# THE SHIFTING DIAGNOSIS.

By

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In the course of a Presidential Address to the Geological Society of London, on Biological Classification (1927, Proc. Geol. Soc., LXXXIII, p. l. XII), limits of time and space forbade the discussion of some minor issues that arose naturally out of the subject as there treated from the standpoint of a palaeontologist. My thoughts may not be wholly out of place if laid at the feet of one who has done so much to illuminate the field of phylogenetic palaeontology as has our honoured teacher, LOUIS DOLLO.

If a species be regarded as a cross-section of a lineage, then it can be represented diagrammatically as a number of dots in an area, and, as Dr. TRUEMAN has shown us (1927, Quart. Journ. Geol. Soc., LXXXIII, p. 215) actual individuals from a district can be plotted in this way so as to portray the distribution of certain characters. Where the crowd is thickest, there will be the norm for that assemblage; and if the species be new, then a normal specimen (as thus determined) should be taken as holotype, and the species defined with reference to that norm, either rigidly (as Dr. TRUEMAN prefers) or to comprise the varieties, each of which could be distinguished if desired by a varietal name. WHEWELL, it may be remembered, recognised that natural groups could not be circumscribed by a definition, and held that each was best determined with reference to an archetype or norm (1840, „Philos. Induct. Sci." I., p. 476). J. W. GREGORY'S simile of the *circulus* obviously applies here, but his definition and use of that term involve the idea of heterogeneity (1896, „Brit. Mus. Catal. Jurass. Bryozoa", p. 22), and a lineage is *ex hypothesi* homogeneous.

The norm as determined by comparison with all available specimens is the same as the mode of a frequency polygon. When the norm of one locality is not the same as the norm in another locality at the same horizon, this may indicate the formation of a sub-species. It is assumed that the norm at a slightly higher horizon at the same locality will differ, probably owing to mutation in WAAGEN'S sense. From both these causes arises the need for arbitrary limitation. Since Dr. TRUEMAN speaks of „restricting the specific name to specimens identical with the holotype" *sans phrase*, it is necessary to point out that the holotype is not necessarily the norm. The fixation of species by holotypes, as usually carried out, is a purely nomenclatorial proceeding, and means that when a name is sought for any given assemblage, then that name must be used which has been definitely attached to some

one member of the assemblage however abnormal that member may be. This is a technical rule and has nothing to do with phylogenetic or taxonomic science. The selection of a holotype for a new species, or of a genotype for a new genus, does not absolve the author of the species or genus from the duty of explaining his conception by some form of diagnosis; and this is the only scientific part of the business.

The conception of the diagnosis, as was pointed out in my Address to Section C of the British Association (1920), has been changed with the introduction of phylogeny. The Artifical System was itself essentially a key, and its diagnoses were directions how to use it. In a non-evolutionary scientific system, whether based on the totality of the evidence, or only on anatomical structure, the key is furnished by the diagnosis. As WHEWELL puts it (1840, „Philos. Ind. Sci.", I, p. 493), the diagnosis is an Artifical Key to a Natural System — artificial, because it selects a few characters for a practical purpose. Such a diagnosis follows the rules of Aristotelian logic.

The construction of diagnoses for groups in a Phylogenetic System is more difficult: partly because of the fluidity of the characters; partly because of excessive parallelism and convergence; partly because the deciding characters are invisible or even non-existent in the adult, perhaps even suppressed in the whole life-history. LINNAEUS met with the last difficulty even in his Artificial Classification, in so far as it ceased to be artifical: the extremes even of a contemporary group may have (it is said) no character in common, and yet may be so linked up that no one doubts their affinity; the difficulty is intensified when the extremes are separated by three geological Eras.

It is clear that a diagnosis of a greatly and rapidly changing group cannot always be very practical; it deals too much in imponderables to serve as a key. A tabulation of characters is therefore required, and such a key may depart considerably from the diagnoses and the system.

A diagnosis, logically considered, should include none of the characters which define the group above, but all the characters which differentiate the group in hand from its peers. Since, however, the number of differentiating characters may be limited only by our finite power of analysis, a selection must be made. The selection must depend on (a) the purpose, (b) the nature of the material. For example, (a) the diagnostic characters of echinoderm larvae are useful to the deep-sea biologist, but useless to the shore-collector, and indeed many larvae are not yet assigned to their adults. Blood tests will certainly throw much light on the affinities of animals, but, while of value to the forensic biological expert (among others), they will not help the museum curator. (b) The stem-characters of crinoids or the radioles of sea-urchins are not of much use to the neontologist, who has complete individuals, but the palaeontologist is often provided by the geologist with no other material.

Such examples show that a diagnosis which may be unexceptionable for a systematist may be useless for another worker. Necessity has taught us that diagnostic characters are to be found in the shape of a single bone, in the microstructure of a rib-fragment or of a bit of wood, even in the structure of the nucleus of a cell. There is scarcely the minutest part of an organism that will not yield diagnostic characters if studied with sufficient care. Every study is justified by its object and results.

Selection then is needed, and the phylogenist will be led to select those characters which he has utilised in tracing the phylogeny. But here it is plain that the differentiating characters will not be the same when a species is to be distinguished from other, approximately contemporary, members of a lineage, as when it is to be distinguished from preceding or succeeding members of a grade. A diagnosis good from one point of view is meaningless from the other. It must never be supposed that a diagnosis is something fixed.

A further principle of selection is the distinction between fundamentals and variables. The variables are, in the terminology of the schoolmen, the „accidents", which may be either present or absent. An accident may be present in nearly every member of a group, as the crystalline stereom of all but a few echinoderms; or in only a small but definite portion of the group, as the character of a single jordanon in a large species. It is plain that an accident may have considerable classificatory value; therefore, after enumeration of fundamentals, the diagnosis should proceed to mention the leading accidentals, grouped if possible in some logical order. Some accidents are strictly diagnostic; others are not. Thus crystalline stereom is believed to indicate an echinoderm without doubt, though not possessed by all echinoderms. The calcareous bivalve shell of the *Lamellibranchia*, on the other hand, though found in all genera, is not diagnostic because such a shell occurs also in *Brachiopoda* and *Ostracoda*.

The description of any group (such as species) should, theoretically, give all the „properties" of the group. But those that are known to be common to the superior group (in this case the genus) need not as a rule be mentioned, except as evidence that the species belongs to the genus. Often, however, the description may contain features that do not at the time appear to be diagnostic, but which become so when new species are found and diagnosed. In this way also the diagnosis is modified. It is almost true to say that every new species or genus necessitates a re-casting of previous diagnoses.

My botanical colleagues J. RAMSBOTTOM and A. J. WILMOTT (Aug. 1926, Trans. Brit. Mycol. Soc., XI, 28) consider it essential to distinguish between the Taxonomic Species and the Natural Species, and I have tried to understand their meaning. A „natural species" appears to be that assemblage of individuals which one supposes to exist, no matter for the moment how it be diagnosed. The „taxonomic species" is defined as „all specimens (? individuals) that agree with the details of a properly drawn up diagnosis". The ideal is „so to define our taxonomic species that they agree absolutely with natural species" Thus the term „taxonomic species" seems intended for a danger-signal to remind us of the imperfection of our knowledge. Botanists may need this; palaeontologists are only too painfully aware of it. A taxonomic species in the ordinary sense of the words (i. e. a species as established by a taxonomist) is always the subjective aspect of the assumed objective natural species, and *qua* subjective is conditioned by human fallibility.

It is, however, questionable whether the attempted distinction is valid or serviceable. It is undeniable that diagnoses change, and that their content may also change. Species, for instance, are split up; or, as previously shown, the concept may be shifted in proportion as the norm is seen to differ from

what was first supposed. The diagnoses therefore change or should change with the concept, but it does not follow that the later diagnosis is any better than the former, or that the new concept is more natural than the old. A taxonomic species, assuming that its diagnosis correctly states the intention of its author, may be just as „natural" as any other assemblage.

The definition of a taxonomic species, as quoted above, is none too clear. It implies that a diagnosis can be drawn up properly, and yet not correspond with anything in nature: „properly" therefore must mean no more than *secundum artem*. The definition is also somewhat peculiar in speaking of a species as „specimens" If the word has been chosen with intention, then the definition excludes all individuals that have not been collected or observed by a botanist or zoologist. It seems that „individuals" must have been meant. This, however, still confuses the abstract with the concrete. The individual content of a species changes from moment to moment, even more than does the content of a regiment or a school. The concept may cover or represent individuals, but the individuals are not the school, or the regiment, or the species.

But the idea of taxonomic species is open to more deeply-founded objection. The palaeontologist will perhaps recognise more readily than the neontologist, not only that the concept of species is an abstraction, but that, as previously shown, it is a relative abstraction. It is therefore impossible for it to „agree absolutely" with anything but itself. If we had to deal with special creations and if time could be withdrawn from consideration then it is possible that one might in some sense speak of an objective species as a „thing-in-itself" But in the world of our modern conception and belief there can be no such rigid, enduring, definable entity. Therefore, if our precise abstraction could agree with anything outside itself, there would be nothing for it to agree with.

The eternal question: What is a Species? is eternal just because a species is an abstract conception and therefore subjective, and because the point of view is perpetually changing. The more practical question is wether a given conception corresponds to something in nature or no. If it does not, it must go. But probably many of the conceptions put forward have their concrete representation, and it really does not matter very much to which one or more of them you limit the title „species" The ordinary species is not polymorphic; but no one doubts that polymorphic species exist. Species as a rule can be distinguished by obvious morphological characters; but that is no reason for refusing rank of some kind to assemblages that apparently can be distinguished only by physiological tests. So also the palaeontologist soon discovers that his classificatory units differ both in their values and in their mutual relations.

All these units have to be built into our system, as bricks and stones and wood and iron are built into a house. But, since knowledge is perpetually advancing, so our unitary concepts perpetually change, and so also the System is constantly modified. To no human question is there an immutable and final answer.