs'; yet all are contained (with the exception of the introduced sesame plant) within the botanical family Leguminosae.

For zoological groupings, the associated generics of animal covert complexes do not so much cut across life-forms as operate independently of the internal structure of the life-form space. If the life-form is internally structured as to size and degree of wildness,[4] one should not expect, say, the common cat and the lion, or the boar and the common pig, to be grouped together within the life-form space; nonetheless, they are usually related in a covert complex.

Another significant factor in the formation of the covert complex noted by Hays (1976: 502) involves an apparent 'chaining effect', first described by Hunn (1975) with reference to American folk ornithological classification. In general, a chain of, say, three taxa x, y and z occurs if x is perceived to be directly linked to y but not to z, and z is perceived to be directly linked to y but not x. The links in the chain, however, need not be symmetrical (see Hunn 1975; 1977 for a discussion) since it is possible that, say, x and y are considered to be more related to one another than y and z are. This may help to explain why it is that the longer the chain, the less likely there appears to be a common name: long and especially asymmetric chains contain fewer characters which are common to *all* the generic links in the chain. This is one possible interpretation of the following observation by Bulmer with respect to the family-level bird classifications of the Kalam of Highland New Guinea:

The Kalam are well aware of certain groupings higher than the species, in which a number of species (which are in fact in most cases in zoological 'genera' or 'families') share a complex of morphological and behavioural characters. Notable examples, where the Kalam appreciate the association of four or more related species are: hawks and falcons; parrots; and within the parrots, lories, or lorikeets, i.e. nectar-feeding small parrots; pigeons; 'typical' nectar-feeding, long-beaked honey eaters; and medium sized birds of paradise and bower birds. I must stress that they do not have standard names for these 'natural' groups—they are what Berlin calls 'covert categories' or 'complexes'—but their reality in Kalam thinking is unquestionable. . . . On the other hand, where a 'natural' group of only 2 or 3 species exists in Kalam territory, and there are many of these also, both within and outside the covert categories I have mentioned (e.g. cuckoos, cuckoo-shrikes, quails and kingfishers, which are not in the larger covert groups; and goshawks, cuckoo-doves and medium sized birds of paradise of *Lophorina* and closely related genera, contained within them), Kalam do tend to have names for them (Bulmer 1979: 62).[5]

Suprageneric folk complexes thus appear to be built on habitus relations similar to those aspects of the facies which mark genera;[6] however, the habitus relations at the family level appear to be somewhat 'looser' than the well-bounded configural relations of the generic facies.[7]

There are yet other, perhaps more compelling, reasons why suprageneric complexes are not usually named: these pertain to the logical, epistemic and substantive natures of higher-order groupings. Life-forms usually manifest three important properties: (1) logically, they are well-bounded by a distinguishing feature or disjunction of such features; (2) epistemically, they represent an ordered analogical field designed to support inferences with respect to the degree of similarity in the habits of life of all constituent genera; and (3) substantively, life-forms represent the major roles in the economy of nature as seen from a human phenomenal perspective. Phenomenal fragmenta, however, are not logically bounded, but often appear to form chains with indeterminate boundaries. From an epistemic point of view, they may be conceived of independently of other such chains: such chains need not exhaustively partition the whole animal or plant domain; they need not partition any given life-form; they may cut across botanical life-forms, and they need not be accountable to the internal structure of an animal life-form space. From a substantive point of view, such complexes represent the habitus relations of plants and animals rather than their habits of life with respect to human-centred ecological considerations. It is probably no mere coincidence, therefore, that covert botanical fragmenta were systematically ordered for the first time in European natural history by the same person who definitively renounced the life-form as a proper division of the plant world;[8] it is plausible, in other words, that fragmenta overtly emerged as the fundamental suprageneric grouping in botany because Linnaeus realised that the logical, epistemic and substantive constraints of life-forms were simply no longer operative.

For Linnaeus, life-forms reflected a common and natural instinct (*Naturalis instinctus*) to regard the objects of the living world in terms of 'the relations they appear to have to ourselves' (1751: sec. 153). Darwin provides an additional insight into their common nature. He recognises such divisions to be 'false' from an evolutionary point of view, though not artificial in the sense of an 'artificial method of enunciating, as briefly as possible, general propositions'; rather, 'It might have been thought (and it was in ancient times thought) that those parts of the structure that determined the habits of life, and the general place of each being in the economy of nature, would be of very high importance for classification' (1883: 365). Appreciation of what plants are *relative to us* will bear little relation to the appreciation of what plants are *relative to one another* once the bounds of the local environment have been transcended: trees are bigger than people, and grasses smaller; trees are where birds most often perch, and grasses are where quadrupeds forage; trees determine the amount of sunlight below, the density of other vegetation and the possible habitats of many of the animals familiar to local folk. Yet, once the boundaries of the local environment are transcended, plant life-forms have hardly more than ecological relevance;[9] they are of little value in organising knowledge of the world outside that economy of nature which is pertinent to man's everyday life.

Nevertheless, it is not surprising that plant life-forms continue to serve as 'natural' frameworks for organising an understanding of the local flora (and fauna), even for the professional naturalist. Thus, we find Linnaeus reporting exotic generics in terms of their life-form status; for example, in the *Critica Botanica* (1737: 238) three new American genera are introduced: *Hernandia* (*arbor Americana*), a tree, *Plumeria* (*arbuscula Americana*), a bush, and *Milleria* (*planta Americana*), a herb. Similarly, Bartlett (1935), in his report on the flora of the Batak Lands of North Sumatra, employs the sorts of life-forms used by the Batak themselves in organising the local flora: (a) trees and shrubs, (b) herbs, (c) ferns and (d) mosses. In any event, once the bounds of the local environment had been transcended for good, natural history was compelled to reconstitute an order of living kinds with whatever fragmentary, common-sense means were still available. It is this reconstitution which came to be known as the 'Natural Method'.

The Method's failure, biology's gain

Generics, like life-forms, constitute a discrete series of well-bounded groups along a *fundamentum relationis*, that is, a mutually exclusive, unequivocal and exhaustive partitioning of organisms. By and large, during the eighteenth century, genera continued to represent the typical facies of local, non-dimensional species (see note 6); hence, they were simultaneously delimited by habits of life *and* habitus from other non-dimensional species by the local naturalist or explorer before they found their way into the reports and classifications of natural historians. Because genera also express habitus relations and not (as with life-forms) exclusively habits of life, they could be maintained in a universal system; however, since the numbers of these basic-level kinds had increased by something like two orders of magnitude from the folk-herbalist period, they had begun to 'burden the memory so' that a more abstract base was required, namely, the family.

Yet, within any local environment this more abstract base is essentially incomplete. First, it is not mutually exclusive: the fragments seem to exhibit a 'chaining' effect over the local flora (and fauna) such that the generics which link family groupings do not clearly belong to one family or another. Second, it is not exhaustive: there is no guarantee that *all* the generics attach to the covert families; there is, in other words, no evidence that the family fragments always cover the entire local flora or fauna. As Lamarck (1778) emphasised, the local flora of the temperate regions of Europe was just too riddled with gaps to provide the complete series required at the family level. Thus, it appears that the local naturalist builds the family habitus by abstraction (omission) of irrelevant content from groups of generics considered in partial isolation from one another, that is, intermittently: if not *in vacuo*, certainly not *in toto* (as are the named taxonomic groupings).

The problem that the natural method posed, then, was that of finding an exhaustive series of families, on the intuitive model of the local covert fragmenta, which would cover any and all local environments. By looking to other environments for similar as well as different fragmenta, and by using such partial series drawn from many different environments, one might eventually hope to fill in the gaps in each and every environment.[10]

Some family-level groupings, such as the umbellifers, conifers, crucifers, curcubits and gramina were already recognised in Europe by the herbalists, and were the first to be given names by the systematists. Other groupings, such as the cacti and the passionflowers, were underrepresented in Europe (by isolated genera or none at all); hence, they were placed within the more prominent European families until further exploration and analysis revealed them as belonging to rich exotic families. It was the Method's task to anticipate all possible families and thereby forego the radical revisions in classification which attended novel empirical discoveries.

Yet, as Candolle (1819) noted, to render the results of the Method unequivocal something more than common sense alone was required. Linnaeus's solution proved unacceptable. It rested on the dubious metaphysical assumption that the functional basis of life was *visibly* manifest in the fructification parts only; and it failed to preserve all previously intuited families, much less account for new ones. In the long run, Adanson's solution was to prove equally unacceptable. Adanson regarded the characters which he attributed to families as fallible and corrigible, in the manner of the 'trial and error' method in mathematics; however, he never doubted that the families themselves were, in fact, invariable placed within the 'scale of nature'. This scale supposedly consisted of a fixed, linear progression of families which increased in complexity from the simplest plant, to the lowliest invertebrate and, ultimately, to man himself. By supposing such a sequence of forms, the intuitive patterns observed could be definitively arranged with respect to future discoveries: additional fragments might be inserted between existing ones (and their defining features slightly altered), but the nature of the scale as a whole would remain the same. Once the general outlines of the scale were discerned, its missing pieces could be computed with apodictic certainty 'as those of the most sublime geometry' (Adanson 1763–4: **I**, cc). But on what basis was such a computation supposed to turn? Presumably, numerical calculations were to be projected in accordance with some conception of 'equilibrium' of parts; families, then, would be ordered in a graduated scale of numerical 'equilibrations', like partial fractions of some integral whole—the integral whole presumably being man himself.

The only problem was that calculation depended upon a definitive mathematical regularity in externally visible parts; however, the anatomical integrity, or machine-like harmony, of family characters was admittedly grounded in internal, functional processes whose external manifestations could only be contingently, not necessarily, related.[11]

At this point, the study of animals seemed to provide the means for saving the scale. Accordingly, Jussieu (1789) argued for a progression based on the complexity of organic functions, and not on visible parts *per se*. There would no longer be the possibility of computing the missing pieces by relating the visible to itself, that is, by a mathematically progressive arrangement of external parts; rather, external facies were to be considered indirect manifestations of 'invisible' vital functions and mechanical causes. This belief was sustained by Candolle (1819) and Lamarck (1809). Not until Cuvier was the anthropocentric bias of this programme fully realised: not only did the notion of 'complexity' uncritically presuppose man as *the* standard of reference,[12] but no regular pattern of functional connexions could be established between plants and animals, or between the invertebrates and vertebrates. Consequently, the putative linear order of connexions between visibly manifest families would be sundered (Cuvier 1829: **1**, xx–xxii). Henceforth, visibly manifest families could only be related via higher-order links based on microanatomy and descent.

In sum, common sense had posed a problem of understanding to the classical naturalists of the seventeenth and eighteenth centuries. They were called upon to modify and extend essentially local means of comprehension to a world-wide scale. Both the rational systematists and the empirical methodists failed to achieve an adequate solution to the problem because they relied upon phenomenal means to deal with a non-phenomenal problem; they remained within the 'common sense episteme' of attempting to 'extend the familiar to the unfamiliar'[13]. Nevertheless, a part of their solution—the creation of the family concept in botany—was to become the indispensable midwife in the birth of biology. By initially disregarding the functional integrity of plant species, and concentrating on the more visibly abstract aspect of plant families, the classical naturalists succeeded in describing, for the first time, a living organisation in the abstract, that is, distilled from environmental and functional context. It happened first in botany, rather than in zoology, because it was easier for common sense to do it that way. Unlike plant life-forms, animal life forms seem to have more than local, ecological relevance, so that the search for an alternative taxonomic order was historically less compelling in the case of animals. Moreover, as Candolle and Lamarck noted, it is simply harder for common sense to separate a living part from the function of the whole when it comes to animals; and since common sense can only apprehend *life* at the individual or species level, it cannot readily see its way to higher-order kinds of functional organisation. Yet, it was zoology which actually presided over the delivery. Once the family was admitted into natural history, zoology would change its status: no longer would the family constitute the basis of classification; it would henceforth represent the most generally visible manifestation of hidden biological functions. As such, the family would furnish the most visibly abstract means whereby common sense could gain access to the non-phenomenal world of biology—a world governed by 'invisible' non-phenomenal causes. But biology itself would develop in accordance with principles alien to the common-sense episteme; it would seek 'to explain the known by the unknown' (Popper 1963: 102)—'to explain the complicated and visible by the simple and invisible' (Jacob 1973: 16)—and so enter the episteme of science.

On the 'naturalness' of fragments

In an important study of the post-Linnaean development of botanical families, Walters convincingly argues that the 'age' of the family (or genus) must be understood '*quite literally* as the age of the taxon as an idea or concept, and *not* in a phylogenetic sense' (1961: 78). In other words the earliest higher taxa to be recognised and incorporated into natural history appear to have been precisely those that were most 'salient' for Europeans (1961: 76). Among the major botanical families of European origin, there appear to be two sorts: those, like the Umbelliferae which have a clearly marked habitus, numerous genera, but only a small number of species per genus; and those like the Rosaceae, which have a less marked habitus. The latter kind more obviously forms a chain of genera linked by partial overlappings in habitus which branch out from a typically well-marked centre (e.g. from the rose).

With regard to the first type of grouping, Walters notes:

> Whilst it is not unreasonable to suppose that our hypothetical New Zealand Bauhin or Linnaeus *might* have recognized a higher grouping corresponding to the Compositae, it seems to me by no means obvious that he would have created a *family* 'Umbelliferae'. . . . Had there been few Umbelliferous plants in Europe, and had they been of no importance for their edible, medicinal or poisonous properties, *Umbella* might well have been a genus (1961: 76–77).

As for the second kind:

> Why is the rose chosen? The answer is clear. This was happening in seventeenth century Europe, where for centuries previously art and literature had been full of certain symbolic flowers. How could any other choice have been made? The 'indefinable' families, then, are associative; the type genus is an important European plant; and the shape of the family is a product of this thought process (1961: 78).

The Rosaceae presumably emerged as a *family* only because there happened to exist a symbolically important plant to which other plants could be 'more or less' associated, and which were not readily included among the other families.

Walters (1961) and Gilmour and Walters (1964) further develop the argument that 'taxonomy has served the general economic needs of man in dealing with the animal and vegetable kingdoms' (1964: 10). They argue that taxonomy, though more rather than less artificial and arbitrary in its beginnings (folk and classical taxonomies), is less rather than more artificial today: nevertheless, even present-day taxonomy is not wholly 'inductive' but is prey to culturally parochial biases. Li (1974) offers a similar analysis:

> At the inception, then, taxonomy was largely based on man's knowledge of cultivated plants. But, as taxonomy gradually evolved from an application of empirical knowledge to a scientific pursuit, botanists more and more shifted their concern to natural vegetation, until, in more recent times, cultivated plants were not only neglected but actually avoided. . . .
>
> In conclusion, we can say that the concept of the family as a category of classification must have been formulated from the very early times when plants were first being domesticated. For ages, the family was, of course, not clearly defined in any way. But the pattern of domestication of plants by mankind clearly indicates that some recognition of plants on the basis of what is interpreted by modern taxonomy as the family must have existed in those early times. The origin of cultivated plants also serves to support the idea that the family is a natural category of classification and is not as arbitrary a concept as the ordinal level above it (1974: 715, 719).

In other words, although culturally parochial interests instigated perception of botanical families, the families thus perceived turned out, in fact, to be by and large 'natural'. Of course, the exact limits remained an arbitrary matter of choice, and the initial scope was restricted to cultivated plants; but nature itself would eventually prevail through the fog of arbitrary prejudice in virtue of the 'scientific' method.

The only problem with this rather straightforward and conceptually elegant account of the family concept is that it is not in the least warranted by the available evidence. In the first place, there is no evidence to support the claim that early or contemporaneous folk classifications were or are, initially, exclusively or primarily 'artificial', 'utilitarian' or 'economic'. In the second place, it is inconceivable that human beings would have recognised the possible links between domesticable plants of the same family unless they already had an antecedent notion of family which allowed the mental connexion to take place.

Although the boundaries of the rose family are subject to doubt, its aspectual nature is not, and has never been, doubtful. Adanson, for example, who was unimpressed by any symbolic connotations of the rose, noted that the Rosaceae family was represented in Africa on the same aspectual basis as described by Tournefort for the case of Europe and the Levant: namely, in terms of the relative proportion of most parts of the habitus, and, in particular, the elements of the corolla and the position of the corolla as a whole with respect to the other parts of the flower. As for the umbellifers, Adanson himself noted that they were very poorly represented in certain parts of the world: indeed, hardly a species, much less a genus, of umbellifers could be found in Senegal (Adanson 1763–1764: clvii). Nonetheless, he insisted that the umbellifers formed a natural family world-wide because of the distinctiveness of its habitus. True, in the course of time certain of Tournefort's sections and Linnaeus's fragments were eventually reduced to genera, while certain genera poorly represented in Europe were eventually raised to the level of family. But this apparently had nothing to do with 'economy', 'symbolism', 'domestication' or the like.

Another version of the argument that those plants which are classified tend to be the most culturally important is contained in Kesby's assertion that the flowering plants are more richly named than non-flowering plants *because* 'with their varied colours and shapes, [they] lend themselves to symbolic interpretation much more than do most other plants' (1979: 45). Such an account is, however, hardly compelling. Symbolic interpretations of flowering plants vary much more from culture to culture than does the nature of the taxonomic structures themselves; so that it is hardly likely that what appears to be a universal tendency to name flowering plants derives from a source so variable and parochial as symbolic interpretation. Moreover, the argument is circular; for if the flowering plants lend themselves to symbolic interpretation, it is precisely because they are more varied in colour and shape, and also larger and more numerous than non-flowering plants. In other words, they are appropriate objects of (widely differing) symbolic evocations because they are so manifestly salient from a phenomenal point of view. But symbolic interpretation itself is neither a necessary nor sufficient condition for the naming of plants, though the antecedent common-sense apprehension of plants is a necessary condition for any symbolic evocation about them.

Such views I take to result from a deceptive empiricist epistemology wherein human knowledge begins with some vague symbolic or semiotic capacity which artificially extends an immediate and practical knowledge of the world to 'metaphysical' truths. On these accounts, such speculative understanding of the world gradually reduces to a more patient, though immediately less gratifying, extension of inductions to 'probable' truths which presumably follow from reasoned observation and experiment. Thus, by concentrating on symbolically idiosyncratic and scientifically specific aspects of thought, one is misled into believing that knowledge has progressed via a change in the *operations* of thought; for, surely—so the story goes—the symbolic operations of the primitive-child differ from those of the adult-scientist.

From a logical point of view, however, cultural symbolism (which is highly evocative and operationally idiosyncratic precisely because it does *not* constitute basic, propositional knowledge of the world in any meaningful sense) and science (which *is* propositional, that is truth-valuable) cannot be related; nor can the former, therefore, presume to be the historical or ontogenetic source of the latter. In fact, symbolism and science are both rather peripheral *sources* of human knowledge (though they are not peripheral activities of minds or societies) in the sense that neither symbolic 'understanding' nor scientific knowledge is a necessary or sufficient condition for our rich and varied knowledge of the everyday world. On the contrary, our innately grounded common-sense understanding of the world (such as its geometrical, organic and chromatic structure) is both prior to, and necessary for, any symbolic or scientific elaborations of that world.

The fact that alternative classifications were, and are possible, and far underdetermined by the biological data at hand, does not constitute grounds for claiming that the taxonomy actually chosen is in some sense culturally biased. The fact that new data may lead to the disconfirmation and subsequent re-evaluation of certain groupings also tells us nothing about cultural 'need'. Certainly it is the case that any re-structuring of a given grouping will depend on some prior perception of structure. Admittedly such prior structuring was biased to a European perspective. But even if one grants that the 'phenomenal' world of the early taxonomists was bound to European flora and fauna, and that subsequent developments in the history of taxonomy were influenced by this fact, there is no cause to consider subsequent taxonomy 'arbitrarily' skewed. The fact that folk taxonomy is restricted to a particular environment also tells us little about its relative 'naturalness'. We are led to consider the covert fragments of the folk taxonomists as 'natural' because for the most part they do happen to correspond to morphologically obvious relations of habitus which can be, and are, ascertained independently of concerns for function or use. Even if the constitution of such groupings significantly alters as more data are accumulated and more detailed comparisons made, there is no reason to believe the previous groupings to be any more 'artificial' than, say, a Euclidean interpretation of the geometrical structure of the world is relative to a Reimannian interpretation.[14]

Conclusion

The preceding was an attempt to elucidate a problem which folk understanding of the living world posed for the classical naturalists of seventeenth and eighteenth century Europe. My aim was thereby to illuminate the conceptual link between our ordinary, everyday understanding of the phenomenal world and scientific understanding. By doing so, I meant to suggest that anthropologists can make a positive contribution to epistemology by exploring the scope and limits of common sense thought. For the most part, anthropologists have merely sought to compare and contrast scientific thinking with phenomenal understanding of the everyday world, and to suggest that the distinctions between these two modes of understanding owe to differences in the operations of thought. Science has been the standard of reference against which the findings of common sense are judged; *ipso facto*, the philosophy of science has become the ultimate arbiter in all matters of epistemology.

But if the standard of reference is reversed—as it should be inasmuch as science is dependent upon the operations of common sense, and not vice versa—then it is anthropology which becomes the principal focus for the theory of human knowledge. On this account, historians and philosophers of science cannot pretend to deal adequately with their subjects without first acquiring a measure of anthropological competence. Anthropologists, in turn, cannot really hope to make sense of history and science if they do not take on the responsibility of grounding those subjects in what is common to humankind. Anthropology, in other words, is obliged to approach these subjects in order to be taught by them: not, however, in the manner of a pupil who agrees to everything the master likes; but as an appointed judge who compels the witnesses to answer the questions which he himself proposes.

NOTES

[1] Cesalpino's *De plantis libri XVI* (1583) represented the first attempt at a rational system of botany.

[2] Bauhin's *Pinax theatri botanici* (1623) was the most exhaustive and accomplished of the European herbals.

[3] This is essentially Mill's view of the aim of 'scientific classification' except that 'generally useful' is Gilmour's substitution for Mill's 'most important'. Either way, this notion of generality, or importance, lies strictly within the eyes of the beholder (user). There just is no way to build an objectively 'natural' classification which does not presuppose some prior point of view (intuitional aspect, structural morphology, functional anatomy, ecology, descent, and so on). Thus, phylogenetic classification is 'special-purpose' from the point of view of the ecologist and vice versa (e.g. the ecologist would group together Euphorbiaceous plants and their Cactaceous homeomorphs but the phylogeneticist would not). Furthermore, it is impossible, in principle, to decide what hitherto unimagined uses may be found for any given mode of classification. There is, in other words, no absolute notion of 'general-purpose' or 'importance' independent of concerns with present context and use.

[4] For example, mammal and bird life-forms appear to be internally structured in terms of degree of ferociousness: e.g. the zebra may be separated from the donkey and horse within the life-form space; however, they may be covertly grouped together (see Atran 1981; in preparation). Note that Buffon, although acknowledging family similarities between the wild and domestic equids (1753, v. 4, 378–91), argues that they should nevertheless be classed apart because of the different relations they have 'relative to us' (1749, **1**, 32–40), that is, according to their structural position in the life-form space (cf. Bonnet 1782: 173). For, if all higher-order groupings are artificial, at least life-form groupings are consistently so. Later (1766, **14**, 335–6), Buffon would come to accept family-status, rather than life-form status, as crucial to an understanding of the natural history of animals; but only because he believed that he had discovered an 'objective' relation underlying the subjective sense of morphological similarity, viz., descent.

[5] Similarly, for the Aguaruna of Peru: 'While not all folk generic categories are members of these [covert] groupings, complexes include the vast majority of all Aguaruna birds. Finally, Aguaruna complexes are remarkably similar to well recognized ornithological taxa at the family and sub-family levels . . . at least some of these mid-level complexes are named' (Berlin *et al.*, 1980: 18).

According to Aristotle, 'common parlance' broadly distinguishes three levels, or 'genera', of animal kinds: the basic-level, or *infimae species (atomon eidos)*, which consists of the horse, the dog, the red-deer, etc.; 'largest (*megista*) genera', which comprise most, if not all, animals, such as birds, fish, bugs ('bloodless animals' but including worms), viviparous and oviparous quadrupeds; and intermediate groupings such as the rays and sharks (*selachi*) and equids (i.e. beasts with 'long-haired tails'). For the most part, however, these intermediate groupings are 'lacking a common name' (*anonyma*): If [species] *anonyma* means [species] having no [name], it cannot refer to [lion, red-deer, etc.]. It must refer to groups that contain these types and are themselves contained within [the viviparous quadrupeds]. What, for example, is the name of the group that contains the lion? We have one ('Cat'), but Aristotle has not. All that he has at this level is ['long-haired tails', i.e. equids], and the rest are nameless. [Species] *anonyma* therefore denotes . . . groups intermediate between the main [life-form] groups and the [basic] types visible in nature (Balme 1962: 91).

[6] This, I take it, is what Linnaeus meant when he stated that: 'The Class is more arbitrary than the Genus' (Linnaeus 1751: sec. 162). Mayr, however, seems to interpret the matter differently when he writes that Linnaeus 'was less interested in categories higher than the genus . . . It is evident that the higher categories were for Linnaeus primarily information retrieval devices' (Mayr 1982: 173, 175; cf. Stafleu 1971: 28). Yet, Linnaeus explicitly declared that a system by which 'one [natural] order can be set aside from another' was 'the primary and ultimate' goal of botany (Linnaeus 1738: 484–7; 1751: sec. 77). Because the habitual boundaries of fragments are more intuitively vague than those of genera, Linnaeus felt that rational devices would play *the* decisive role in the exact delimitation of the former. Apparently, he eventually despaired of finding just the right devices, and even crossed out the statement that 'nature makes no leaps' in his own copy of the *Philosophia Botanica* (see above). But he never abandoned his stated interest and belief in the significance of family-level groupings. If anything, his interest increased as the ever greater number of reported genera began to prove too unwieldly for his system. In any event, throughout his mature works (including the sixth edition of the *Genera Plantarum*, 1764), he maintained that the plant world was naturally divided into four ranks; of these, three were essential (Class, Genus, Species) and one, incidental (Variety).

[7] For the most part, fokgenerics are represented in terms of well-bounded 'gestalts' which correspond to the non dimensional, morpho-geographical species of the modern taxonomist (see Atran 1981).

[8] Actually, it was Jungius (1587–1657), the seventeenth century Paduan logician and herbalist, followed by the Leipzig physician Rivinus (1652–1753), who first explicitly rejected the botanical life-forms. Similarly, Ray points out that these ancient divisions are 'popular' but not 'philosophical' (1682: 24–5). However, unlike Linnaeus these authors offered no intuitively obvious phenomenal substitute as a basis for higher-order groupings. In the *Critica botanica* (1737: sec. 12), Linnaeus first made the point of labelling Tournefort's (1694) unlabelled 'sections'. In the *Classes plantarum* (1738), Linnaeus numbered all of his own corresponding fragmenta, and in the *Philosophia botanica* (1751) he named them. Adanson renamed many of these but did not significantly alter their substance (i.e., from a modern viewpoint, the range of properly admissible genera and species, cf. Croizat 1945).

[9] The case is somewhat different for animal life-forms. Since we ourselves are vertebrates (whether or not we are classified as such by the folk naturalist) initial common sense appreciation of vertebrate life will not be so far removed from what was eventually to become an appreciation of what the vertebrates are relative to one another. For this reason, vertebrate life-forms (e.g. mammal, fish, bird) remained the principal higher-order rank of zoological taxonomy throughout the eighteenth century, that is, until microanatomical study of the phenomenally residual invertebrate life-forms (e.g. insects, worms) revealed that the vertebrates, taken as a whole, were no more inherently diversified than any one of the major sub-groupings of 'bugs'.

[10] A.-P. Candolle describes the process as follows: Hardly has he [the naturalist] cast his eye upon the plants, that he recognises certain well pronounced groups by the *ensemble* of their structure, wherein the individuals, taken together, have a certain family resemblance (*air de famille*), as, for example, the Graminae, the Umbelliferae, the Cruciferae, etc. He will perceive that it is easier to recognize, at first glance (*dès le premier coup d'oeil*), these natural groupings, than to research in detail their characters each time he finds them in a new individual; at length, he will think that Europe is not the only part of the world where one can find such groups. As he advances in study, he will perceive that most of the plants of Europe which seem to him isolated by their structure, are part of families in which the majority of individuals are exotic; he will then conceive that it would be possible to arrange all of the well known plants in such natural groups, that is to say, determined by

the *ensemble* of their anatomical resemblances, and that such an order will give to whomsoever comes to know it the most faithful image of all that we know of the structure, and consequently of the history of plants. It is this manner of arranging the plants after the *ensemble* of their essential organs which carries the name of the Natural Method; it is to this study that all of the most celebrated naturalists have consecrated themselves; it is this which Linnaeus, whose name one so often abuses, declared to be the goal of all natural history (1819: 51–52).

[11] According to the neo-Adansonians, the natural method has always consisted of a comparative examination of *all* the parts of the organism. Thus, to avoid '*apriorism*' the doctrine allows neither intuition nor abstract theory to determine which characters are available for analysis. This, however, is absurd. By excluding 'intuition', not to mention more consciously elaborated inferential frameworks, one thereby excludes from possible consideration not only enzymes, nucleotides and chromosomes, but gills, feet, antennae, wings, roots, leaves, stems, flowers and branches. All such characters depend upon the human being's intuitional ability to discriminate certain perceptible sensory-motor configurations in his environment. Indeed, it is precisely intuition which, for Adanson, was to provide 'all' of the characters for analysis. He knew little of, and cared even less for, the internal or microstructural characters so important to all modern systematists (including the beo-Adansonians) (cf. Adanson 1763–1764: *1*, clv*i*, clxv*ii*). Adanson was exclusively a natural historian who sought to establish the visible order of things; he was not a biologist interested in nonphenomenal causes and processes.

[12] Man was first introduced among the families of animals by Linnaeus (1734).

[13] True, it was no longer simply a matter of reducing the novel to the customary by denying the specificity of all that which was not intuitively familiar—a strategy typical of folk (Atran 1981) as well as those early European explorers whom Buffon criticized for naively 'assimilating the known to the unknown' (Roger 1982: 12). As Roger emphasises, however, there is a failure to realize that the great object of Buffon's own *Histoire naturelle* (which served as a model for the method of Adanson and others), 'had been to found the knowledge of the eternal order which reigns in nature upon the study of sensible phenomena' (1971: 584). Natural history had thus not yet fully abandoned the privileged position it accorded to phenomenal kinds in the scheme of things; for in science phenomenal kinds merely provide access to underlying essences, and are not themselves necessarily those underlying natural kinds to which nomic laws apply. Thus for Darwin, as opposed to Buffon, descent relations are not invariably marked by morphological similarities.

[14] As Nagel points out: 'If Euclidean geometry rather than one of its rivals is accepted as true [natural] . . . it is accepted partly for the historical reason that the Euclidean system was the first to be developed, and partly because it appears to be psychologically simpler than alternatives to it' (Nagel 1961: 266). Clearly one cannot conclude from the fact that modern physics was influenced by Euclidean geometry that either the original Euclidean-based Egyptian folk-science of 'earth measurement' or modern physics is more or less 'artificial'. Theoretically, there are any number of possible systems of physics all equally well justified, just as there are any number of well-justified biological taxonomies; nonetheless, at any given moment, out of all conceivable constructions, 'a single one has proved decidedly superior to the rest' (Einstein 1954: 221). It is 'superior' only to the extent that it is historically and psychologically 'familiar' (cf. Poincaré 1903) hence, all the more easily tractable and progressively modifiable. This familiarity likely owes to an innate cognitive disposition to apprehend the everyday phenomenal world in particular ways (cf. Kant 1923; Atran, 1982). This common-sense appreciation of the world remains wholly valid, however, only so long as it does not assume to account for data which transcend the bounds of ordinary perception (e.g. of astronomical, evolutionary or microscopic dimensions). When such bounds are transcended common sense no longer suffices, though it remains the ineluctable base from which all further understanding must necessarily proceed (Atran in press).

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