

bit of writing would rarely appeal to any large number of children in an equal degree or in the same way; consequently their relation to it would not be of a strictly comparative kind in a literary sense.

The examples given seem to me absolutely valueless for comparison. Number 607 is the production of an idiot. Number 520 is a quotation; no child in its teens could have conceived it. Number 434, if a genuine original, is the only one showing anything but a lesson poorly remembered, it is the only one not quoted or paraphrased from an adult production which has any literary merit at all.

WM. H. DALL

SMITHSONIAN INSTITUTION,
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GENOTYPES ARE THE SPECIES UPON WHICH GENERA ARE BASED

THE case presented by Dr. Stiles on page 620 of *SCIENCE* for April 21 last, possesses exceptional importance for the student of muscoid flies. Probably in no other superfamily of animals have as many misidentifications been made as in the Muscoidea. Species have been repeatedly confused, combined, jumbled and wrongly determined ever since the time of Meigen, if not before, until the tangle has now become frightfully intricate in character. Especially within the past decade or two have misidentifications of North American forms enormously increased, so that the literature is now overburdened with the resulting error, from which it will be a labor of great magnitude to free it.

The principle involved in misidentifications or cases of mistaken identity is always the same for all cases, and the problem is capable of only one correct solution. Of two diametrically opposed propositions, one must necessarily be right and the other wrong. While I can see the case clearly from both points of view, the wrong premises of the one view stand forth distinctly in my mind, and I can not grant that there exists here any necessity for arbitrary decision. The whole matter rests, of course, upon the adoption of rational and correct premises.

Properly approaching the question, its solution is simple, and I need only repeat here the axiomatic title at the head of these remarks.

The correct and only logical premises are represented in the axiom that EVERY RECORD OF A SPECIES OR OTHER TAXONOMIC UNIT IN THE LITERATURE BECOMES AT ONCE A PART OF THE SYNONYMY OF THE SPECIES OR UNIT INTENDED FOR RECORD BY THE RECORDER. It makes no difference under what name the record be made, the entity referred to remains the same, and the synonymy of that entity is thereby enriched by the name used followed by the name of the author making the record together with the date of same. This precludes confusion whether or not misidentification exists. The genus *X-us Jones, 1900*, unmistakably has for its type, under the conditions of the problem as stated, the species *albus Jones, 1900*. The genotype can be no other than this, which is the particular form so identified by Jones at the time and by him intended as the type of his genus. Jones has misidentified his genotype with Smith's species, hence the name *albus Jones, 1900* (*non Smith, 1890*), becomes a synonym of the name that shall finally hold for the genotype, that is to say, the particular form indicated by Jones. It is conceivable that Jones might differently identify the same form at different times, hence the necessity for a synonym to take the date of publication, which should include the month and day if Jones is a voluminous and frequent publisher.

The fallacy of the opposite premises is very evident. Were we to admit the latter it would be impossible to present a rational synonymy of forms. In the above case, *albus Smith, 1890*, has no further connection with the matter in hand after it has been proved that *albus Jones, 1900*, is a different form. It should be evident that an author's record of a form must remain always a record of that form in his sense at the time of record. The name he uses is merely a handle by which we can ourselves find and locate that form. If we ever decide that a record of a form is *not* a record of the form in the sense

intended to be recorded, we are clearly on the wrong road. And this is exactly where we should be were we to decide in the above stated question that the record of albus Jones, 1900, is not a record of albus Jones, 1900, but a record of albus Smith, 1890, knowing the contrary to be the case. The wording of the question itself in Dr. Stiles's title carries the correct solution. The species upon which a genus is based is necessarily the type of that genus. If it be found that the species has been erroneously determined, the determination must be corrected, and if it is found to be undescribed it should be at once characterized by the discoverer of the erroneous determination or some one else; otherwise the genus might by some be held to fall, being left without a described type species that can be designated. I would suggest that a special provision be made for such cases, whereby the genus need not fall in event of its type species proving undescribed. It can always be referred to by the name used in the original record, as albus Jones, 1900 (non Smith, 1890), until it can be better characterized. The species, whatever it prove to be, remains the type in the end.

Suppose the case of A and B, two men who are look-alike twins. I am acquainted with A, but I am ignorant of the existence of B. I see B, whom I believe to be A, commit a crime, and I give evidence in court, in my mistaken but conscientious belief, due to a misidentification of individuals, that A committed the said crime. Does this make A the criminal in the case, or does B remain the criminal? I think no argument is needed to show clearly that the person whom I saw commit the crime is bound to remain the criminal in the case, regardless of the name by which I designate him; my A is synonymous with B. Entities must be maintained. If individuals are confounded, their individuality is lost.

Following still further the principle of mistaken identity, it is evident that an author can not correctly put a previously published record into his synonymy without correctly ascertaining the identity of the forms con-

cerned. It is equally evident that, whether he has or has not correctly ascertained the same, he personally, and no other, is responsible for the synonymy published under or over his name. Still further, it is evident that, if his synonymy be found incorrect, it does not hold, and the status of the particular forms which he has wrongly so indicated remains the same as before. No synonymy is entitled to recognition unless founded on material studied, hence the detection of error carries with it a location of the material under consideration at the time by the said author. If the points involved in the same ever become of sufficient importance to warrant, then the forms represented in the said material must properly, for synonymic purposes, take the names by which the said author recorded them plus his own name and date.

The element of protection demands consideration. It is evident that a taxonomic unit once correctly defined and named must be recognized and protected from distortion. What protection has albus Smith, 1890, if we allow it to be cited as the type of a genus that not only was manifestly not intended for it by its author but may even prove to be incompatible with it in its characters? If the characters of the genus X-us Jones, 1900, are not stated by its author, the same are to be found only in the material of albus Jones, 1900. If no such material has been studied and the new genus has been proposed on the strength of the description of the genotype cited, then no misidentification exists and the case as stated does not apply. Likewise if the type material of the genotype is cited the case does not apply. All phases which do not carry the misidentification principle may be similarly eliminated from the present consideration.

Those who would maintain, in the face of the above remarks and under the conditions of the question, as stated by Dr. Stiles, that albus Smith, 1890, is the genotype of X-us Jones, 1900, can in my opinion have no other excuse for their action than the desire to shirk taxonomic responsibilities because they involve increased labor. Clearly an author

has no right to treat a subject in the literature without complying with the responsibilities which his treatment, so far as it goes, demands. If he does so, he alone is at fault and he alone must suffer. Slipshod taxonomic methods carry their own germs of decay. If I myself have offended in this respect, I neither deserve nor desire sympathy as to the particular points of my offense. Every author's work must be verified until it becomes apparent that correctness has been attained. In this manner only can we put taxonomy on a sound basis. It is evident that the desired consummation of demonstrated taxonomic correctness for most forms is a long way off; but deplorable as this may be, and as difficult of achievement as it is deplorable, we can not in any event justly dodge the points at issue. Nomenclatorial problems must be fairly met or we shall never attain the desired end.

I have heretofore held aloof from discussions of nomenclatorial intricacies in general, knowing that the conditions of muscoid taxonomy are at present such that few cases can yet be definitely stated, although the future holds a multitude of them for ultimate solution. But I consider that the necessity for deciding the present question as above suggested is of such paramount importance to the welfare of future taxonomy that I have, at the risk of prolixity, presented the evidence both direct and indirect as fully as I am able to see it at the present time. The effect of the final decision by the international commission of questions involving the misidentification principle will have the utmost bearing on muscoid taxonomy, from which confusion will never be eliminated until we know the morphology of the reproductive system, egg and early stages thoroughly, as well as every detail of the external anatomy of the fly, and perhaps all the details of its internal anatomy. The conditions in the Muscoidea are quite unique, forms belonging to distinct genera and tribes, or even distinct subfamilies, often being closely similar in external adult structure. Many authors have in consequence sadly mixed and confused distinct forms throughout their work, and if

we ever decide against the *intent* of an author it goes without saying that we shall be irretrievably lost in muscoid synonymy. Correct interpretation of an author's meaning is as important to us as priority in nomenclature. Therefore the importance of securing a rational working decision can not be overrated.

CHARLES H. T. TOWNSEND

PIURA, PERU,
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LATIN DIAGNOSIS OF FOSSIL PLANTS

AMONG the rather numerous nomenclatorial rulings of the International Botanical Congress which are considered retrogressive by a large number of systematists is that which requires the diagnoses of new species, genera, etc., to be in Latin (*sic*).

In order to test current opinion among paleobotanical workers a memorandum has been circulated by Professor Nathorst, of Stockholm, and Mr. Arber, of Cambridge, and the result, published in a recent number of *Nature*¹ will be of much interest to American systematic botanists.

The rather remarkable result of this interchange of opinion shows that every paleobotanist in Scandinavia, Great Britain and North America proposes to disregard this ruling of the congress.

The memorandum which was circulated contained the following statements of intention:

1. I do *not* propose to include a diagnosis in Latin in the description of any new species, genus or family that I may institute in the future, unless there appear to me, in particular cases, to be special reasons for so doing.
2. I will *not* refuse to accept new species, genera or families of fossil plants instituted by other workers in the future, solely on the ground that their description is not accompanied by a diagnosis in Latin.

This was signed, with some modification of wording in the case of Mr. and Mrs. Clement Reid and Professor Seward, by the following

¹ May 18, 1911, pp. 380, 381.