

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,
May 7, 1911

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.

students: Nathorst, Bartholin and Halle, of Stockholm; Benson, Royal Holloway College; Berry, Johns Hopkins; Cockerell, University of Colorado; Gordon, Edinburgh University; Hartz, Copenhagen; Hickling, Stopes, Watson and Weiss, of Manchester; Holden, Nottingham; Hollick, New York Botanical Garden; Jeffrey, Harvard; Kidston, Stirling; Knowlton and White, of Washington; Lewis, Liverpool; Maslen, Oliver and Mr. and Mrs. Clement Reid, of London; Möller, Sweden; Mr. and Mrs. D. H. Scott, Oakley, Hants; Arber and Thomas, Cambridge; Wieland, of Yale.

Judging by the protests one hears in the United States and the accounts of the Botanical Congress, it would appear that a good many of the rulings which it adopted are very far removed from being international in character or origin. Certainly its proposals regarding fossil plants, which emanated for the most part from Berlin, did not display much insight into the subject.

EDWARD W. BERRY

EXECUTIVE RESPONSIBILITY

TO THE EDITOR OF SCIENCE: May I trespass on your space to the extent of replying briefly to the criticism by Professor A. D. Mead in your issue of June 23, of my letter on academic tenure, which was printed in SCIENCE on May 12?

Professor Mead's criticism has such a moderate tone, and there is so little in it that at all affects the tenability of my position, that it would not demand a reply were it not for the fact that it seems to imply that the "freedom of opinion and utterance" he declares to be so well guaranteed to the Brown faculty, should not extend to the columns of SCIENCE as well; and that it has other implications which suggest the workings of the theological rather than the scientific mind, by a reliance on dogmatic assertion instead of evidence.

Professor Mead admits that men have been removed from the Brown faculty, but declines to enter into any "futile controversy" over the cause. They must have been removed

justly, he argues, because the charter forbids such action for anything else than "misdeemeanor, incapacity or unfaithfulness," and the present administration only enforces it for such reasons. This may be the case, but we have no evidence of its being so except Professor Mead's opinion; and that is offset, in my mind at least, by the assurance of several present and past members of the Brown faculty, that tenure is extremely uncertain there, and that arbitrary removals are frequent.

There are always two sides to any question, and it would be unjust to accept the statements of men who have been removed from the Brown faculty, as unbiased evidence. The statements of such men, however, that I have heard made with increasing frequency during the last few years, go far to call into question, if they do not disprove, the assertion of Professor Mead that men of long service in the university are not removed until they are given a "reasonably fair chance of readjustment in other positions." Of course there is room for difference of opinion as to what constitutes a reasonably fair chance; but I question very much if, even after the statements of these men had been much discounted to allow for personal interest, an ordinary jury would agree that they had had much of anything in the way of a chance to readjust themselves in new positions.

Leaving out of the question, however, the statements of men who have been removed, a case is made out against the Brown administration by the very arguments with which Professor Mead tries to justify its course. He admits that men have been appointed to various professorial grades and continued in them for years, only to be removed afterwards by the same administration that advanced them. Such a course as that can not be justified, and the attempt to do so by statements about "having reached the limit of growth in the environment of the particular institution," should be very severely reprehended by everyone who desires to save education from serious discredit. Even in our largest institutions too much is said about the necessity for rare and special talents, and in Brown,