LOGIC AND MEMORY IN LINNAEUS'S SYSTEM OF TAXONOMY.

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INTRODUCTION.

Taxonomists have been consulting Linnaeus's books ever since he wrote them, in order to improve his and their own arrangements, definitions, or nomenclature of particular sorts and groups of living things, both plants and animals. Svenson (1945) has reminded us of Linnaeus's actual methods of description and nomenclature, and has discussed briefly his notions of the variety, species and genus, and the natural system. Ramsbottom (1938) has examined in detail the changes in his notion of the species. I myself (1956) have discussed the genus in Linnaean and subsequent taxonomy. There is also a highly significant remark by Uggla (in Schmidt, 1952) on Linnaeus's use of logic. Apart from these, few have commented at all on the theoretical considerations that influenced him.

They should be examined for two reasons. In the first place there is no better method for scientists of one period to bring to light their own unconscious, or at least undiscussed, presuppositions (which may insidiously undermine all their work) than to study their own subject in a different period. And secondly, when the writings of an earlier author have apparently been taken as the basis of subsequent work, constant scrutiny is necessary to prevent his presuppositions becoming fossilized, so to speak, in the subject and producing unnoticed inconsistencies when modifications have been made as a result of subsequent work.

In the course of a comparison between Linnaeus's principles and those appropriate to evolutionary taxonomy, it became obvious that two of the most important features of his system had hardly been commented on. These are, his use of the general principles of all classification laid down by Aristotle in his *Logic*, and his insistence that all botanists must know and remember all the genera. The peculiarities of Linnaeus's system are in great part consequences of these.

This paper gives a brief survey of their effects, as a preliminary to a detailed study of Linnaeus's own theory of taxonomy, not as a revision of his arrangement of any particular group of organisms. Quotations are usually given in English for convenience; the translations are my own, as few suitable ones are available. The elegant translation of the *Critica Botanica* by Hort (1938) has unfortunately sacrificed terminological accuracy to literary polish and is consequently unsatisfactory in its rendering of words which to Linnaeus were technical terms and therefore not to be replaced for aesthetic reasons by any of a number of vague synonyms.

Logic.

(i) *Genus and Differentia.*

In the present state of Western education it is easy for biologists to forget that every well-educated man in and before Linnaeus's time might have been given some instruction in the principles of classification in general, as laid down by Aristotle. This he would receive in the study of *Logic*; and in consequence some terms such as Definition, Genus, Differentia and Species would have for him as general an application and significance as is possessed to-day, for most biologists, only by Definition or Difference, terms obviously not confined to biological taxonomy. Useful introductions to this system of Logic and its corresponding philosophy are given by Maritain (1946*a*, 1946*b*) and an important full discussion by Joseph (1916).

A brief summary of the Aristotelian position as it concerns this paper is as follows. If B is predicable of A, it stands to A in one of five relationships; it may be a Definition, a Genus, a Differentia, a Property, or an Accident. The *Definition* of a subject is the statement of what that subject must be in order to be just that and not something else, the statement of its *essence*. The essence cannot be changed and the subject remain the same. The essence is that which mediates its actual existence (Maritain, 1946*a*). The *Genus* is that part of the definition (and essence) of a subject which is predicable also of other subjects. For example, in the famous definition of Man as a reasoning animal (*animal rationale*) the genus is animal, which can be predicated not only of man but of other living beings (namely, non-rational animals) as well. The *Differentia* is that part of the definition which is not predicable of other species in that genus, and therefore expresses the difference between a particular species and all others in the genus. A *Property* is an attribute of any subject which is common and peculiar to it, and follows from its essence as a consequence; it is therefore not included in the definition. An *Accident* is any other attribute of a subject, which has no necessary connection with its essence, and may or may not be present in any given individual of it. For example, the colour of a jug has no necessary connection with its "jugginess" and can vary without having any effect on it.

According to Aristotelian logic, the genus should not be regarded merely as a collection of species. The genus and the differentia taken together are the definition of the species, the statement of its essence; "the genus is the general type or plan, the differentia the 'specific' mode in which that is realized or developed . . ." (Joseph, 1916, p. 83). Consequently, "Individuals are not included in one genus because agreeing in certain attributes, and then in one species within the genus because agreeing in certain other attributes that have no connection with the first; as you might include in one island all men who had red hair, and then rail off separately within it those of them who had wooden legs; *wooden-legged* could not be a differentia of the genus *red-haired*; it must be some modification of red-hairedness itself, and not of the men having it, which could serve as a differentia to that genus. It is therefore a phrase that may mislead, to say that the differentia *added* to the genus makes the

species, or makes up the definition. For adding suggests the arbitrary juxtaposition of independent units; but the differentia is not extraneously attached to the genus; it is a particular mode in which the genus may exist. And hence, when we distinguish the various species of one genus, in what is called a logical division, assigning to every species the differentia that marks it off from the rest, our several differentiae must be themselves homogeneous, variations, as it were, upon one theme . . . The principle that the differentiae must be thus cognate is technically expressed by saying that there must be one *fundamentum divisionis* . . ." In the genus triangle for example, if one species is isosceles, the others will be equilateral and scalene, the *fundamentum divisionis* being here the proportions of the sides; but to have one species isosceles and another right-angled means that two *fundamenta* are being used simultaneously, and this is illogical.

Obviously, to classify in this way is most important, if it can be done, since by exhibiting the essence one is showing what each species really is, and what are the fundamental differences between it and the others in that genus.

(ii) *Taxonomy of Unanalysed Entities.*

But in such a classification the essence of the species must be determined before a definition, by genus and differentia, can be produced. Now it is true that where *a priori* knowledge is concerned, we can determine what is the essence of any subject necessarily; that of plane triangularity, for example is " a plane figure bounded by three straight lines ". What follows necessarily from such a definition will be a property of the subject, what does not (e.g. the colour of the lines actually used to bound one such figure) will be accidental. The ideal classification described above can therefore be put into practice. Mathematics, and especially geometry, studied by both Plato and Aristotle, is the most obvious example of a subject-matter suitable for classification by genus and differentia.

But when we cannot, or at least do not, see the necessary connections between the essence and the properties, and do not know what is a property and what an accident, and indeed can approach no nearer to a definition than a mere statement of what we intend a particular word shall mean, such a classification becomes impossible. We can only proceed empirically, simply finding out what subjects exist and what are their attributes, not deducing them from known principles and axioms. Where logical division is possible, we can have a *taxonomy of analysed entities*; where not, only a *taxonomy of unanalysed entities* is possible, and the best example of it is indeed biological taxonomy.

This difficulty with regard to the subject-matter of biology and other natural sciences has, of course, been recognized by logicians for centuries. Maritain (1946*a*) in his introduction to Aristotelian and Thomistic philosophy puts it thus (p. 154). " It is important to bear in mind that the experimental sciences are very far from being able to know perfectly the essence of the things which they study. They are, in fact, unable to attain a truly distinct notion of their essences, and never possess more than a confused or purely descriptive notion of them. They know them, so to speak, after the fashion of a blind man by means of indirect signs." Joseph (1916) remarks that " the problem of distinguishing between essence and property in regard to organic kinds may be declared insoluble. If species were fixed: if there were in each a certain nucleus of characters, that must belong to the members of any species either not at all or all in all: if it were only upon condition of exhibiting at least such a specific nucleus of characters that the functions of life could go on in the individual at all; then this nucleus would form the essence of the kind. But such is not the case. . . . There may be deviation from the type, to a greater or lesser

degree, in endless directions; and we cannot fix by any hard-and-fast rule the amount of deviation consistent with being of the species, nor can we enumerate all the points, of function or structure, that in reality enter into the determination of a thing's kind. Hence for definition, such as we have it in geometry, we must substitute classification; and for the demonstration of properties, the discovery of laws. A classification attempts to establish types; it selects some particular characteristics as determining the type of any species; these characteristics should be (*a*) of the same general kind for each type within one genus, or . . . variations upon the same theme, in order to exhibit the mutual relations of agreement and divergence among the various types: (*b*) important, or, as one might say, pervasive: that is, they should connect themselves in as many ways as possible with the other characters of the species. It will be the description of the type, drawn up on such principles as these, that will serve for definition".

The relevance of these quotations from logicians for the whole history of biological taxonomy from Aristotle to the present day can hardly be overestimated. They epitomize the most important change in taxonomic theory that has occurred, namely the gradual abandoning of attempts to set up classifications on *a priori* principles agreeable to the rules of logic and some particular theory, and the partial substitution of an empirical attitude. This substitution was not complete when the theory of evolution arrived to provide a new theoretical approach to the problem of classifying organisms, the full implications of which have still not been completely thought out. Moreover, in the quotation from Joseph, we see the logically trained mind finding a practical solution to the problem by setting up just such a system of types as was in fact evolved by taxonomists, and doing so for exactly those reasons, as I believe, that influenced them, namely that this was the best approach to a logical division that could be made. The principles of logic were still their guide and pattern. It is not surprising, therefore, that Linnaeus should declare the arrangement (*Dispositio*) of plants to be theoretical, which institutes the Classes, Orders and Genera, or practical, which institutes the species and varieties and can be carried on without the knowledge of any System (*Philosophia Botanica*, Aphorism 152). Theoretical arrangement for Linnaeus is that to be carried on according to principles, practical that which can be only empirical.

(iii) *Definition.*

As Hopwood has reminded us (1950), certain rules can be given for defining anything, to avoid tautology or irrelevance. Those he gives are as follows.

1. The definiens [defining expression] must be the exact equivalent of the definiendum [that which is to be defined]; it must be neither wider nor narrower in its scope.

2. The definiens must not include any expression which is contained in the definiendum, or which could be defined in terms of the definiendum. (Otherwise, to that extent the definition is circular.)

3. The definiens must be expressed in the clearest possible terms.

4. The definiens must always be positive unless the definiendum itself is negative.

These rules are purely formal (except perhaps No. 3, which is primarily aesthetic or economical) and have no foundation in any particular philosophical theory. It is of interest to compare them with those given by Joseph, which are more closely Aristotelian and therefore more like those which Linnaeus would have been taught to observe. In these, the above four are given, in different words, but there are two others which are placed first, namely :—

1. A definition must give the essence of that which is to be defined [and

where this is impossible, one must just do the best one can, along the lines suggested in the quotation given on p. 147].

2. A definition must be *per genus et differentiam* (*sive differentias*). The reason is that this indicates both the general kind of the subject, and the attributes which qualify it in particular.

The addition of these two rules firmly attaches the theory of definitions to Aristotelian logic in particular. Linnaeus's own ideas on definition were clearly based on Aristotle's as far as possible. The *Generic Character* (not character in the modern taxonomic sense, for which he always used *nota*, a note, characteristic or mark) is the same as a definition of the genus. ("Character genericus idem est ac *Definitio generis*" *Phil. Bot.*, Aphorism 186). The *Essential Character* of a genus is that which gives some characteristic peculiar to it, if there is one such, which will instantly serve to distinguish it from all others in the same natural order. (The *Natural Character*, which is the best, contains all the generic characteristics of each genus and is the most difficult to draw up; the *Factitious Character* is one that distinguishes a genus from all others in an artificial order and is used only as a succedaneum (*Phil. Bot.*, 190) until the natural classification can be discerned.) The true specific name is a *Differentia essentialis* (*Phil. Bot.*, 257) distinguishing that species from all others in the same genus—and Linnaeus insisted that all genera are natural. Specific names taken from hypotheses—i.e. those based on artificial classifications made according to preconceived rules—are false (*Phil. Bot.*, 258). The *Character naturalis* of a species is a description; the *Character essentialis* is a Differentia (*Phil. Bot.*, 258).

From these statements, it can be seen that the essential character, i.e. one that states the essence, and the differentia are respectively that which defines uniquely the general kind (genus) and that which states the peculiarities that characterize one particular species (differentia of the species within the genus). This is pure Logical Division. But, as already shown, such definition is appropriate only to a taxonomy of analysed entities; here we have no such thing, and it is understandable, therefore, that Linnaeus found himself unable, because of the empirical facts, to prescribe any set principles on which species could be differentiated, or to accept the first division into woody and herbaceous plants made by Cesalpino (1583) on first principles. Yet he maintained the necessity of basing the theoretical part of classification on the fructification, which is the next division on Cesalpino's principles.

It is easy to see how the emphasis of the logical aspect of such a theoretical classification, with the obvious reminder that only individuals exist in Nature, not genera as such, will very readily suggest that the genus in the logical sense is an abstraction, a general idea, existing only as far as it is realized with modification in the individuals of particular species. The step from this to its being an Idea in the mind of the Creator is reasonable; the species of a genus are then variations on a supernatural theme. From this point of view Fries (1835, p. xvi) was right in saying that the classification expressed "*quoddam supranaturale*". Linnaeus, too, was quite right on his own principles in insisting that *because* the stamens, pistil, etc., subserve the extremely important function of genuine sexual reproduction, the classification must be based upon them; in this he was following the rules of logical division and the practice of Cesalpino by trying to get at the essence. Sachs's criticism (1875), perfectly right in practice, that they would be just as useful if their functions were unknown or they had none, reveals a considerable misunderstanding of Linnaeus's point of view. Whether in the then state of knowledge, it is reasonable to expect that Linnaeus ought to have perceived the impossibility of any taxonomy of analysed entities, and have gone straight over to a purely empirical classification, as Sachs implies, I do not know. Sachs (1906, pp. 41,

81, 92) makes the special point that Linnaeus was the first to appreciate the difference between artificial and natural classifications, and takes that to mean the difference between a classification founded on predetermined principles and one according to natural affinity; consequently, he blamed him (1906, pp. 101–102) for not working more assiduously at the natural system. I think, however, that what Linnaeus meant by the natural system was not quite what Sachs meant (see p. 154), and in consequence have put the word *natural* into quotation-marks when the Linnaean meaning is intended. De Candolle (1813) also stated that Linnaeus was the first to separate natural and artificial methods, but his usage of the word "natural" is much more like that of Linnaeus (Cain, in preparation).

What Linnaeus did find, at least in practice, was the ease of transition from the purely logical attitude to the empirical in which the genus is indeed a larger group containing its species as smaller constituent groups. And since one could not know what sort of species with what peculiarities would turn up next, the application with any consistency of the principle that in every genus there must be only one *fundamentum divisionis* very rapidly became impossible. It is not surprising that he described the *Dispositio* of species and varieties as practical and not theoretical. By experience it was known that they could be determined only by going and seeing. Once discovered, they could be arranged according to pre-determined principles, of which important ones for him were that groups must be based on the fructification, and that in any sort of classification the genus and species must be "natural", and the sooner the other groups could be made so, the better.

Linnaeus was never faced with the problem of defining separate and distinct sorts in either a time-series of fossils or a widespread species showing continuous geographical variation. Definition in the logical sense of separate entities, as remarked above, is not possible in a taxonomy of unanalysed entities, and this was a serious problem for him; but the possibility that sorts of organisms might intergrade *smoothly* in time or from one country to another seems not to have arisen. Where continuous variation between otherwise distinct sorts occurs, the *universal* application of the definition of discrete groups breaks down. Biologists have known this for many years but only recently has it become an urgent practical problem. The rules of definition given by Hopwood are free from any reference to essence, genus and differentia; but they still presuppose that there are definienda with ascertainable limits. Some of the greatest difficulties in the species-concept at the present day arise because that presupposition is not obviously justified (Cain, 1953, 1956; Mayr, 1942; Simpson, 1951).

(iv) *Division.*

The process of taking a particular genus and distinguishing the species within it is called Logical Division. And "the better our division of the genus into its species, the larger will be the number of general propositions that can be made about its species or parts" (Joseph, 1916, p. 115). The utility of good division is therefore obvious, and it is not surprising that systematists wished to follow it as closely as possible. The close connection between division and definition is also obvious. Joseph uses classification as the opposite of division; in division the general kind is broken up into specific modes, in classification the species are brought together under their proper genera. If division (or classification) is carried through several stages, the technical terms for the stages are *genus summum* for the main kind to be divided, *genera subalterna* for the intermediate kinds into which it is divided and for their divisions similarly, until one reaches the *species infimae* with which the division stops. Linnaeus specially quotes this hierarchy (*Phil. Bot.* 155; *Syst. Nat.*, 10th Ed.,

p. 7, and partially in *Crit. Bot.*, 251) in the form Genus summum, Genus intermedium, Genus proximum, Species, Individuum, as an exact parallel with Class, Order, Genus, Species, Variety. (In fact, it is not strictly parallel since logical division, dealing necessarily only with universals, cannot proceed to the individual as such and the Variety is still a universal.) The same hierarchy was used explicitly by Ray and others of Linnaeus's predecessors as their arrangement of ranks.

The rules given by Joseph, by which division must proceed in Aristotelian logic, can be stated as follows.

1. A division must be exhaustive (otherwise something will be omitted).
2. The constituent species of the genus must exclude each other (otherwise there will be an incomplete separation and therefore faulty definition of the species).
3. A division must proceed at every stage, and so far as possible through all its stages, upon one principle, or fundamentum divisionis.

Only if a single fundamentum is used in each group will the species actually exclude each other. A division of the genus *Metazoa* into Diploblastica, Triploblastica and Parasites is clearly illogical and conducive only to confusion; it is proceeding on more than one fundamentum, and is called a cross-division, since the division by one fundamentum cuts across that by another. However, genera may be divided on the different *combinations* of the same set of characters without this error being committed, and distinguished well (logically) if the possible combinations can all be stated.

This does not mean, of course, that the fundamentum must not change throughout a hierarchy. There is no objection to dividing the genus Animal into Unicellulars and Multicellulars and then dividing the genus subalternum Multicellulars into Coelomata and Acoelomata. But the more changes of the fundamentum, the more differentiae must obviously be entered in the definition of any one species infima. Consequently it is better to proceed as far as possible on a single fundamentum. It makes for conciseness and a more direct appreciation of the nature of each species and its relations with others. As is shown later (p. 157), conciseness and clarity of idea presented by a name were specially valued by Linnaeus, and for him the specific Differentia was a name.

These rules of division have had an enormous influence on our classification of organisms. The logical necessity of defining clear-cut discrete groups was a practical necessity in Linnaeus's day (and still is for keys) but has almost no justification theoretically in evolutionary taxonomy even though it retains its importance in badly known groups (Mayr, 1942; Simpson, 1951; Cain, 1953, 1956). How often its prestige has driven the taxonomist to "define" groups which cannot in fact be defined although they are clearly natural (Cain, 1954), or worse, to mutilate his natural groups until each is definable, a study of the whole taxonomic literature would show, and the result would be depressing. One thing is quite obvious, that the overworked taxonomist, required to get out such a classification, will inevitably use the most striking characters as basis for his "definitions" because *they will be the most easily described in words*, and they may then assume a wholly disproportionate importance in his classification. The most striking and easy division will be made first, and by an insidious change of emphasis will be taken to be, first, most fundamental logically, and then in the present age of taxonomy, most fundamental in an evolutionary sense. Often it can be seen that by this last is meant that the particular divergence in question was a great step forward (on whatever definition of evolutionary progress the particular taxonomist fancies) or was very hard to achieve, or took place very long ago, or could not be expected to have happened more than once, or all of these. This is usually the real basis of the assumption that greater overall differences between related forms indicate

a more remote common ancestry, and lesser ones a more recent, as though the evolution of morphological (or any other sort of) diversity goes on at the same rate in all lines of every group. It may be argued that where the fossil history is unknown, this assumption is all we have to go on; the reply is that when one has nothing to go on, it is better not to go, and not to pretend that a classification is phyletic when it is not.

Fortunately, there are correctives which can be applied in practice. Even striking characters must be disregarded if found to vary from one individual to another, or from species to species in a group which on all other characters seems to be natural and not very heterogeneous. Under such circumstances, the taxonomic importance of a character is determined by its covariation with all other characters within a group (Cain 1953, p. 25).

Linnaeus uses division in the logical sense given above. For example, in *Critica Botanica*, Aphorism 285, which states that the generic must precede the specific name, he says " Whatever makes the first division should be given first, consequently the generic is stated before the specific name: before I distinguish something I need to know what is to be distinguished, and so I shall give the genus which is divided into parts by the differentia, before I touch the species". This corresponds exactly with Joseph's second rule of definition (p. 148) and illustrates the close connection between division and definition. What Hort translates as " the classification of the Ancients " is *Veterum divisiones* (*Crit. Bot.*, 246), and a similar use of the term division occurs many times. For example (*Phil. Bot.*, 152) " Arrangement teaches the Divisions or Conjunctions of plants ". Again, the parallel given above between the hierarchy of logical division and of the taxonomic ranks, and the statement (*Phil. Bot.*, 204) that the Order is the (logical) genus of (taxonomic) Genera, but the Class is that of Orders, and the description of Orders as " Classes subalternae " (*Genera Plantarum*, 1764, p. 5), are entirely appropriate to logical division.

Sachs (1875), who has stressed Linnaeus's indebtedness to Cesalpino, says that Cesalpino's ascription of great importance in botanical taxonomy (after the division into woody and non-woody plants) to the fructification is because it subserves the second most important function of plants, namely to reproduce themselves. (The first, connected with presence or absence of woody tissue, is to feed.) As the 1906 translation of Sachs gives it, " Though these parts [the fructification] are only found in the more perfect forms, yet the subdivisions (' posteriora genera ') must be derived in both trees and herbs from likeness and unlikeness in the fructification. And thus Cesalpino was led, not by induction but by the deductive path of pure Aristotelian philosophy, to the conclusion that the principles of a natural classification are to be drawn from the organs of fructification; for which conclusion Linnaeus declared him to be the first of systematists . . . " (What Linnaeus actually called him was *primus verus Systematicus*, the first true systematist (*Phil. Bot.*, para. 54) which is not quite the same thing. The original German (1875, p. 57) gives " ein Satz um deswillen Linné den Caesalpin als den ersten Systematiker seierte ", which is a better rendering of Linnaeus's remark.)

Whatever the theoretical reason given, there is a good practical reason for paying considerable attention to the fructification; it is usually morphologically the most complex organ-system of the plant and provides a large number of characters for study and comparison. Moreover, some at least of its constituents are usually reduplicated a definite number of times (e.g. the five sepals and petals of the common field buttercup, the two long and two short stamens of many labiates etc.). Number is a particularly good attribute for use in a division which is to be as logical as possible. It is easily ascertained, expressed, and understood, and clear-cut if only the integers are needed. Linnaeus analyses

the variation in structure of the fructification into Number, Figure (shape), Proportion, and Situation (*Phil. Bot.*, 92–97 ; *Genera Plantarum*, title-page) in that order. He found it suitable, therefore, for a logical division. At the same time, when the flowers and fruit of two species, in all their peculiarities and complexities of structure, are closely similar, it seems reasonable to believe that the two are closely related, even though their leaves or root or mode of growth may differ. (He himself says (*Phil. Bot.*, 163) that the fructification opens the highroad to the natural method.) Exactly the same procedure is used in would-be evolutionary taxonomy of any group in which the fossil evidence is insufficient ; the guess is made that two forms agreeing in such a large character-complex are most unlikely to have evolved it independently and must therefore be regarded as closely related phyletically.

It would seem, then, that the real justification for Linnaeus's choice of the fructification is the empirical one. But if so it need not be derived from any theoretical considerations such as Cesalpino professed. That Linnaeus did adopt them is suggested by the fact that he declares the fructification to be the true foundation (*fundamentum*) of all Method, without qualification (*Phil. Bot.*, 26), and is therefore using it as a single *fundamentum divisionis* in strict accordance with the third rule of division (p. 150), so he must have regarded it as of prime importance for understanding the essences of plants. However, he had already found it necessary to abandon the primary distinction between woody and non-woody plants since it so obviously separated extremely closely related forms. Svenson (1945) has shown in his discussion of the genus, how soon after Linnaeus the mere weight of empirical evidence caused a similar rejection of the doctrine that all genera were "natural" and distinct. Certainly, at the present day, even the best-known genera can be called natural only in a very restricted sense (Cain, 1956).

Linnaeus was indeed remarkably successful in his application of the principles of logical division to the production of an artificial system—in other words, a key ; and one need only think of the characteristics of a good key to realize that these principles are very suitable for its production. The characters chosen from the fructification were clearly marked, readily appreciated, easily described in words, and usually determinable on herbarium specimens. One tends to forget that for Linnaeus the Class and Order, being indeed only names for genera subalterna at different stages of division, were as applicable to an artificial as to a "natural classification". Nowadays, the ranks of the taxonomic hierarchy are not used (except incidentally) in a key. Linnaeus's own remarks on the Class and Order are that the Class is a large group of genera which agree in important characters, and so that there should not be too many things in one group to be easily distinguished, the Class is subdivided into Orders each of which will contain a manageably small number of Genera ; since it is easy to distinguish ten things from each other, very hard if there are a hundred (*Phil. Bot.*, 160, 161). The Class is therefore less "natural" than the Genus, and the Order less "natural" than the Class (*Phil. Bot.*, 205). This statement must refer to at least partially artificial systems. Nowadays we hope that all the groups in all the ranks of the classification are equally natural, and Linnaeus would have agreed heartily that a natural system in some sense is indeed what one should aim at.

(v) *Gaps between Groups.*

As the result of his use of Logical Division, Linnaeus's groups had to be clear-cut, divided as far as possible upon one fundamentum divisionis, exhaustive, and exclusive. That this was indeed his attitude even to "natural" groups is clearly seen from his treatment of genera and species, which he

expressly declared to be "natural". He insists that one must have clear and separate ideas, genera and names (*Crit. Bot.*, 225; *Species Plantarum*, introduction), that there can be no such thing as an intermediate between genera (*Crit. Bot.*, 224), and that in the definition of genera and species only those characters (in the modern sense) which are truly constant and peculiar can be used (*Crit. Bot.*, 256), which implies that such characters exist for all genera and species.

When the genus is divided, then, the division must be exhaustive. All plane triangles are equilateral, isosceles or scalene in respect of the proportions of their sides, and these terms are so defined that all plane triangles are included in them. But scalene contains triangles differing widely in the proportions of their sides, while equilateral contains only one member. There are in fact two infinities of possible triangles in the species scalene and one in isosceles. An actual continuum of variations is therefore broken up into species of very unequal content. There is, however, no gap between the species, which are contiguous. The only vacant species in this division must be a species of all plane triangles which are neither equilateral, isosceles nor scalene—i.e. a species which cannot be exemplified because its definition is a contradiction in terms.

Now we can see that if flowers have stamens and these are discrete entities, they must have either 1 or 2 or 3 or 4 . . . and so on. It does not follow, however, that actual flowers exemplifying all these classes can be found; in fact Linnaeus had to omit from his sexual system the class of flowers with 11 stamens (*Phil. Bot.*, 68) because it was not known to be exemplified. In such a classification absentees can be detected when all existing forms have been discovered, but their absence can only be noted as an empirical fact. Given the definitions of plane triangle, scalene etc., one can see that no other species of triangles are possible; but given the definitions of flower, stamen, and positive integers, one can see no reason why flowers with any number should or should not exist. The absence of a class does not mean that its definition is internally contradictory, and it tells us nothing about the nature of the gaps between exemplified classes; these may be either parts of a continuum, as in the example of triangles given above, or discrete entities, as are for example the species of the genus Regular Polygon as differentiated by the number of sides.

In the current system of taxonomy the meaning of some gaps, at least, is quite definite. If one species is placed by itself in a monotypic genus and that in a monotypic family, while adjacent families contain many genera with many species, this is interpreted as meaning that the overall differences between the first-named species and its nearest relatives are far greater than those between other closely related species or even genera within the group of all species under consideration, and about as great as those between families. No precise account of what is meant by overall difference or resemblance (i.e. affinity) has yet been produced, and this is one of the principal necessities in taxonomic theory (Cain & Harrison, in preparation). What is clear, however, is that rank in the taxonomic hierarchy is being used as a rough measure of the homogeneity of a particular group, and of the difference between it and the most closely related of known groups, on some sort of subjective scale of similarity. The procedure could easily be extended to other subjects—for example, in the genus Regular Polygon, the overall morphological difference between the members of the genus clearly decreases as the number of sides increases. There is more difference morphologically between an equilateral triangle and a square than between a twenty-sided and a twenty-one-sided regular polygon, and as n rises towards infinity the resemblance to a circle becomes ever more perfect. But in strict Logical Division, such a criterion is clearly useless; it is merely

an overall estimate based on the properties (if the essence is known, or merely on properties and other attributes indiscriminately if it is not) and as such does not give the essence, from which all the properties would follow. To use it would be like working from a description of all the reactions which a given chemical element can take part in, instead of determining its atomic number and other characters, when classifying chemical elements. In fact, it would mean being content with plain description with no attempt at analysis, or nowadays with no attempt at experimental analysis where this is in fact possible; one must constantly remember that for most people at the time of Linnaeus, Logical Division did mean the attempt to find out the real *nature* of things instead of being content with mere superficial appearances; their mistake was to apply it to all subjects instead of restricting it to *a priori* knowledge, and considering the triumphs of mathematics as applied to musical harmony by the Greeks and to mechanics by Galileo, Newton and others, the belief that principles discoverable purely by the use of reason could be found throughout Nature was not unreasonable, at the time.

As far as Linnaeus retained the principles of Logical Division, then, he was in *theory* committing himself to the view that the principles upon which plants were constructed were known, the number and peculiarities of all genera subalterna could be ascertained, and the properties of forms not yet discovered could be prophesied. In practice, of course, no such view could be taken. The most that could be said was that in a system as nearly logical as possible, the place of a species could be determined at once from its own attributes—for example, a species with eleven stamens in order to maintain the symmetry of the system must be put into a different Class from one with ten, when it has been decided beforehand to make the division into classes on the stamens. This makes for efficient identification. But when he tried to work out a "natural" system using overall affinity within the fructification as his basis, it is not surprising that he had the greatest difficulty in defining the "natural" groups by clear-cut characters (in his sense). What we see principally in Linnaeus's system is the conflict between the attempt to base a "natural" system on an analysis of plants by hard thinking according to the best rules available, in other words to create a taxonomy of analysed entities, and the attempt to do so by overall affinities. The bases are different fundamentally, and although the resulting arrangements may coincide to a large extent, it cannot be expected that they should always do so.

(vi) *The Idea of a Natural System.*

The use of the concept *natural* by Linnaeus, therefore, merits further consideration. What in fact did he mean by a "natural" system? It is obviously one that follows Nature. Nature to us nowadays means the world we find and do not make—the opposite of natural is artificial. A devotion to a natural system could mean to us, and often does, an attitude of the purest empiricism. It could hardly do so to Linnaeus. Hawkins (1946), speaking of the philosopher John Scotus Erigena's ideas on Nature, says "Nature here has no narrower signification than reality, but reality, for Erigena, is indeed a nature in the Aristotelian sense, a principle of becoming . . ." In Aristotelian and Thomistic philosophy the word is a technical term of considerable subtlety. Maritain (1946*a*) says "The primary intelligible being of a thing is called *essence* (*essentia*) because since the intellect is modelled on being, what a thing primarily is for the intellect must be that which is of primary importance in it from the standpoint of being itself; in fact . . . it is by and in its *essence* that a thing possesses being or *existence* (*esse*). It is called *quiddity* (*quidditas*) because it is that which the definition expresses and declares, which in turn

answers the question *quid est hoc ? What is this ?* It is called *nature* (*natura*) because it is the first principle of the operations for the performance of which the thing has come into being (*nata*) " and he quotes in support St. Thomas Aquinas (1254) " *Entitas vero rei considerata in ordine ad esse, dicitur* essentia ; *in ordine ad operationem dicitur* natura ". (The being of a thing considered in the order of existence is called essence, in the order of operation it is called nature.)

To appreciate the full subtlety of these remarks and the role of the concepts expressed in them in Aristotelian and Thomistic philosophy a close study of the latter is advisable. Maritain (1946*a*) will serve as a useful introduction, warning us against too simple-minded an interpretation. The relations between the nature and the essence of a subject are not simple—for example, only in the case of pure spirits is the concept of complete essence identical with that of individual nature (p. 158, fn.). But he says (p. 162) " The term *nature* . . . is used in reference to the operations which anything is adapted to perform. A thing, however, does not act solely in accordance with its archetypal or primarily intelligible being but also as it is subject to particular material conditions and possesses a particular individuality. Nothing therefore prevents our diverting the term *nature* from its primary significance to denote secondarily *what a thing is as* individual ". It is easy to see how the mistake could be made of substituting for individual (which is of course meant technically in this quotation) such an entity as one particular species, variety etc., whereupon nature would be treated as well-nigh synonymous with essence. In any case, to understand the nature of a thing is to go far towards determining its essence, which is the first desideratum of Logical Division. That Linnaeus did not confine himself strictly to the philosophical definitions in his use of the idea of the individual is already apparent from his comparison of it with the *species infima* quoted above (p. 150). It is likely that for him a " natural " system was one that showed the *natures* of things, and that natures meant in practice essences.

It is true that Collingwood (1945), for example, considers a great revolution in the idea of Nature (in general) to have begun with Copernicus (1473–1543) and Bruno (1548–1600) and to have gone far by the eighteenth century, in fact to have begun by then to be superseded by the modern outlook, the first signs of which are found in Turgot's *Discours sur l'histoire universelle*, 1750, and Voltaire's *Le Siècle de Louis XIV*, 1751. Linnaeus, however, was still content to use the doctrine of the construction of the flower which was advocated by Cesalpino in the sixteenth century. In this, as Sachs has pointed out (1906, p. 104) empirical findings might have caused him to change his attitude ; but there was nothing to disturb his ideas on the principles of classification that classified things, so reasonably, by their essences and (by a slight extension) their natures. To that extent he was indeed using a natural classification, expressing the principles on which the existence of this subjects depended—in short, in intention at least a taxonomy of analysed entities such as we would like to-day. Sachs's idea of a natural system as against an artificial one, however, appears to be one based on overall affinity. The two are by no means the same ; and, in fact, it is likely that Sachs, in reading Linnaeus, substituted his own for Linnaeus's meaning of "'natural'' and in consequence praised him for being least Aristotelian exactly where he was being most so in intention. Linnaeus's insistence that all characters (in the modern sense) of the fructification should be taken into consideration sounds very like the modern empirical attitude ; but his restriction *on principle* of usable characters to those of the fructification (and his insistence that the habitus could never be used to the neglect of the fructification, *Phil. Bot.*, p. 209) shows the resemblance of his principles to those of Cesalpino. Nevertheless, his statement

(*Classes Plantarum*, 1747, preface) that into a natural Class can be admitted only those plants that are related (*affines*) to each other and agree in their *whole aspect* (*Facies*) and nature suggests that he might at times relax a little from confinement to the fructification. The theoretical basis of his procedure is made perfectly clear and explicit by Ray (1703) who begins his essay "De Methodo Plantarum in genere" with the words *Definitio perfecta conficitur è Genere proximo et Differentia essentiali: At essentiae rerum nobis incognitae sunt, proinde et Differentiae earum essentiales. Verum cum ex eadem essentiâ eaedem qualitates, operationes, aliaque accidentia fluant, non alia certior convenientiae essentialis seu genericae nota esse potest, quàm plura habere attributa communia, seu plures partes et accidentia similia, vel totam faciem, habitum, et texturam eandem.* "A complete Definition is made up out of a Genus proximum and an essential Difference: but the essences of things are unknown to us, and therefore the essential Differences of them also. However, since from the same essences flow the same qualities, operations, and other things which are accidents, there can be no surer mark of essential, and so of generic, agreement than to have many common attributes, that is, many parts and accidents similar, or to have the whole facies, habit, and structure the same." This passage shows that Ray was clearly aware of the difficulty of constructing a taxonomy of analysed entities for plants (and of course equally for animals) by the use of Logical Division and proposed to overcome it by using overall affinity as a clue to essential similarity or difference. Linnaeus did the same, but since the fructification must be of the first importance on purely Aristotelian principles, he had to confine himself in the first place to overall affinity as shown by it.

Memory.

(i) *Requirement and Consequences.*

Linnaeus states explicitly and repeatedly that the botanist (and, since his rules apply equally to all kingdoms of Nature (*Crit. Bot.*, preface), the zoologist too) must know all genera, and commit their names to memory (e.g. *Crit. Bot.*, aphorism 213, 218, 251; *Phil. Bot.*, 256) but cannot be expected to remember all specific names (*Crit. Bot.*, 213; *Phil. Bot.*, 256). I think the practical reason for this requirement is fairly evident. Species in the aggregate were too numerous to be remembered and in detail often hard to determine and define clearly. The largest groups often varied greatly in the works of different botanists who had based their systems on different attributes, e.g. on the shape of the leaf (de l'Obel) the corolla (Rivinus) the calyx (Magnol) or, as far as was then possible, on general affinity (Ray). Consequently there was the greatest difficulty in comparing the system of one botanist with that of another, since the constituent groups would differ in name and contents and the identification of the same species in different systems would not always be easy, especially since the same species would acquire different differentiae as it was moved from genus to genus. However, there was some hope of general agreement on genera, which, if they could be defined clearly and given distinct names, could be treated as the practical units of classification. Whatever system was employed, the genera could stay the same (*Crit. Bot.*, 216) and be arranged in larger groups according to what particular criteria were chosen. Then neither the names of the genera would be changed according to the system used (*Crit. Bot.*, 216) nor would their definitions (*Phil. Bot.*, 202), and the concept of each genus would not become confused and indistinct in the mind (*Crit. Bot.*, 216, 224, 225), all of which things Linnaeus explicitly said were to be avoided. Consequently, there would be far less burden on the memory, and far greater stability in nomenclature. The genera and their names being fixed, comparison of the literature would be simplified without either pinning the botanist down

to one system of classification or requiring a firm decision on every species before any comparison could be made. Moreover, as Joseph (1916, p. 87) remarks, the genus is often presented to us (after we have become acquainted with some individuals) before it can be defined. " . . . the use of general names shows that some apprehension of their common nature comes to us from the beginning along with an experience of individuals; only we may long remain unable or not endeavour, to formulate it." The genus, therefore, is to that extent a suitable entity to expect agreement on.

As consequences it followed that genera must be, as Linnaeus said,

(i) " natural " (so that there would be general agreement on them);
(ii) the smallest groups above species;
(iii) distinct (so that every species would fall clearly into place in one or other, with no untidy intermediates and unplaced forms. This is a requirement also of Logical Division).

Moreover, every generic name, to be efficient as a mnemonic should be

(i) univerbal (to be as concise as possible);
(ii) distinct, not compounded out of other generic names nor modifications of them (which would suggest indistinct and intermediate groups);
(iii) literally meaningful, not meaningless and therefore hard to retain in the memory;
(iv) expressing (if possible, but Linnaeus recognized how seldom this rule could be obeyed) the peculiar characteristic of each genus, so that the name would also be a diagnosis: or at all events,
(v) not contradicted by the features of any species within the genus (or people would think, on arriving at it, that they had made a mistake in identification).

It was not long before the weight of empirical evidence made it impossible to maintain most of these rules; at present, the use of one word only for a generic name is still in force, and for the same reason, but the rest have gone. It is interesting to note one other survival. As the generic name was a single word referring to a kind of thing, and therefore requiring a definition, but the specific name was a phrase acting as a differentia, Linnaeus was quite right in forbidding tautologous names. Such a name as *Troglodytes troglodytes* was not a name at all to him (or only a trivial name) since it was a mere repetition of the generic name with no differentia; under no conceivable circumstances whatever could the name of a genus be also the differentia of one of its species. His (rational) dislike of tautologies is still to be met with in botanical nomenclature, where, however, it is applied to the trivial name in which it does not matter in the least and I find in conversation that this taxonomic Malapropism is justified on " aesthetic " grounds.

(ii) *Natural Genera.*

Linnaeus expressed his opinion on the " naturalness " of genera with considerable emphasis. " Genera, on the other hand [in contrast to species] are as many as there are, of distinct species, common proximal attributes [i.e. nearest in the ranks of Logical Division to the attributes of particular species] according to which they were created in the beginning: this is confirmed by revelation, discovery, observation. Hence *All Genera are natural* . . . And so we must by attentive and careful observation inquire into the limits of Genera, since *a priori* they are determined with greater difficulty, although this is the task which must be undertaken for *the confusion of genera brings inevitably the confusion of everything* (Caesalpinus) . . . And so I urge all on sensible

Botanists as a thing to be recognized if any certainty in the art is to be desired, that all *Genera and Species are natural*, without the adoption of which principle no soundness in the art can be achieved." (*Genera Plantarum*, pp. II, IV–V.)

I suggest that this principle which must be adopted (*assumto*) is in reality an assumption in the modern sense, which is made desirable by the sound practical reasons given above, and that revelation is brought in to confirm it, as something of a rationalization. He repeated many times the statement (quoted from Cesalpino) that if genera are confused all is confused (e.g. *Genera Plantarum*, introduction ; *Crit. Bot.*, 225 ; *Phil. Bot.*, 159). Even a glance at the works of some of his predecessors is enough to show the difficulties he and they must have had when comparing the systems, groups and definitions of different authors, and to evoke the liveliest sympathy for his earnest desire to do something practical about it. Nevertheless the problem was primarily a practical one and his solution can hardly be said to be confirmed by revelation. It may well have been by experience.

(iii) *Monotypic Genera.*

From the quotation given just above, it appears that for Linnaeus each genus differs in (at least) one particular attribute from its nearest relatives (in accordance with Logical Division) and must be the smallest group of species. Genera, in fact, are the Genera proxima of Logical Division. This must not be taken to mean that single species can be split off from a genus and made into separate genera on the basis of their *specific* characteristics ; we must not agree with those "who think that they can divide a genus with the best of reasons in whatever way they like when they have discovered the smallest characteristic, the cause of their false report". (*Crit. Bot.*, 215). His exact words for the last phrase are "*dum minimam investigarunt notam falsi testimonii caussa*". Such a *nota* (character in the modern sense) might of course be characteristic only of that particular species. He fully agreed that there are indeed genera containing only a single species each (*Phil. Bot.*, 203). As a consequence, since the specific name was a differentia, he maintained with perfect consistency that a species which is the only one in its genus cannot have a specific name (e.g. *Genera Plantarum*, Ratio Operis, 4). How can one possibly know whether another species will be discovered, and when it is what its characteristics will be ? But no true differentia can be given to either, until these are known (*Crit. Bot.*, 293).

How then can one distinguish between a merely specific attribute and one which, although carried so far as is known only by one species is nevertheless sufficient to require a generic distinction ? Unless some criterion can be found there can be no certainty. This is a difficult question and it is not surprising that Linnaeus's views on it are not too clearly stated. One possible solution would be to take one particular structure *a priori* and make the variations in it of generic importance, in the same way as within his artificial system the Classes are taken from the stamens and the Orders from the position of the ovary. Difference in fructification is his criterion for the foundation (*fundamentum*) of genera (*Phil. Bot.*, 98). But even then, he says that since there are variations in some parts of the fructification at least, in most genera, it would be easy to arrive at a state in which there were as many genera as species (*Phil. Bot.*, 170, 175). Some more modern systematists (e.g. G. M. Mathews in his *Birds of Australia*) have well-nigh reached this state. Linnaeus's own words are merely (*Phil. Bot.*, 159) that "We say there are as many genera as there are similarly constructed fructifications revealed by diverse natural species" . . Ortega (1792), as Svenson points out, had already realized and stated the consequences of this remark, namely, that, unless there are more species than one in a genus it is

not possible to know what are generic and what specific attributes, and that Linnaeus was on dangerous ground when asserting the existence of monotypic genera.

Linnaeus himself said that a definition of a genus (*character*) was not infallible until it had been checked on all the species (*Phil. Bot.*, 193), that the greater the number of species in which a particular character (*nota*) was found to be constant, the more likely it was to be of generic value (*Phil. Bot.*, 169), that the *habitus* could be used to check the "naturalness" of a genus (*Phil. Bot.*, 168) but never in place of or to the neglect of the fructification (*Phil. Bot.*, 209), that although variation occurred in the fructification in most genera all the parts did not vary at once (*Phil. Bot.*, 170) and that if the flowers agree, a species should not be split off from the genus on differences in the fruit (*Phil. Bot.*, 177). All this suggests strongly that he was in fact recognizing "natural" groups, as far as possible, by means of the character-complex (Cain, 1953, p. 25; 1954), in which the importance of characters is determined by their covariation with all other characters in the complex. Then no single character within the complex is of magistral importance, and characters otherwise present throughout may be absent in one or other species, which is not separated from the genus on that account because all the other characters in the complex clearly tie it to that genus. This is a feature of a natural classification in the modern sense, and it is not surprising, therefore, that his practice should be so similar to the modern (although his principles were so different, p. 154) as to mislead the far from uncritical Sachs over his ideas of the natural classification.

This interpretation is confirmed by a letter of the younger Linnaeus, my knowledge and the following translation of which I owe to the kindness of Dr. Arvid H. Uggla of Uppsala. The letter, dated 21 April 1778, is to Dr. Abraham Baeck, who was preparing his oration in memory of Linnaeus, to be given before the Academy of Science in Stockholm, and had asked certain questions about the elder Linnaeus's ideas. The passage in question is as follows. Latin words in parenthesis stand so in the letter instead of Swedish words.

"My Father's secret art of determining (delimiting) genera in such a way that Species should not become genera? This was no other than his practice in knowing a plant from its external appearance (externa facie). Therefore he often deviated from his own principles in such a way that variation as to the number of parts (numero partium) did not disturb him, if only the character of the genus (character generis) could be preserved. Foreigners don't do so, but as soon as a plant has a different splitting (cleavage) of the corolla and calyx, or if the number of stamens and pistils (numerus staminum et pistillorum) varies, they make a new genus. This is what Aublet, Forster and others have done. If possible he (Linnaeus) tried to build the character genericus on the cleavage of the fruit so that all species that constitute a genus should have the same shape of their fruit.

"I have accepted the same principle in my *Mantissa*. In this way some among Forster's genera will have to suffer, which I have told him. Maybe this will not please him, which might explain why for some time I have had no letters from him."

As a result of this procedure Linnaeus had arrived at or retained a large number of natural groups, many of which were clearly marked off from each other, and each of which was a close-knit group of several species. When he came across a species that was as distinct from these groups as they were from each other, it would seem that he placed it in a separate genus to mark the degree of its difference, by a simple extension of his method for avoiding the complete fragmentation of genera. This, as mentioned above, is the modern procedure in which the taxonomic hierarchy above the species is used as a rough indication

of the homogeneity of the groups (and so of the gaps between subgroups) but it is a departure from strict Logical Division.

Here again is an instance of the conflict in the Linnaean system between his adherence to the principles of Logical Division—because they were in so many ways so reasonable, so much common sense—and his constant attempts to recognize natural groupings on overall affinity. Another example is his insistence that some characters (colour, size, taste, etc.) are never of specific value. Granted that they are often demonstrably variable within many if not most species, it does not follow that any of them are always utterly worthless, nor in fact did he so treat them always in practice. But obviously in Logical Division each species must be marked off by some essential difference and this clearly cannot vary within the species. Consequently he was bound to reject in theory all characters known to vary conspicuously, and bound to overlook the fact that if one defines species as "breeding true" and demands also that each species shall have its characteristic difference, one may be led into difficulties. Sibling species are precisely those which have only trivial morphological differences but do not interbreed to any extent in the wild, and are, biologically speaking, good species. Linnaeus's own principles inevitably blinded him to the possibility that such should exist, and indeed it remained unrecognized for a long time afterwards.

(iv) *Distinctness of Genera.*

That taxonomic genera should be distinct follows immediately from the use made by Linnaeus of Logical Division, in which they are merely one example of genera subalterna. The requirement was, however, strongly reinforced in practice by the necessity of memorizing them. Many times he says that we must have distinct names, distinct genera, distinct concepts, or all will be confusion. The limits of genera will only be known, he says, when all the species are discovered, and similarly those of classes when all the genera are known (*Phil. Bot.*, 160). This is obvious for the system based on overall affinity, but he does not restrict the application of the last remark explicitly to natural classes. However, as artificial classes are stated to be only succedanea for natural ones (*Phil. Bot.*, 160; *Genera Plantarum*, p. 6) the application seems reasonable. When all species are known, it will then be evident what characteristics are only specific or common to a few species within a natural genus, and what are truly generic.

But this implies either (*a*) that there are diagnostic characters of all genera, or (*b*) that there are not, but it will always be found that natural groups (recognized by their character-complexes, not by single diagnostic characters) are separated by clear gaps from their nearest relatives. The second implication, as already mentioned, probably did not occur to Linnaeus, and he would have subscribed to the first. But in the present evolutionary taxonomy, it is possible that distinct genera will be found to change imperceptibly as one passes backwards in time, with no obvious breaks or gaps until they merge into one another. It is also perfectly possible that between two close-knit clusters of species at the present day there should be not a clear gap but one sprinkled with species intermediate in character. In fact the situation is not uncommon. Since there is no law quantifying the angles of divergence of phyletic lines (Cain, 1956) and very good reason to suppose that none will be discovered, the Linnaean insistence on the distinctness of genera cannot possibly be maintained. The genus is, in fact, a much more dubious entity than the species. But we still follow the practice, which derives entirely from Logical Division, of insisting that every species must be included in one and only one genus. No provision for recognizing intermediates is made.

Bases of the Linnaean System.

As a result of even this brief and incomplete survey of the principal features of Linnaeus's taxonomy it can be seen that much of it is attributable to three things, namely, the principles of Logical Division, the insistence that all genera must be memorised, and of course the actual empirical evidence obtained from the study of plants, which was sometimes in serious conflict with the requirements of the other two. The feeling for overall affinity and presumably the hope of attaining to a system which would reveal the natures of plants, drove him to work at his great desideratum, the natural system. He first, according to Sachs, saw clearly that no natural system could be based on *a priori* principles. As explained above (p. 155) I think this is incorrect or rather that if Linnaeus did come to that conclusion in practice, his practice was a little ahead of his own theory. Even in genera, where he was expressly concerned with the establishing of natural groups, he insisted (with some slight modifications, perhaps, in practice) that the generic distinctions must be based on the fructification, for reasons which were purely Aristotelian in theory. In a truly empirical system there is no such must; no one part can be pointed to as *a priori* more important than any other. If in fact genera are still defined on the fructification, it must be either because, as suggested above, that provides the largest character-complex in the plant, and so is likely to be the best basis for an estimate of overall affinity, or perhaps, in some groups, because of strong conservatism.

It is clear that in its theoretical basis, the evolutionary taxonomy of the present day shares with Linnaeus's taxonomy only the binominal nomenclature as a reference system (which is now often an embarrassment since it places so much emphasis on genera) and the names Class, Order, Genus, Species and Variety as rank-names in the hierarchy, and perhaps the concept of the species as "breeding true", which is applicable only to bisexual forms. Whatever else it shares is likely to be a remnant from the principles of Logical Division, retained only because they can still be made to apply to badly-known groups and because taxonomists in general are too overworked to have time to study the theory of taxonomy. It is more than doubtful whether we should call the present system of taxonomy Linnaean at all.

If we are to have a truly scientific taxonomy, we must first ask whether classification *per genus et differentiam* is applicable to evolutionary continua at all and, if so, whether in itself it causes any bias in the presentation of the data. We must also ask whether the binominal nomenclature, which is a direct product of this mode of classification is so firmly established in the literature as to make a fundamental reform impossible although desirable.

But, more important still, we must ask how far the principles of Logical Division, whether explicitly recognized as such or not, are still in use in present-day taxonomy, especially in those groups with little or no known fossil history. It is frequently said—indeed I have said it myself (Cain 1953. p. 18)—that when the theory of descent with modification was established all that taxonomists had to do was to equate close affinity with descent from a proximate common ancestor, less close affinity with descent from a more distant one and carry on as before. Consequently Darwin could take the classification as produced by non-evolutionary taxonomists and point out the complete concordance between it and the consequences of evolutionary theory. This was no doubt true for well-known groups in which overall affinities had been carefully worked out (and nowadays for groups in which the fossil history is very completely known). But in groups with little or no fossil history especially in those with very large numbers of closely related forms whose affinities seem to make up a veritable reticulum the classification adopted may often depend

very much on just what characters are regarded of primary taxonomic importance, i.e. what is taken as *fundamentum divisionis* for the first division; indeed a change of emphasis from one class of characters to another may involve a complete reclassification of the group. In such circumstances, it is essential that the taxonomist should rid his mind of all preconceived notions of what "must" be more primitive, or more important in any other theoretical way, and adopt the purely empirical approach, beginning with species or taxa at and around this rank, and grouping them into higher and higher natural groups using all possible classes of evidence. There is no "must" for any particular sort of character whatever (p. 161). The result will be not only a salutary exercise in intellectual humility for the taxonomist concerned, but an arrangement free from individual quirks of theory, and therefore the most useful and stable.

Summary.

1. The principles on which Linnaeus based his taxonomy are shown to be, at least in intention, primarily Aristotelian.

2. He, like others, was trying to produce a classification founded on the real natures of organisms, not on superficial resemblance; the method he adopted was to classify by the rules of Logical Division, which involve the determination of the essence of each entity.

3. Consequently his idea of a natural system was not that in use at the present; certain comments by historians of science are therefore incorrect.

4. A distinction is made between taxonomies of analysed and of unanalysed entities. Logical Division is appropriate only to the former.

5. Linnaeus's classificatory unit was the genus, not the species. His principles led him to assert that all genera must be distinct and natural; moreover, since they were stable units, however much the higher groups of a classification changed from author to author, he insisted that their names must be memorized.

6. Most of his rules of nomenclature follow directly from this requirement that the botanist must know and remember all genera.

7. The increase in knowledge of plants since Cesalpino had already led to the breakdown of at least one important theoretical grouping; in Linnaeus, the conflict between theoretical requirements and actual facts is very evident. Because of it, his practice often approached the modern when his theory did not.

8. It is suggested that the principles of Logical Division, unrecognized as such, may still be a powerful source of unwarranted bias in modern classifications.

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References.

Aquinas, T. (Saint). 1254. *De Ente et Essentia.*

Cain, A. J. 1953. *Animal Species and their Evolution.* London, pp. i–x, 11–190.

——. 1954. Subdivisions of the genus *Ptilinopus* (Aves, Columbae). *Bull. Brit. Mus. (N.H.) Zool.*, 2, 265–284.

——. 1956. The genus in evolutionary taxonomy. *Syst. Zool.*, 5, 97–109.

Candolle, A. P. de. 1813. *Théorie élémentaire de la Botanique* etc. Paris, pp. i–viii, 1–500 (+28 pp. index).

Cesalpino, A. 1583. *De plantis libri XIV.* Florence.

Collingwood, R. G. 1945. *The Idea of Nature.* Oxford, pp. i–viii, 1–183.

FRIES, E. 1835. *Corpus florarum provincialium Sueciae.* Uppsala, 1, pp. I–XXIV, 1–394.
HAWKINS, D. J. B. 1946. *A Sketch of Mediaeval Philosophy.* London, pp. i–viii, 9–148.
HOPWOOD, A. T. 1950. Animal classification from Linnaeus to Darwin. In *Lectures on the Development of Taxonomy*, Linnean Society of London publication, pp. 46–59.
HORT, A. 1938. *The Critica Botanica of Linnaeus.* London, Ray Society, pp. i–xxvii, 1–239.
JOSEPH, H. W. B. 1916. *An Introduction to Logic.* Ed. 2, Oxford, pp. i–xii, 1–608.
LINNAEUS, C. 1737. *Critica botanica.* Leyden, pp. 1–270 (+ 44 pp. indexes and errata).
——. 1747. *Classes Plantarum.* Halae Magdeburgicae, pp. i–viii, 1–656.
——. 1753. *Species plantarum.* Stockholm (reprinted Tokyo 1934), 1, 12 pp. introduction etc. + 1–560.
——. 1759. *Systema naturae.* Stockholm, Ed. 10 (reprinted, Leipzig 1894), 1, 1–824.
——. 1764. *Genera plantarum.* Ed. 6. Stockholm, pp. i–xx, 1–580 (+ 44 pp. indexes etc.).
——. 1770. *Philosophia botanica.* Vienna (editio altera), pp. 2 + 1–364, 11 pls.
MARITAIN, J. 1946*a*. *An Introduction to Philosophy*, tr. E. I. Watkin. London, pp. i–xvi, 17–207.
——. 1946*b*. *An Introduction to Logic*, tr. I. C. London, pp. i–xii, 1–300.
MAYR, E. 1942. *Systematics and the Origin of Species.* New York, pp. i–xiv, 1–334.
ORTEGA, C. G. 1792. *Caroli Linnaei, Botanicorum principis, Philosophia botanica* etc. Madrid, pp. i–viii, 1–426.
RAMSBOTTOM, J. 1938. Linnaeus and the species concept. *Proc. Linn. Soc. Lond.*, 150 (1937–38), 192–219.
RAY, J. 1703. *Methodus plantarum emendata.* Amsterdam, pp. (34 prefaces), 1–102 (+ 26 pp. indexes).
SACHS, J. VON. 1875. *Geschichte der Botanik vom 16. Jahrhundert bis 1860.* München. Authorised translation by Garnsey, H. E. F., & Balfour, I. B. 2nd impression, 1906. Oxford, pp. i–xv, 1–568.
SCHMIDT, K. P. 1952. The " Methodus " of Linnaeus, 1736. *J. Soc. Bibl. nat. Hist.*, 2, 369–374.
SIMPSON, G. G. 1951. The species concept. *Evolution*, 5, 285–298.
SVENSON, H. 1945. On the descriptive method of Linnaeus. *Rhodora*, 47 (562) : 273–302 and (563), 363–388.

GENETIC STABILITY IN THE SNAIL CEPAEA NEMORALIS [L.] : A FURTHER EXAMPLE

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(With 1 text-figure.)

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In a previous paper (Goodhart, 1956) a comparison was made between some samples of the polymorphic land-snail *Cepaea nemoralis* (L.) collected by the late Mr. H. H. Brindley in 1906 and 1911 from small carefully defined areas, and further samples from the same localities taken in 1953. In some of the colonies the percentages of different shell colour and banding pattern morphs had remained remarkably constant over the 40-year period between the samples. On p. 58 of that paper a further collection, made by Mr. Brindley in 1892, was mentioned but its exact locality had not been identified. A reference has now been found (Brindley, 1904) which does make this clear, and further samples were collected from the area in 1956. Once again there is evidence of considerable genetical stability and there are other features of interest, as well as the long period covered by the observations, that may be worth recording.

The relevant passages from Brindley's article, on p. 122 of the *Local Natura-*