DISCUSSION AND CORRESPONDENCE

A NECESSARY AMENDMENT IN THE APPLICATION OF THE LAW OF PRIORITY IN ZOOLOGICAL NOMENCLATURE

THE keystone of all nomenclature of species and genera is the rigid and exceptionless application of the law of priority; without it no uniformity could be attained. There is practically complete agreement on this point among all who have had considerable experience in the definition of genera and species, that is, among those who best understand the difficulties of reaching ultimate agreement.

Now the International Code of Zoological Nomenclature has adopted the following:

Art. 25. The valid name of a genus or species can be only that name under which it was first designated on the condition:

(*a*) That this name was published and accompanied by an indication, or a definition, or a description; and

(*b*) That the author has applied the principles of binary nomenclature.

It is clear that the ruling of clause (*a*) allows too much laxity and is too indefinite. The amendment that has probably occurred to others, and that I would urge as most necessary, is the change of clause (*a*) to read as follows:

That this name was published accompanied by a published recognizable description or by a published recognizable drawing.

Publication is to be understood as meaning expression in print; and "recognizable," a description or drawing sufficiently accurate and detailed for distinguishing the species or genus named from any other species or genus known at the time when the name was applied. A few considerations among many may be mentioned to justify this amendment.

In the first place, as the code rules at present it is only necessary for a systematist to publish a name in accordance with the other rules of binary nomenclature, and to give an "indication," indicating, *e. g.*, a particular type specimen in a particular collection; he is really not obliged to give any description whatsoever, or he may give an indifferent or even an inaccurate description provided he makes this indication. In time this loophole will lead to the utmost confusion; it would be impossible to get any idea of a newly named species without resource to the type specimen. This is the main reason for eliminating from the clause in question the vague and meaningless word "indication"—vague and meaningless unless it signifies the indication of a particular specimen. Surely it is not the spirit of scientific nomenclature to point to a specimen as an idea!

In the second place, it is becoming more and more necessary that the name should be accompanied by a recognizable description or drawing. There are many of the most modern systematists, as well as large numbers of the earlier ones, whose diagnoses are worthless for purposes of positive identification. These diagnoses either do not mention the important characteristics, or do not describe them fully or accurately enough, or else do not draw comparisons with the closely allied forms. If there is one thing we want to get out of the description of a new species, it is the item of how it differs from the known forms of the same group. Such descriptions are a positive hindrance to systematic progress, they may even arrest it entirely. They are scientifically valueless, they vilify the journals containing them. Under the present code one must have resource to the type specimen. Suppose then I am monographing a particular group, and find in the literature some papers in which the descriptions are practically meaningless; then I must ask for the loan of the specimens; but they may be in a museum the rules of which forbid the loaning of types; then I must in person visit that museum to find perhaps that the specimens have been mutilated beyond recall or have even been lost. Or should I wish to undertake the revision of the species of a comprehensive genus, say *Epeira* or *Bulimulus,* then I should have to undertake a *Weltreise,* for which my finances would probably be inadequate, and visit practically every museum in the world. Then everyone knows how type specimens suffer from handling, how perishable the average dried or alcoholic specimen is. The mere statement that a type specimen exists in such a collection is not the spread of a scientific

knowledge. Suppose a man should undertake a review of the present status of the problems of evolution; would we not expect that he would at least describe the various theories? Would it not be ridiculous for the reviewer to give simply a list of books and papers, and indicate in what libraries these are to be found? Science can make no advance when nothing but names are given, or only unrecognizable descriptions, with the indication where specimens are to be found. The retention of a type specimen is always desirable for future reference, but the publication of a good description is the *sine qua non* of scientific advance.

Those who take the pains to furnish adequate descriptions, and who draw comparisons with the previously known species, will always be regarded as the original describers whether the rules of nomenclature give them credit or not.

The suggestion of demanding a recognizable description is, of course, open to the objection that it is difficult to decide what constitutes a recognizable diagnosis. The decision must be made separately for each particular case. All would concur in the fairness of the principle to consider the adequateness of the description at the time when it was made. A description would be recognizable if at the time it was published it served to demarcate the species from all the other then known species of the genus, or the genus from all the other then known genera of the family. Questions of this kind can generally be decided by any monographic reviser of a group.

As the case stands at present there is all incentive for hasty and insufficient diagnoses, at least our codes do not prevent them. We have either to hunt up the type specimen, to spend fruitless hours to try to read some meaning into a description, or to try by the unsafe method of elimination to determine what species an author intended. As the A. O. U. Code of 1886 put it, "Zoological nomenclature is a means, not an end, of zoological science"; it ought to make the path clear and not maintain obstacles. Too much of a premium is placed upon age, there is too strong a tendency to resurrect the oldest name and try to fit to some particular species, whether it was accompanied by a good diagnosis or not. So long as this continues so long will the names in use be unstable.

Personally, I never have and never will regard a name as tenable unless it is based on a recognizable description or drawing, and that whether type specimens are preserved or not. There are many who share this view, and in this connection attention may be drawn to the very cogent recent arguments of Looss.[1] We are all in sympathy with the endeavors of the International Committee, most of us realize the difficulty of the questions it has to decide, and we are ready to relinquish personal views in order to reach uniformity. But there will never be uniformity of opinion in regard to the matter of allowing a name to be based simply upon an "indication." New names are multiplying in a geometrical ratio, some of them newly coined and others raked out of the ash heap of describers who deserve oblivion; few genera have been thoroughly revised; if mere "indications" and inadequate descriptions continue to be permitted the task of revision will before long be hopeless. Then there will be need for far more radical reform than the one here suggested. Now while our rules are still plastic let us insist on the absolute necessity of adequate diagnoses of genera and species. Thus insecurity may be abolished, each describer be given his just due and no more, and science as well as nomenclature be benefited.

One recommendation the committee might embody in the code to clarify future systematic work, though, of course, it could not be applied to the work of the past. That is, that when structural characteristics enter into a diagnosis they should be represented so far as possible by drawings rather than by words. A drawing is immediately clear, few descriptions are. Above all it is often very difficult to build a conception of a structure from a brief Latin description since the Latin is too poor in adjectives for our present needs. Describers are too intent upon their own convenience, give too little attention to the con-

[1] *Zoologischer Anzeiger*, 1907, Nr. 19.

venience of those who have to read their descriptions. This recommendation will be of great aid in identifying species and genera and will help towards that end when men will see there is honor in furnishing good diagnoses, but no honor in simply naming species.

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ANOTHER WORD ON THE VULTUR CASE

MY brief allusion to Dr. Allen's inconsistency in his latest elimination of *Vultur* seems to have been clear to all with whom I have discussed the question except Dr. Allen, who fails entirely to see my point.

It seems necessary, therefore, to restate the matter. The case is as follows:

Sarcorhamphus 1806.
gryphus.
papa = type of Gypagus 1816.
auricularis = type of Torgos 1828.
Cathartes 1811.
papa = type of Gypagus 1816.
aura.
Gypagus 1816.
papa.
gryphus = type of Gryphus 1854.

Dr. Allen says that while *gryphus* is the type of *Sarcorhamphus* it was not the type in 1806 and only became so in 1828 by the removal of the other species. Therefore, he claims that in eliminating *Vultur* we have no right to remove *gryphus* at 1806 and can only remove it at the date at which *it became* the type of *Sarcorhamphus*.

This is absolutely contradictory to his own practise in all other cases, nor can I find a precedent in the "current usage" of other eliminators. For instance, *papa* is the type of *Gypagus* 1816, but it was not the type in 1816, and only became such in 1854; and yet Dr. Allen in all his eliminations removes *papa* at 1816, which any one can see is the date of establishment of the genus, not the date at which *papa* became its type. To be consistent *gryphus* must, of course, be removed at 1806, as I stated previously. Dr. Allen's recent note in which he repeats that *papa* must be removed at the date at which its genus was established, while *gryphus* must be removed at the date it became the type of its genus, only emphasizes his inconsistency—an inconsistency which is too self-evident to require the employment of any "imagination."

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SPECIAL ARTICLES

RELATION BETWEEN BIRTH RATES AND DEATH RATES

A SHORT notice appeared on page 641 of SCIENCE, 1907, of a paper read by C. E. Woodruff before the American Association for the Advancement of Science, on the relation between birth rates and death rates, etc.

In this connection, it may be of interest to note that a mathematical expression can be obtained for the relation between the birth rate per head b and the death rate per head d, for the case where the general conditions in the community are constant, and the influence of emigration and immigration is negligible.

Comparison with some figures taken from actual observation shows that these at times approach very nearly the relation deduced on the assumptions indicated above.

I give here the development of the formula, and some figures obtained by calculation by its aid, together with the observed values, for comparison.

Let $c(a)$ be such a coefficient that out of the total number N_t of individuals in the community at time t, the number whose age lies between the values a and $(a+da)$ is given by $N_t c(a)da$.

Now the $N_t c(a)da$ individuals whose age at time t lies between the values a and $(a+da)$, are the survivors of the individuals born in time da at time $(t-a)$.

If we denote by $B_{(t-a)}$ the total birth rate at time $(t-a)$, and by $p(a)$ the probability at its birth, that any individual will reach age a, then the number of the above-mentioned survivors is evidently $B_{(t-a)}p(a)da$.

Hence:

$$N_t c(a)da = B_{(t-a)}p(a)da$$