TYPES IN MODERN TAXONOMY.

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ABSTRACT. Items called "types" have been used in taxonomy in three ways: as bases for definitions, as standards of comparison, and as fixed points to which names are attached. The modern conception of taxonomy as involving the inference of population characters from samples makes it impossible for the same items properly to serve all three of these purposes.

Inferences as to populations, involved in definition and in comparison, should be based equally on all available items believed by the given author to belong to the population in question. No items should be singled out as "primary" or "secondary," or emphasized as "types" of any sort. For this whole group of items, the sample properly constituted, the name "hypodigm" is proposed.

The only purpose that "types" in the classic sense can serve is that of name-bearers, and it is proposed to confine them explicitly to this function. Most of the dozens of terms proposed for various sorts of types should be discarded. In new work the one term "type" is all that is needed. In revision, "syntype," "lectotype," and "neotype," at most, may also be useful.

The hypodigms of superspecific units are also groups of concrete specimens although their types are abstractions.

A recent paper by Dennler recognizes, in part, the difficulty of basing taxonomic concepts on 'types" in the old sense, but the proposals made in that paper are believed to be inadequate and impractical.

INTRODUCTION.

IT is difficult in the technical literature of zoölogy and paleontology to find a clear and complete answer to the question "What purposes do types serve?" or even a full statement of the question. A selection of quotations shows that types are expected to serve several different purposes and that in general these purposes are felt to be self-evident and to be congruent if not identical. Thus Schenck and McMasters,[1] in their republication and discussion of the International Rules of Nomenclature, say, "The desire of specialists in nomenclature to have particular specimens as bases for specific concepts, definite species as bases for concepts of genera, and designated genera for the foundation of families has resulted in that group of particular specimens, species, or genera known under the general heading of 'types.'" In other words, types are bases

[1] Schenck, E. T., and McMasters, J. H.: "Procedure in Taxonomy." Stanford University Press, 1936.

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29

for the formation of taxonomic concepts. Marsh[2] long since wrote that, "As the number of described forms increases, the necessity of a direct comparison of types becomes imperative, and the comparative value of each type specimen is thus brought into notice," and he otherwise emphasized that types are standards of comparison. The International Rules[3] say, among other things, of types that if a genus is subdivided into two or more genera the original name is retained for the restricted genus containing the original type (Art. 29). In other words, the type is a thing that carries the technical name around with it wherever it may go in a classification.

The thesis of this paper is that these three purposes of types, however obviously related, are distinct and that it is now scientifically unsound procedure to expect or demand that the same specimens (or other types) serve all three purposes. It is proposed to examine and to define the three uses of types in more explicit and conscious detail, to consider what things or ideas best serve each separate purpose, and to revise the concepts and the nomenclature of types so that each is in fact served as well as possible according to modern methods in taxonomy.

THREE BASIC OPERATIONS IN TAXONOMY.

There are thus three basic taxonomic operations in which "types," in the broadest and, I think, most confused sense of the word, are concerned. For the moment attention may be confined to types that are actual specimens, individual animals or parts of them, and to the taxonomic categories, species and subspecies, based on these. A species, as it is actually defined or diagnosed[4] and used in the literature is a subjective concept. The usual theory, often questioned but believed by me and by most taxonomists, is that this concept corresponds more or less with a real thing in nature, a group of individual animals that are truly related in a way that makes them a natural unit of a certain approximate scope. This thing in nature may

[2] Marsh, O. C.: "The Value of Type Specimens and Importance of Their Preservation." This Journal, 6, pp. 401-405, 1898.

[3] "International Rules of Zoölogical Nomenclature." Any of several editions such as that in Schenck and McMasters, cited above.

[4] "Definition" and "diagnosis" are not quite the same thing and are often confused, but discussion of this point would necessarily be somewhat lengthy and is not essential for present purposes.

be considered the real species, but it is not and cannot be the species of taxonomy. The mental concept that is the species of taxonomy cannot be shown to be coextensive with the real species and even if by chance the two were coextensive, it would be an error to suppose them identical. One, the taxonomic species, is an estimate of the other, the real species.

Although it is a subjective concept and not a material thing, this estimate is not drawn out of the void but is based on observations of concrete specimens. These specimens do not constitute the real species but they are selected on the basis of belief (whether well- or ill-founded) that they do belong to one natural specific unit. The description of them is not a description of a species unless it implies or expresses a belief that they represent a natural group of which they are only a part, usually a relatively small part. The species concept is poorly formed, or indeed really absent, unless an attempt is made to infer from the specimens the probable characters of other animals that belong to the group but that are not in hand as specimens and that are not exactly like the specimens in hand. The specimens are, then, a sample from which group or population characters are estimated and this is the only proper sense in which they are the basis of a species in taxonomy. The members of this sample for estimation are thus "types" in one of the three senses in which that word is currently used.

The second basic operation of taxonomy is comparison, either comparison of species or identification of specimens, which is a different aspect of the same operation. Having formed an estimate from a sample, or a species concept from certain specimens, the purpose now is to see whether specimens hitherto excluded from the sample fall within the boundaries of that estimate and concept. If it is decided that they do or should (that the two species are synonymous or that the specimens being identified belong to a given species), then these additional specimens should be placed in the sample and one should return again to the first operation and redefine the species, because any change in the sample permits and demands a change in the inferences based on it. The change may be either a shift of boundaries or a narrowing of the limits of estimation, bringing them closer to the scope of the real species, or it may be and usually is both.

The sample taken at the beginning of this operation as representative of the species is a standard of comparison and its

members are thus "types" in the second of the three senses. The procedure here outlined is, however, importantly unlike the usual classical "comparison with types." That procedure usually involved taking a specimen or a series of specimens, "types," as inflexibly standard and typical for a given species and making a subjective judgment as to the degree of difference from them to be permitted to other specimens referred to the same species. It is a distinctly different procedure to take all the available material hitherto referred to the species and to determine whether the addition of other specimens would still give a sample such that an orderly group concept developed from it by controlled inference would correspond with the idea of a species. It is this procedure that is now advocated as the proper method of comparison and identification in modern taxonomy.

According to this theory of taxonomy, any identification or reference of specimens to a species makes possible and implicitly or explicitly involves redefinition of that species. Two workers seldom base their concepts of a species on exactly the same suites of specimens. Their concepts therefore will seldom be exactly the same and since these concepts, and not the natural or real groups, are the species of taxonomy, it follows that two original workers seldom use the same species. Taxonomy might thus disintegrate into an almost mystic chaos of individual systems were it not for a working agreement to which all taxonomists subscribe. This is that however much their bases for inference may differ in all other respects they will always include one or more specified specimens. Individual concepts can be conveyed in more or less concrete terms and this communication permits different students to bring their concepts into some agreement, or at least non-disagreement, as far as consistent with their general frames of reference. By a convention essential to this communication of ideas, it is agreed that the application of a certain name to a species is an implicit guarantee that specimens attached in a legal way to that name are included in the sample on which the species concept of the author now using the name is based.

A third basic operation of taxonomy is thus determining whether or not certain published specimens with names attached to them fall into a given specific concept, selecting the correct name if two or more such name-bearers fall into a single group concept, and coining a new name and attaching it to a par-

ticular specimen if no previous name-bearer is included in the concept. These specimens with names attached to them are also called "types," and it should be apparent that "types" in this sense are being used in a very different way from the two sorts previously mentioned.

HYPODIGMS AND NAME-BEARERS.

Most taxonomy has hitherto assumed that the same specimens, the "types," adequately serve all three of these functions. Some, perhaps most, taxonomists apparently feel not only that the single set of specified "types" can do this triple duty but also that it must do so. The newer theory of taxonomy as a system of group concepts based on inferences about populations from samples, a theory that is rapidly gaining ground and to which I strongly adhere, is decisively incompatible with this use of "types." According to this theory the specimens used as "types" in the three different ways not only need not be, but also cannot be, the same if proper scientific methods are followed.

In the original proposal of a species, the inference involved should be based on all the available specimens that are then considered as belonging to the species and on all of them equally. No extra weight should be given to any one specimen or small group of specimens that are shown or believed to be more "typical," nearer a norm or point of central tendency for the species. Every specimen, however "aberrant" or deviant from the norm, necessarily has equal weight in the correct drawing of inferences from the sample, providing that the specimen can properly be placed in the sample. In other words, every specimen that is referred to a species by its original describer should be used by that describer as part of his basis for the species and none should be singled out as "type" and considered as contributing more to the concept than does any other specimen.[5]

In order to have a necessary word to use in this discussion, the matter of terminology may here be anticipated on one point. It would be possible to extend the term "types" to include all the specimens used by the original author, not "holotype" and

[5] Of course in paleontology, for instance, a skeleton will contribute to inferences regarding more different characters than will a single bone, but as regards the characters shown by both the contribution of the skeleton should not be weighted any more than that of the bone.

"paratypes," etc., but simply "types," because all are coördinate in this use. This would, however, lead to endless confusion, as will become still more apparent in the following paragraphs. I dislike adding to terminology, but I do not see how to convey this idea clearly and succinctly without the use of some single word not liable to serious confusion with the quite different concepts hitherto included under the names "types," and I therefore propose the term "hypodigm" (pronounced hy′-podim, from the Greek ὑπόδειγμα, "token, example"). All the specimens used by the author of a species as his basis for inference, and this should mean all the specimens that he referred to the species, constitute his hypodigm of that species.

In a subsequent comparison or identification, the basis of comparison is not correctly a "type," in any restricted sense, but a hypodigm. Even if only one specimen was known to the original author, it should be used by him and for subsequent comparison as a hypodigm and not as a type in any other sense. Here "type" and "hypodigm" necessarily coincide in referring to the same specimen, but they do not become synonymous unless "type" be redefined. The type is an isolated object. The hypodigm, whether it include one specimen or a thousand, is a sample from which the characters of a population are to be inferred. In one case the mental process of an identifier is, in essence, "This specimen does not appear to differ notably from that (the type), so I will call it by the name of that." In the other, and scientifically correct, procedure, the identifier might say, "From this hypodigm, even though it includes only one specimen, I infer certain broad limits within which the boundaries of a real group probably lie. Since the specimen to be identified is within these broad limits, it may belong within the boundaries of the real group. As a working hypothesis I will place the two specimens together and use them as an enlarged hypodigm on which I can base certain narrower limits around the unknown real boundaries of the natural group that I am calling a species."

Each subsequent identification adds to the material on which the concept of the species can and should then be based and with which the next comparison should be made. These subsequent materials thus become "types" of the species as it is constantly redefined and as bases for comparisons, but to call *them* so would obviously violate the now accepted meaning of the word "type." They become parts of the hypodigm, which

is properly subject to frequent change. Every specimen placed in a hypodigm at a given time by any one author, whether at the time of original publication or centuries later, is coördinate with every other specimen then in the hypodigm. To distinguish principal, original, or primary from subsidiary, subsequent, or secondary hypodigms, as has been done for "types," would be to miss the significance of the whole procedure as it is correctly carried out on a sampling basis. A unique original specimen on which a species was first based has no more authority or weight for forming a valid concept of that species than has any one of a hundred specimens subsequently referred to that species and is no better as a standard of comparison.

Since data not imparted are not part of science as an impersonal body of fact and theory, specimens cannot be recognized as parts of a hypodigm unless publication has been based on them. With this exception every specimen definitely referred to a species is or has been part of its hypodigm. It is advisable that every worker specify the members of his hypodigm, some of which may be known to him at first-hand and others through the literature based on them. Commonly this can be done by stating that the current hypodigm is that of the original describer, or of some other reviser, plus and minus certain specimens. It is, of course, frequently necessary for a reviser to drop from a hypodigm specimens formerly placed in it. The hypodigm, the real basis for the species, is thus never properly a specified and static list of specimens placed in the species at any particular time or labelled as any particular sort of "type," but is and should be a fluid and constantly changing thing. In historic review it is proper to refer to what the hypodigm was for a particular author at a particular time, and this is generally far more significant than to state what that author accepted as "type."

From the point of view of strict comparison, a hypodigm is an indirect means of comparing with the real species that it represents. A species cannot be directly observed or compared; the real species because it is never available as such, the taxonomic species because it is merely a mental concept based on observations, not a thing subject to observation. When a single specimen or limited group of specimens are singled out as "types" to be standards of comparison, some observation or assumption must be made as to their position in a hypodigm. The classic "comparison with the type" is properly considered

only as a means of indirect comparison with a hypodigm, itself a means of indirect comparison with the real species. Surely every effort should be made to modify a method so full of indirection. Even if the degree of indirection cannot be much reduced, there is no mystic virtue in "types," as such, that makes them any better for comparison than would be any other member of a hypodigm. If one or a few specimens must be singled out, they should be as nearly as possible near an average point in the hypodigm, and in practice "types" very frequently are worse in this respect than are many simple "referred specimens."

For the third "type" function, that of name-bearer and anchor, a specimen that gives a species concept a name and that is by the name used guaranteed to be included in the hypodigm of the species, the requirements are very different. This convention requires a type in the classic sense of some specimen designated as such at the outset, once and for always.

No one can ever guarantee that two specimens will always be placed in one hypodigm, so that it is highly desirable that only one name-bearer be designated. If two or more co-bearers were originally given to a name, a subsequent reviser has the duty of cutting all but one of them adrift and leaving each name fastened to only one object.

Within the hypodigm where it is currently placed, a name-bearer may be a very poor basis for inference and a worse means of comparison, but this does not matter. It does not matter, that is, as long as no one insists that the name-bearer must be used exclusively or must be given added weight and authority for these other two purposes. To the extent that the confused idea of "types" so widespread in the past has considered these to be certain specimens of particular importance for all three purposes, it should now be apparent that these ideas and the terminology and practices based on them require revision.

TERMINOLOGY.

There is every reason, both practical and theoretical, why the fluctuating basis for inference and standard of comparison, which at any one time should be as nearly the same as possible, should be clearly and absolutely distinguished from the fixed name-bearer. As a matter of abstract terminology, the word "type" might be restricted to any one of these things, or used

for both and to include subordinate terms given to each, or abandoned altogether. The name-bearer might simply be called that, or perhaps "onomatophore."

As a matter of practical usage, however, it is evident that the word "type" is so deeply rooted in our science that it is not desirable and probably not possible to uproot it. It is also clear that what I have already called the "hypodigm" involves a concept different from that associated with the word "type" in the minds of practically all zoölogists but that the idea of the name-bearer does not. The hypodigm is not strictly anything that has hitherto been called a type although things called types were formerly expected to do what the hypodigm does better. The name-bearer, on the other hand, is strictly something that has been called a type. I propose, therefore, to use the word "type" for the name-bearing, anchoring specimen, and to cease to use it or to think of it as having any other relationship to the bases for specific concepts and standards of comparison, which are hypodigms and not types.

The subsidiary terminology of types has been expanded into an irregular framework—it cannot be called a system—that is approaching the fantastic. A list of these terms, although incomplete, includes fifty-one different words. Of course there are reasons for this spate of terms and I do not mean to ridicule them as a whole. I do mean to suggest that most of the reasons for them are obsolete, that most of the terms themselves should be obsolete, and that their continued proposal now is sometimes a curious diversion rather than a scientific contribution.

Probably the best of reasons for the complication of type terminology was that it was felt, just as I here insist, that the type, or holotype, as a name-bearer should be a single specimen but that this specimen was an inadequate basis for definition of species and identification of specimens. Thence the proposal of names for subsidiary types, primary, like "allotype," "apotype" and "paratype," or secondary, like "heautotype," "homeotype," "ideotype," "metatype," "morphotype" and "plesiotype." But all these seem to have the underlying idea that somehow there is a limited suite of specimens that really represent or give rise to the species, hence must be "types," while others, although they belong to the species, do not and hence are not "types." Now or soon, one may hope, the idea will prevail that every specimen referred to a species represents it and that all do so equally, mere

priority of description or convenience of name-bearing having nothing to do with this case. This terminology was a valuable recognition of the fact that what had been called type specimens did serve other functions than that of name-bearing, but it tended merely to retard recognition of the fact that these other functions do belong equally to all specimens, to the hypodigm and not to any specified "types" in a classical sense. Since such names do merely tend to preserve what I am convinced is an erroneous, in fact a vicious, sort of procedure, I urge their abandonment now that they are outgrown.

Other terms were proposed in order to suggest the reliability of identifications. Thus "homeotype" guarantees that the specimen has been compared with the original type, "ideotype" that it was identified by the original describer, and "topotype" that it came from near the original locality and (for fossils) horizon. But the factors of reliability of identification are myriad and it is futile, if not silly, to try to express all or even the most important in a "type" terminology. Comparison with the original type is less likely to give reliable identification than is comparison with a large suite of specimens (whether it include "the type" or not)—perhaps one should indicate by a Greek prefix the exact size of sample with which comparison was made. The original describer is seldom more likely to make a good identification than is any other competent reviser and often is distinctly less likely—perhaps we should all be rated for reliability and this, too, indicated by a prefix on our "types." The probable influence of origin of specimens is important and should of course be stated by a reviser, but I can see no reason for attempting to indicate by "type" terminology this and the hundred other subtleties involved in reliability.

Finally one can hardly avoid the impression that some "type" terms have been proposed and adopted more for their prestige value than for a more scientific purpose. The importance of a collection is often judged by the number of included "types," and curators have been human enough to be biased in favor of a terminology that promotes their aspirations. "Every specimen a type" is the curatorial equivalent of "every man a "king." How else can one really explain such terms as "icotype," a specimen identified as belonging to a species but about which nothing has been published? Only some term like "atype," a specimen that has not been identified, is lacking to complete

the *reductio ad absurdum.* With the unbounded multiplication of so-called "types" the prestige value of types in the strict sense tends to wane and perhaps this is one factor in the use of some terms like "diplotype" that confer a little extra luster on specimens that are, indeed, types but that are liable to oversight among the throngs of upstart "types."[6]

When types are viewed only in their proper function of name-bearers and anchors, without too much attention to their power of making an otherwise poor collection look important, it is apparent that some nine-tenths of these terms are not necessary or desirable.

The word "type," alone, is usually sufficient and is already understood to mean "holotype" unless explicitly modified. "Syntypes" are seldom used now and should not be used in future, but they are with us from the past and the term does still serve some purpose: it warns that several bearers started off with the name and permits use of "type" alone to mean a unique type. Hence "lectotype" also might be retained to warn that the present name-bearer was picked out from several starters. The word "paratype" is more respectable than most of these terms, but I would now discard it.

A paratype does no name-bearing or anchoring and is not necessarily a more important member of the hypodigm than specimens that are not types or paratypes. If, as some do, we define it as every specimen except the type (holotype) used in preparing the original description, a conscientious student working on a good collection might inadvertently make dozens of paratypes in each species, for of course he would use all his materials.[7]

Again, a "paratype" is no more valuable in reality than a subsequently identified specimen of similar character, but the terminology makes it hard to keep this important truth always in mind. It need hardly be added that the whole thesis of this

[6] A diplotype is the type specimen of a type species and the term is synonymous with the clumsy word "genoholotype." The thought apparently is that the specimen is doubly a type, an idea that is at least equivocal when the type of a genus is specified as a species, not a specimen. Except as accolades, I cannot see that such terms are particularly useful.

[7] This has another practical effect to be kept in mind. Almost all institutions prohibit sale or exchange of types and some include paratypes as a matter of policy. Curators have been faced by the dilemma of either omitting pertinent material from their research or being unable to dispose of specimens that could properly and profitably be exchanged.

paper is opposed to applying the name "paratype" to a syntype that is not made lectotype.

The problem of the use and definition of neotypes (or "protypes" as they have also been called, in part) is much disputed and here only the influence of the present point of view on the problem need be specifically stated. I would, of course, view neotypes only as substitute name-bearers and anchors. It often happens that it is impossible to assign a type with certainty to one or the other of the hypodigms currently recognized. This can arise in two ways: because the type is lost and the record of it is not explicit or surely reliable on points that distinguish current hypodigms, or because the type, although available, does not permit the sure determination of these points. The effect of these very common situations on nomenclature is always unsettling and sometimes disastrous. The rules provide no way to eliminate names under these circumstances, and at best they hang on, excrescences on nomenclature that mean nothing but that cannot be ignored and that may at any time, like cancerous growths, invade the sounder tissues of the science. The extra-legal proposal of neotypes, especially in paleontology, has arisen as an attempt to solve this problem by shifting the carrying of the name to a specimen that can surely be placed in a current specific hypodigm. Obviously this is not a complete solution and it would benefit by some agreed definition of permissible circumstances and procedures, but in principle I favor it strongly and consider it entirely harmonious with the present concept of types.

For things now sometimes called "types," I would thus use only the following terms:

In new work and review:

Hypodigm—as above, distinguished from types and discarding the idea of types as basis for species and for comparisons.

Type—The single name-bearing and anchoring specimen.

In review and revision only:

Syntype—One of two or more specimens to which a single name was equally attached in original publication.

Lectotype—A syntype subsequently selected to be sole name-bearer.

Neotype—A specimen substituted as name-bearer for one not now assignable to a definite hypodigm.

TYPES OF SUPERSPECIFIC GROUPS.

All types of subspecies and species, however the word "types" be used, are universally agreed to be concrete things, specimens.[8]

By rule (International Rules, Articles 4 and 30)[9] the types of subgenera and genera are species and those of subfamilies and families are genera. Species and genera are subjective concepts, not concrete objects, and so here the word "type" is used for a totally different sort of thing, almost as distinct from "types" of species as a fugue is from a pianoforte. There is no good reason why this should be; if a species and a subspecies, which is part of it, have the same sort of types, a genus and a species, which is part of a genus, could also have the same sort. The difference has, however, crystallized into rule and usage and the fact that it is a difference obscured by the use of the same term for both sorts of types. It has been a fruitful source of confusion and of argument in taxonomy and nomenclature.

Like species, genera and families are subjective group concepts that represent inferences as to real groups believed to occur in nature. The technique of drawing the inference is different and the groups that they estimate are taken to be of broader scope and, often, limited in different ways, but the nature of the concepts is the same. On what objective basis are the inferences drawn? The only objects in the procedure are specimens, so that they are necessarily this basis. This may be obscured by the technique of drawing up a verbal definition of a genus from the definitions of its included species, but the latter are only used as time-saving summaries of the characters of groups of specimens and it remains true that these specimens, not the species as such, are the basis of the generic concept.[10] If this is true of genera, it is also true of

[8] Open to quibble or exception, like all generalizations, but it does not seem necessary here to discuss the question of "type figures," "type casts," etc.

[9] As a matter of fact the Rules nowhere discuss types of species and are curiously indefinite on the subject of types in higher categories, but they imply these usages and are almost invariably understood to demand them.

[10] That this is really what is intended is perhaps more obvious if one considers the related problem of identification, which usually begins by referring a specimen directly to a family or genus before working down to the species. Also if a generic definition is modified, this is done on the basis of new observations on concrete specimens already in the genus or newly transferred to it.

families and all higher categories. Yet these are all agreed not to have specimens as their types.

Even more obviously than in the case of species, this confusion is cleared up by the recognition of the difference between hypodigms and types. The hypodigms of genera, like those of species or of any taxonomic group, are composed of specimens. The hypodigm of a genus, for any given worker, is the conjunction of his hypodigms for all the species that he places in that genus. Exactly the same technical procedure cannot be followed in the two cases because the hypodigm of a taxonomic genus cannot be so selected as to represent the natural or real genus in the same way that that of a species (or particularly subspecies) can. For genera with only one species the hypodigms of genera and species coincide although the inferences based on them are different. The way of estimating the population from the sample necessarily differs because the relationship of population to sample differs, but the basis of inference is the same.

The type of a genus is thus not its basis or its standard of reference but, as for species, is simply its name-bearer and anchor, which is fixed by rule as being a species.

Thus far the situation seems clear, but there is a more or less conscious difference of opinion as to how the generic name is ultimately linked to objective realty, to specimens, and neither rules nor usage clearly settles the question. The necessity for such a link is not entirely obvious theoretically, but all taxonomists soon feel its need in practice. According to one point of view, the type of the genus is a species as a concept regardless of name and regardless of any possibly erroneous assignment of material to it. The generic name then is linked not necessarily or primarily to the type of the species but to the hypodigm of the species. This is in some respects the more logical and theoretically desirable point of view and at first sight appears to be practical, but it does not work well. The principal case for which a rule is needed is when a name-bearing type is removed from a specific hypodigm. On the basis of this point of view, if this removal did not significantly alter the hypodigm, what is left of that hypodigm still represents the same species, although its name must be altered, and the species under some other name continues to be type of the genus. The joker is that two authorities frequently disagree as to whether the hypodigm has or has not been signifi-

cantly altered. Each is left free to follow his individual feeling in the matter, the "rule" is no rule at all, and stable nomenclature cannot be achieved in this way.

The other point of view is that the generic name is linked through its type, the species, to the type specimen of the latter. When an author gives a name to a species he implicitly guarantees that the type specimen to which the name adheres is a part of his hypodigm for the species and that he is to this extent anchoring his hypodigm to that of the original author, however much it may differ in other respects. This guarantee may be a fiction if he does not in fact know that type specimen and has erroneous ideas about it, but it is a fiction that is agreed to be binding in specific nomenclature and that works in that field as a practical rule. Those who maintain this second point of view on generic nomenclature hold that the same fiction (in the lesser number of cases when it is a fiction) or fact (as it usually is) should apply also to generic and family nomenclature. They would say that when the type species of a genus is named, a guarantee is given that the type specimen of that species is in the hypodigm of the genus and they would make the rule that the type species is and will remain through all the continued flux of the hypodigms the species whose hypodigm currently contains that type specimen. In very rare cases this rule can give rise to what appears to be an undesirable result, viewed as an isolated case, but it is an objective rule that works automatically, not dependent on caprice or opinion, and that therefore is in the spirit of the attempt to promote stability by rules. It is, moreover, the only truly logical way of linking the established practice of nomenclatural specific types to the now contradictory practices regarding nomenclatural generic types.

Taxonomic groups of higher than family rank technically have no types.[11] Of course they have hypodigms, which are the sums of the hypodigms of the included lesser groups, but their names are not tied to any particular specimen or group concept. They have no anchors which force different concepts

[11] The custom, not rule, of forming superfamily names from family names with altered endings gives the feeling that these, too, have types which are either families or the type genera of those families. There is no objection to this and superfamily names may be considered as ultimately linked to specific types, like generic and family names. The following remarks then apply to groups of more than superfamily rank.

going under the same name to overlap to the extent of including the type. Everyone decides for himself what he is going to place in a group and what he is going to call it. On the whole this anarchy works out very well, although the fact that it works for these relatively few groups familiar to most zoölogists does not mean that it could work for the tens or hundreds of thousands of minor taxonomic subdivisions most of which are known only within a small circle of specialists. A general scientific public opinion or consensus functions well enough to keep the upper levels of taxonomy reasonably stable and integrated, which it rather surely could not do amidst the mass of detail on the lower levels. Even though they change the hypodigm and the concept of, say, an order, most workers will do so only as regards minor points or only for very impelling reasons and will use an established name if their hypodigm is recognizably similar to those previously used. If they do not follow this conservative course, there is little chance that other zoölogists will accept their classifications.

CRITIQUE OF DENNLER ON TYPES.

Since this study was completed, there has appeared an interesting paper by Dennler[12] bearing on its subject. Dennler's paper refers primarily to the species problem and a proposed solution of it on a geographic basis, an important question stimulating discussion but not pertinent here. Dennler goes on, however, to produce from his conclusions on this problem some suggested changes in the concepts and nomenclature of types. His thesis here is that the description of a single subspecies, or a species without subspecies, is based on or derived from a type specimen, but that the description (i.e. the diagnosis or definition) of a species with two or more subspecies must be based not on a concrete specimen but on the combination of the types of its subspecies. The "type" is not necessarily "typical," and Dennler proposes to suppress the word "type" and replace it by "test" (for "testimonium"). A species with two or more subspecies cannot have a "test" because there is no one specimen that unites the characters of two or more subspecies. Without definition, he also uses the word "para-

[12] Dennler, J. G.: "La importancia de la distribución geográfica en la sistemática de los vertebrados." Physis (Buenos Aires), XVI, pp. 41-53, 1939.

test," evidently not in the usual sense of "paratype" but for a secondary type of some sort.

The proposal to substitute "test" for "type" is regrettable. A change of name is not a solution of any problem. Here the redefinition is essentially the limiting of types to type specimens of subspecies, which hardly requires the use of a new term. The elimination of the word "type" in taxonomy is probably impossible and certainly undesirable. The fact that a "type" may not be "typical" is, indeed, confusing, but is corrected by definition, consensus, and context. "Test," in English, is even more ambiguous, since the word already exists in that language with about a dozen different meanings, none of which is that intended by Dennler and two of which (a cupel and a shell or integument) are radically different. The word also already exists in French and in German, with equal ambiguity and inappropriateness. The average Italian or Spanish-speaking person (although Dennler is writing in Spanish) would find it practically impossible to pronounce "test" without a final vowel, and in those languages, also, the word with a final vowel already exists with a totally different meaning. In short, Dennler's feeling that the new term could well be used in any language is not true for any scientific language.

This unfortunate proposal is not important and belaboring it is not the reason for discussing Dennler's paper here. The important point, on which his paper is a valuable contribution, is the problem of unit as opposed to group concepts and the relation to these of the type concept. Dennler's opinion here (if I read his implications correctly) is that the smallest recognized taxonomic category, which may be a subspecies or a species, is a unit concept and it, or at least its verbalization, is based on a unit type, a single concrete specimen, while higher categories, species with more than one subspecies and by implication genera with more than one species, etc., are group concepts based on group types, the conjunction of the unit types of the included lesser categories.

This is an important step in the same direction as that of the present paper. Its prior appearance without my knowledge illustrates the common phenomenon of independent similar solutions of scientific problems. It has not, however, led me to modify any of the preceding passages of this paper, because it

is only a partial and in part an impractical solution, whereas a practical and essentially complete solution is possible. Dennler sees that a group category is not really based on a type, and that is the most fundamental point in my discussion, but he does not feel, as I do, that the smallest named taxonomic unit is still a group category and that it, too, cannot properly be based on a type but should be based on a hypodigm. The conjunction of included subspecific types is, certainly, a better basis for a species than is the type of a species, but still it is not the best basis. The best basis, beyond serious dispute, is the conjunction of included hypodigms. It is quite true, as Dennler implies and as I had already stated above before reading his paper, that the method of drawing inferences regarding larger categories is different from that for the smallest categories, but it is not true, I maintain, that the difference is reflected in that the smallest categories can properly be based on one specimen but the higher categories on several specimens.

Dennler's study also ignores the absolute practical necessity under the present conventions of nomenclature that a genus with several species and a species with several subspecies must still have a single name-bearer of some sort—a result of his failure (which is that of taxonomists in general almost without exception) to distinguish between the name-bearing and the group-exemplifying functions of what have been called types. The solution of discarding name-bearers, which is a probable implication of his suggestions, would lead to chaos, the destruction of our present nomenclatural system without substituting another.[13]

No two students would apply such a rule in the same way, and its application in any way would forthwith demolish such stability as our conventional nomenclature has finally achieved.

THE WORD "HYPODIGM" IN OTHER LANGUAGES.

The difficulty regarding the use of the word "test" internationally suggests pointing out that "hypodigm" is not

[13] This is reflected in a further suggestion made by Dennler. When a species is first described from a local form under a name appropriate to such a form and various other subspecies are later described, he proposes to limit the original specific name to the first known subspecies and to give the species a new name appropriate for the conjunction of all the subspecies. This new specific name would have no type or name-bearer.

ambiguous or confusing in any scientific language, and giving proper spellings for languages other than English. By analogy with the same roots as used in other combinations, the correct French form would be "hypodigme," German (and Latin) "hypodigma," Italian "ipodigma," Spanish "hipodigma." In all these languages the *g* is pronounced, hard, although silent in English. The *h*, pronounced in English and German, is silent in French and Spanish and omitted in Italian.

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