

CLASSIFICATION AND PHYLOGENY*

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ABSTRACT

A scientific study of classification of plants started with Linnaeus when in 1753 he published his monumental 'Species Plantarum' that made him the father of systematic botany. Linnaeus' system of classification was an artificial one, based as it was on some arbitrarily selected characters. He himself regarded it as "one of convenience until the time when a Natural System could take its place". Subsequently, however, classification began to be based on general resemblances and differences and the species came to be regarded as a concept rather than a fixed entity. Such a classification came to be known as a 'natural', 'logical' or more recently 'general' classification.

It is emphasized that in the present state of our knowledge it is almost impossible to trace any group phylogenies. Even if we are able to determine them it will be very difficult to incorporate them in our classificatory schemes. It is therefore concluded that the main task of the systematist should be "to make a general classification which shall express as far as possible in rational order all that is known concerning plants and animals. This ideal which, even if never attained, is one which may still make the systematist proud in the magnitude of his task. It is an ideal greater than the phylogenetic ideal which is included in it and one which in the process of attempted attainment must make taxonomy what it should be, the focal point of biology".

Ever since man became interested in things around him he has tried to make his experiences more and more intelligible by discovering some rational order out of the apparent chaos. Whether it be the nature around him—the plants and the animal, the rocks and the minerals, the stars and the planets—or the people with whom he lives—their customs and traditions, their foods and dresses, their governments and religions—the way to understand them has been the way to arrange them in some supposedly rational order which we call classification.

A scientific study of classification of plants, however, started with Linnaeus when in 1753 he published his monumental 'Species Plantarum' that made him the father of systematic botany. Linnaean system of classification was an artificial one, based as it was on some arbitrarily selected characters. He himself regarded it as "one of convenience until the time when a Natural System could take its place." (see Turrill, 1942, p. 256). Subsequently, however, classification began to be based on general resemblances and differences and the species came to be regarded as a concept rather than a fixed entity. Such a classification came to be known as a 'natural', 'logical' or more recently 'general' classification.

For an adequate appraisal of the angiosperm classification it is necessary to take into account its historical background and the climate under which it developed. As is well known, Europe is the birth place of our present biological systems of classification. The vegetation of this part of the world is by no means rich and the naturalists of those days had limited access—of course in the present day sense—to the floras of other places. Besides, the

'type' concept in the allied field of morphology was at its height in this period and it did affect classification (witness Lindley's magnificent expositions 1830, 1853).

All this determined the shape and form—the limits of species and their groupings into higher categories—of the classification that we have. Obviously it has many short-comings. If the Linnaeus of today were to propose an entirely new system, say at Kew, New York or Paris, the result will be very different although this too shall not be universally acceptable. So we have little justification to grumble or to exaggerate the short-comings of our heritage that is indeed very valuable.

Classification of angiosperms with which we are concerned here is based on the principle that correspondence in structure determines affinity. Those plants that show the greatest morphological correspondence and greatest number of correlations are close to one another while those that show the least correspondence are farthest apart. Following this principle plants have been recognized as constituting species that are in turn grouped into genera, families, orders, etc. with a view to bringing order into chaos of numbers. Each species, genus, tribe, family, etc., stands nearest to those which it "most resembles in all respects, or rather in the whole plan of structure." (Gray, 1880, p. 331). This has been called a natural system for the simple reason that it seeks to express as perfectly as possible the plan of nature underlying the various categories. It is true that the plan of nature is fixed and unalterable but it is also true that it can be looked at from a number of different angles much in the same way as the facts of history can be narrated in different combinations by different authors (c.f. Lindley, 1853).

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So there can be more than one natural system. We are interested in one such system and in making it as broad-based and comprehensive as possible, so that it may be useful in a variety of ways. Besides, it should epitomize simply and conveniently all those empirically discovered correlations and similarities on which it is based and should enable predication to be made about individuals subsequently discovered (Gilmour, 1940).

Considered in this broad sense it is not antagonistic to, or exclusive of, the so-called phylogenetic system. Rather in its ultimate form, if and when it is obtained, it will automatically reflect phylogenies as also information about any other aspect of plants.

Purpose is the back-bone of any classification; rather it is the nucleus around which a classification develops. The purpose of our natural classification has been to lay bear as best as possible the plan of nature underlying the various categories which man has recognised for his convenience. In the post-evolutionary era this abstract purpose has often been confused with phylogeny and so the prefix 'natural' seems to have lost its original hue. It has been recently substituted by 'general' (Turrill, 1942; Gilmour, 1961; see also Lorch, 1961) which seems to be more appropriate in so far as it emphasizes the 'general' purpose of our classification as against 'special'.

Attention must be drawn to one practical difficulty at this stage. All the workers in descriptive fields of botany appear to be working in the interest of taxonomist; at least they quite often profess their objective as providing new and additional data to him for his arduous work. The poor taxonomist with all the best intentions is unable to incorporate all this data into his scheme. After all there is limit to pouring 'new wine into old bottles'. An easy way out has been suggested a number of times (see Gilmour, 1961). Specialists in major fields of descriptive botany, e.g. anatomy, morphology, genetics, ecology, etc. may have their own special classifications of groups, they are dealing with, wherein they can incorporate the details of their studies. Major contributions of these special (artificial) classifications may, when necessary be incorporated in the general system so that the latter may ultimately become an "epitome of our knowledge of plants" (Gray, 1880, p. 315).

Evolutionary era ushered by the publication of the 'Origin of Species' in 1859 brought about a revolution in classification though in a very imperceptible manner. The classical taxonomy*, like the classical morphology, was transformed into evolutionary biology and the 'archetype' or the 'ground plan' of the systematist was replaced, as if by a feat of legerdemain, by 'ancestral' plant. Characters so far attributed to a mental concept of 'type' were

now freely applied to "an actual, historically existent ancestor" (Arber, 1950), without understanding the implications of this transformation. It must be realized that such forms, with *all primitive* or *all advanced* characters, can live only *in mind* and not *in nature*. For an independent living, all forms must have some sort of blending or balancing of primitive and advanced characters. Without this they cannot live, a point so ably emphasized by Meyer-abich (1942). So the 'archetypal' plant can not be identified with 'ancestral' plant.

This was a murder of classical taxonomy, yet its practitioners did not realize it. They were so overwhelmed by the doctrine of evolution that they readily allowed themselves to be swept away by the current of phylogeny. In fact they made it as their main object, and some even went so far as to suggest that phylogeny gives taxonomy "vitality and elevates it to the dignity of a science" (White, 1940).

One important factor responsible for the ease with which this transformation took place was the borrowing of the terminology of the classical taxonomy and giving it an evolutionary context. Darwin (1859) himself wrote that "Community of descent is the hidden bond which Naturalists have been unconsciously seeking." One word which has been most significant in this connection is 'affinity'. In taxonomic literature of the pre-Darwinian era it always meant that 'one thing resembles another in the principal part of its structure' (Lindley and Moore, 1876). In the literature of this period it is freely used as synonymous with 'similarity' or 'resemblance'. Linnaeus himself is said to have expressed the opinion that "Plants show affinities on all sides like the district of a geographical map" (see Turrill, 1942). Used in this context the word affinity cannot mean genetical relationships. Since 'affinity' in common language also means 'blood relationship' (see Webster's International Dictionary 1957), 'similarities' and 'resemblances' in context of evolution began to mean 'relationships', 'real relationships' or 'phylogenetic relationships'. So the classification that was uptil now based on resemblances now began to be considered as if based** on relationships or phylogeny.

In the same way such terms as 'simple' and 'complex' were freely identified with 'primitive' and 'advanced' and finally into 'ancient' and 'recent' respectively. In pre-Darwinian period 'type' concept was very prevalent in plant morphology. In taxonomic literature also there is frequent mention of 'fundamental type' or 'archetype' or 'ground-plan' for each natural group or category from which all the different forms can be mentally derived through

* As against phylogenetic taxonomy.

** Actually speaking classification is not based on phylogenetic relationships. Rather these latter are read into the former as second thought and the classification is certified as a phylogenetic one. At best it is just one *made possible* through the influence of phylogeny (See Gilmour, 1961).

such mental processes as 'reduction', 'suppression', 'fusion', etc. These expressions are always used metaphorically and never literally. But in the evolutionary era that followed, this sort of derivation at once became synonymous with 'origin' in the literal sense and the archetype was given a historical existence and such expressions as 'reduction', 'suppression', 'fusion' began to mean literally so.

It is not intended to imply here that this transformation of classical terminology into phylogenetic terminology was in any way deliberately attempted. Rather it was caused more by the imperfection of our language than anything else, the uncritical author not being careful enough to specify what he actually meant.

Phylogeny is a natural corollary of evolution and is defined as "the history of the origin and development of taxonomic groups" (Gilmour, 1940). It would have been very desirable if we could express it somehow in our classification. For, it would have given it a more rational basis* and consequently a much wider appeal. Besides, it would have been a great guiding principle to workers in different fields of botanical activity. But is it at all possible to determine phylogeny in angiosperms in a logical manner? Theoretically it should be, provided we have all the historical facts, fossil and extent, at our disposal. It is common knowledge that we have no fossil records whatsoever concerning angiosperms. So the origin and evolution of this group still continues to be the same "abominable mystery" that it was more than a hundred years ago. In fact, Tutin (1952) goes so far as to assert "that no more is known now about the origin of any major group of plants than was known in 1859." We have therefore no direct basis for determining phylogenies of any taxon. This is a basic fact and there are no two opinions about it. Leave aside phylogenies we have no direct means of determining even genealogies of individuals which should comparatively be much simpler. Those who think otherwise are obviously working under an illusion of identifying group phylogenies with genealogy of a human babe (c.f. Gilmour, 1961).

The only other alternative, therefore, is to derive or infer phylogenies from the available morphological data. Is this possible? There is considerable difference of opinion on this point. One school of thought believes that phylogenies cannot be legitimately inferred in the absence of fossil record. For determining these we have to know the 'how', 'when' and 'where' of the origin of the taxon in question. Unlike the origin of a human babe the

members of a Taxon may arise (1) in one or more genealogical lineages, (2) at one or more times and (3) in one or more places. So long as we do not have dependable information on all these points—and we have to admit frankly we have none—our so-called phylogenies are bound to be based on mere assumptions and speculations (c.f. Gilmour, 1961). According to the advocates of this school many phylogenetic arguments, therefore, appear to run in a circle—a group is primitive because its members have primitive characters, while the characters are primitive because they appear in a primitive group.

The other school of thought believes that classification, to be worth its name, must somehow express phylogeny. The exponents of this view have employed all types of dubious methods for determining phylogenies. Usually they consider morphic similarities as a safe criterion for phyletic relationships. As indicated earlier this is not a dependable basis. It is true that closeness of relationship is one of the most important factors causing similarity of attributes, but it is also true that similarity is not an exact measure of relationship (c.f. Gilmour, 1940). An oft-quoted genetic example should bring this fact readily to mind: two cousins may *resemble* one another more than two brothers, and yet the latter are more closely *related* to one another than the former. Thus similarities cannot ever be synonymous with relationships.

Sporne (1959), while freely admitting that no where "can genealogical trees (organism phylogenies) be built up with any degree of confidence and whether this will always be so remains to be seen", has devoted considerable attention to the problem of determining the course of descent in different groups by some indirect means. His main thesis is that "evolution has proceeded at different rates in different lines with the result that in the present-day flora of the world some individuals are more highly evolved than others, i.e. some are advanced and some 'primitive'". This concept of relative advancement that is open to serious objection (see Bremekamp, 1939), deserves, according to him, a prominent place in our classification. For determining this he depends upon individual characters which he distinguishes into primitive and advanced, an approach too much simplified to be effective (see Stebbins, 1951). A primitive character is defined as one "which, possessed by a present-day taxon, was also possessed by its ancestors". Here Dr. Sporne appears to be arguing in a circle—primitive character is that which was possessed by an ancestor; an ancestor is one that possessed primitive characters. And how can we determine the rate of evolution of a taxon without knowing the point of its origin? Obviously there are some inherent assumptions in this line of approach.

Sporne (1959) himself admits that all the various methods which are used in determining primitive

* In this connection an alternative suggested by Bather (1931) deserves better attention from botanists. He writes, "In all cases, from species to phyla, material originally indifferent or heterogeneous has been integrated into classifiable groups by environment. It is, no less than descent, worthy of consideration, as a basis of rational classification"

characters and which he has discussed so critically and ably—the so-called doctrines of conservative regions, 'recapitulation', 'teratology', 'sequence', 'association', 'basic ground plan', and correlation—are "open to criticism; some more than others". The first five of these are themselves based on certain assumption regarding the course of evolution, a matter which they in turn seek to determine. The methods of 'basic ground plan' and of 'correlation' are regarded as of some use. Let us consider them in some detail.

The idea of 'basic ground plan' is borrowed from classical taxonomy, where it is just a mental concept of a hypothetical individual with which all the existing forms are compared to determine their relative positions. They can be 'derived' from it by imagining the play upon it of such processes as 'reduction',

'adnation' or 'cohesion' of parts. None of these, including the basic ground plan, has any physical existence in classical taxonomy. But the modern phylogenists, in their frantic efforts to find out some basis for their speculation, catch hold of the basic ground plan as a portrait of the ancestor of the group, give it a historical existence and make it produce, as it were, the present-day forms through such physical processes as reduction, suppression or fusion, using them literally. Those that are nearest to the ancestor are primitive and those farthest apart, the most advanced. Some of the results obtained this way appear to be correct but as pointed out by Good (1956, p. 371), "they are often no more than intelligent guesses or if true no more than fortunate choice between two possibilities." A few examples at this stage will be useful.

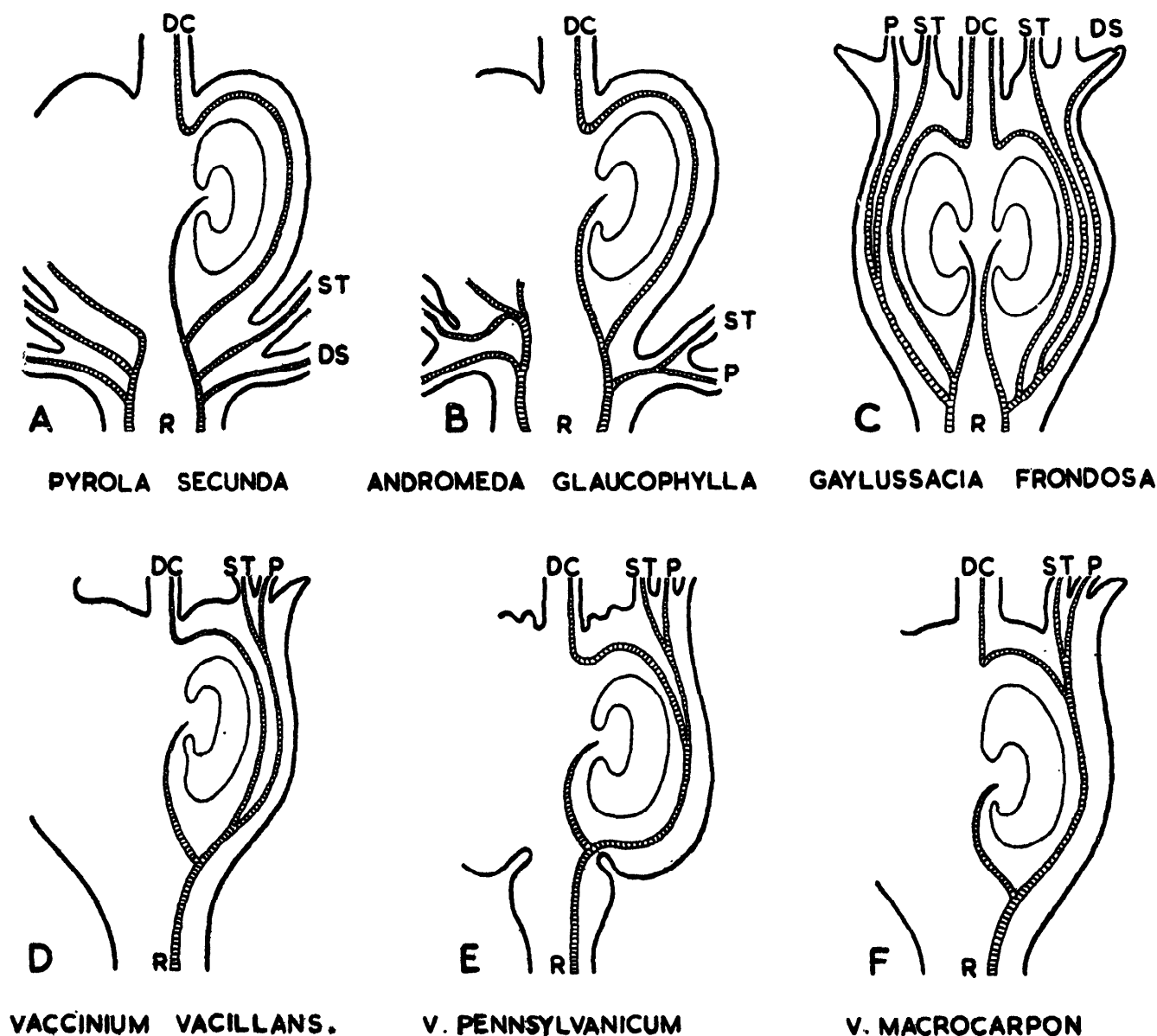


Fig. 1. Diagrams of longitudinal sections of ericaceous flowers showing progressive adnation of outer floral whorls and their vascular supplies to ovary wall (After Eames, 1931).

Eames (1931) gives a series of diagrams showing progressive fusion of the floral tube in ericaceous flowers to the ovary wall in the probable development of inferior ovary. The condition in A is considered to be most primitive—next to the ancestral form and that in F as most advanced (Fig. 1, A-F).

Eames & MacDaniels (1947) give a series of diagrams to show reduction in the number of ovules and fusion and reduction of vascular supply. The condition in A is described as most primitive and that in J as most advanced (Fig. 2, A-J).

Readers will be familiar with many series of diagrams of this type. All such cases show a number of variations occurring in the modern forms. Whether any of them occurred in the past we have no knowledge whatsoever. Some of these variations may be just diversifications not having any thing to do with the main current of evolution (see Goldschmidt, 1952). However, a phylogenist considers all of them alike and gives those that suit his convenience a phylogenetic interpretation. What is done is that such variations occurring in 'space' are regarded without any justification as

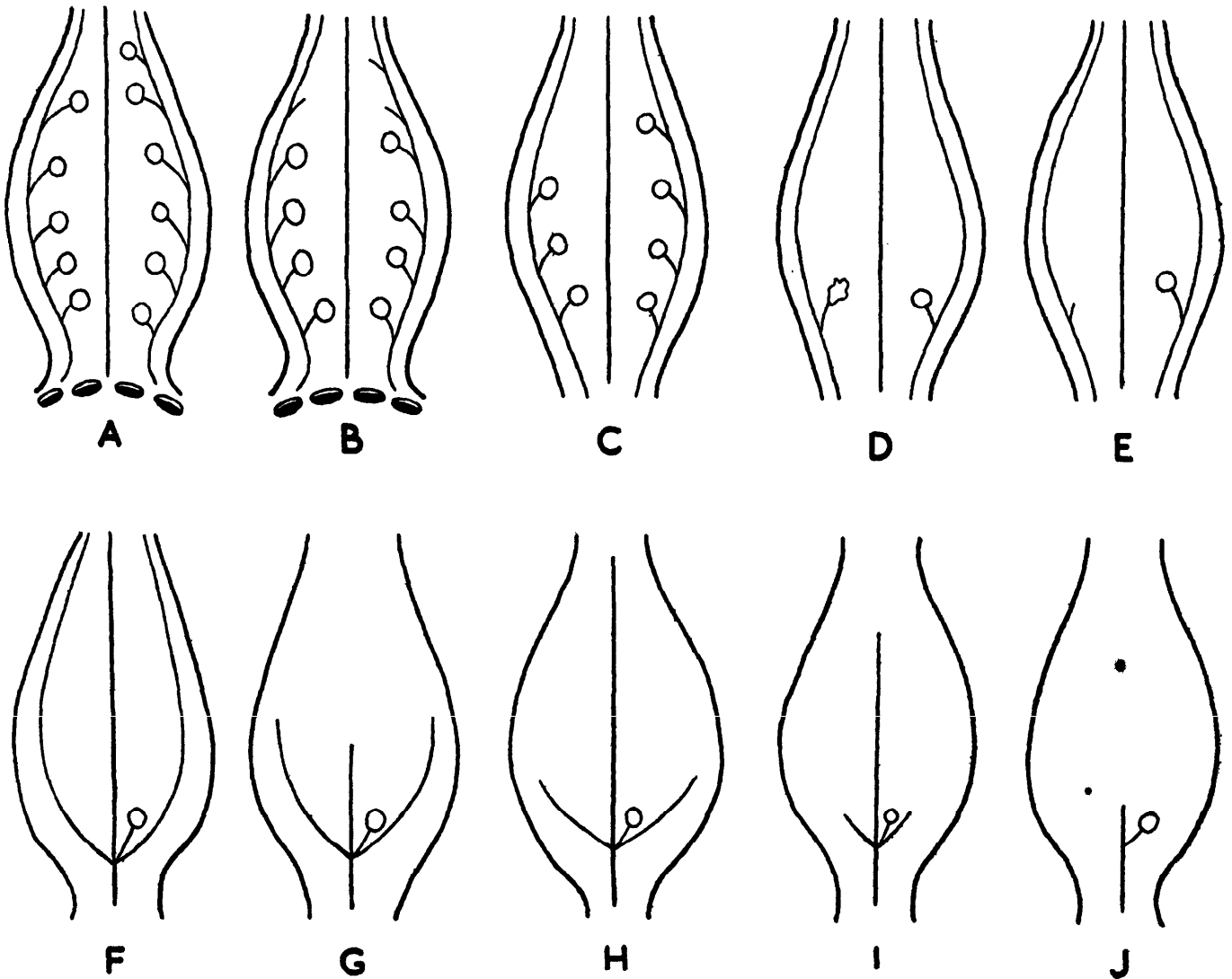


Fig. 2. Diagrams of carpel structure showing reduction of ovules and fusion and reduction of vascular in certain Ranunculaceae (After Eames & Mac Daniels, 1947).

variations having occurred in the course of 'time'. The only basis for this is that certain of the present-day characters are assumed to be more primitive than others. Following this the various modifications observed in living forms are considered as distinct stages by which the ancestral form has given rise to the modern form that is most different from it or most highly evolved. How much of this is

based on assumption is not difficult to see. Even the phylogenist recognizes it to begin with but he is apt to forget it very quickly even during the course of his argument.

Regarding 'correlation', all correlated characters are believed to be of the same age—primitive or advanced. If they have to be interpreted as primitive at least one of them has to be proved as such

through some other means. In the absence of fossil evidence this primitiveness is never proved but always inferred. Besides, correlation may occur in polyphyletic forms when much reliance cannot be placed upon it.

Thus all this talk about phylogeny appears to be meaningless in the absence of fossil record which alone can give us some insight into the reading of the variation gradient in the right direction. From his critical and exhaustive analysis of the data one would have expected Dr. Sporne also to have reached the same conclusion. But his final assessment seems to be determined not so much by the result of the analysis of the criteria given above, as by the apparent end results that are visible here and there, in our classification. For instance, he argues, "It is, of course, no surprise to many botanists to be told that the Magnoliaceae (in the broad sense) are among the most primitive dicotyledons . . . that this conclusion is no longer based on preconceived ideas or on circular arguments. It has been reached by logical process . . ." (Sporne, 1956). There will be many more example of this type.

The present author has no intention of denying the possible truth of this example or any other of its type. He is just keen to emphasize that we understand what it all means. It is commonly accepted that the Magnoliaceae is among the simplest families. But to say that it is among the most primitive dicotyledons or more primitive than such and such, or that it makes the parental or ancestral stock for such and such makes considerable difference. All this implies evolutionary history of the group, of which, as emphasized above, we know nothing whatsoever. Whatever we know about Magnoliaceae we owe to the untiring efforts of professional taxonomists, plant anatomists, morphologists, cytotaxonomists, plant geographers, etc. On the basis of the data collected thus it is interpreted as among the simplest of families. What the phylogenist has done is that he assumes this simplicity as synonymous with primitiveness. Let us not make any mistake about this.

The problem of determining phylogenies is further complicated by such processes as parallelism, convergence, interbreeding and hybridization. Vaviloff is said to have shown that parallel forms, or isomorphs appearing in allied species may represent identical genetic factors. Even fossil populations of a single species are shown to be of composite nature (see Bather, 1931). Such are the complexities of the problem we are faced with.

So even if we know all that is possible we shall not be able to construct phylogenetic schemes that may be logically sound. The patterns of evolution are too numerous and too complicated to be comprehended within such schemes. From the very nature

of things they are speculations, bad, good or even splendid. If our classification has to be of lasting value, if it is to be a precise and logical arrangement of verifiable facts let it not be mixed up with any speculation howsoever attractive or scientific it may appear. If need be separate phylogenetic schemes be developed to serve specific purpose.

Finally let us emphasize with Turrill (1942) that the main task of the systematist should be "to make a general classification which shall express as far as possible in rational order all that is known concerning plants and animals. This . . . ideal which, even if never attained, is one which may well make the systematist proud in the magnitude of his task. It is an ideal greater than the phylogenetic ideal which is included in it and one which in the process of attempted attainment must make taxonomy what it should be, the focal point of biology".

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