moral interest and rights in private collections which have become the basis of important published work and what steps can be taken looking toward the eventual public ownership of such collections? Could a close cooperation be developed between certain leading collections of each country or at least continent to the end that by an automatic distribution of surplus material with each other, there would eventually be built up one or two central world collections in each major country or continent? How far can the progress of taxonomy be best served by the free loan of material to specialists? Can a uniformity of practice be established concerning division of material thus borrowed between the museum and the specialist?

Types: What progress can be made toward definite fixation of the type specimens of older authors? Is a cooperative undertaking possible looking toward the eventual recording and possibly also joint publication of the location of all type specimens now in existence? How far are special designations for different categories of "types" useful and advisable, and can we come to a uniform practice in their application?

What should be the policy of custodians of types toward their loan and what toward their isolation from generally used collections?

Determinations: Vigorously expanding research in ecology, life histories, morphology, genetics, applied entomology has not been accompanied by a corresponding increase in taxonomists, upon whom these researchers are dependent for determinations. How can this need, which will increase as time goes on, be met?

FORUM ON PROBLEMS OF NOMENCLATURE

There will be an address on "The Future of Zoological Nomenclature," by Dr. Charles W. Stiles, secretary of the International Commission on Zoological Nomenclature. The remainder of the session will be devoted to an informal discussion of nomenclatorial problems, as affecting entomology.

The Theory of Nomenclature: There exist two conflicting points of view. The one is that the principle of priority must be strictly observed in all cases in order to do justice to the taxonomist who first proposed each name. This, therefore, makes its application a matter of moral obligation. The other point of view is that there is no essential principle behind nomenclature, that the sole aim is to secure uniformity of practice, and that while the principle of priority is in the main a useful tool, it should be discarded just at the point where it hampers and impedes more than it assists in securing uniformity and convenience.

Family Names: Is it desirable for this congress to take steps looking toward the adoption of a definite method for determining the type genus of a family, and consequently the family name?

Would it be desirable to establish an international committee to compile information upon the status of all family or subfamily names in order to determine the sense in which they have been most generally used and to recommend to the International Commission on Zoological Nomenclature that their usage be conserved in such sense?

Other problems of nomenclature that any one in attendance may wish to raise.

J. CHESTER BRADLEY,
Secretary, Section on Taxonomy, Distribution and Nomenclature

DOES THE AMOUNT OF FOOD CONSUMED INFLUENCE THE GROWTH OF AN ANIMAL?

THE original article appearing in this journal[1] under the above title solicited the attention of investigators in animal physiology to what appeared to the author to be a serious defect in methods of experimentation being widely used in attacking problems relating to nutrient requirements, the nutritive values of food materials and the synthetic capacities of animal cells. It was an attempt to define the tacit implications upon which these methods are based and to show that they are not sufficiently well established, or, in some cases, even sufficiently plausible, to render any method based upon them an effective instrument of research. It was hoped that the article would stimulate discussion of the fundamental principles of such experimental methods, since these principles have not been critically discussed elsewhere.

W. C. Rose,[2] in an article bearing the same title as this communication, has offered a defense of the methods criticized, specifically a defense of feeding experiments in which the intake of food by otherwise comparable animals or in otherwise comparable experimental periods has not been equalized. This defense was prompted by the fact that some of his work was cited anonymously in my original criticism for purposes of illustration.

In his work on the indispensability of arginine and histidine,[3] Rose is convinced that the inadequate food consumption of rats on the low-histidine rations was the result of dietary inadequacy, and in defense of his conclusion he cites a number of opinions of eminent investigators in nutrition to the effect that animals

[1] Mitchell, H. H., SCIENCE, 1927, lxvi, 596.

[2] Rose, W. C., SCIENCE, 1928, lxvii, 488.

[3] Rose, W. C., and Cox, G. J., *J. Biol. Chem.*, 1924, lxi, 757; 1926, lxviii, 217.

on inadequate diets tend to reduce their food intake. This tendency was specifically recognized in my preceding article, but surely the fact that incomplete or inadequate diets ultimately, or even immediately, lead to a diminution in the intake of food is no justification for the assumption that a diminished or inadequate intake of food is *prima facie* evidence of the incompleteness or the inadequacy of the diet fed. The latter assumption requires also the proof that qualitative inadequacy in the diet is the *only* cause of inadequate food intake, and this proof has not been furnished. The only evidence offered for the inadequacy of the histidine-free diets is the failure to obtain growth with them. But in no case was it shown that these diets were consumed in amounts which would support growth or maintenance if they had been adequate in every respect.

Rose cites the consistency with which his experimental rats ate sparingly of the histidine-free (but arginine-containing) diets as "convincing proof that arginine and histidine are not interchangeable in metabolism . . ." It is true that this uniformity in the reaction of his animals to histidine-free diets contributes to the significance of the conclusion drawn, but after all such evidence is circumstantial in character and can not be entirely convincing.[4] If this position is not acceptable to Rose, the fact that other investigators,[5] working on the same problem with methods identical in all essentials with his own, have also obtained consistent results but of an opposite or a different significance, should lead him to modify the positiveness of his conclusions.

Failures of growth in experimental animals when consuming amounts of food that, regardless of its composition, would be unable to support growth, may fairly be regarded as negative evidence with reference to the nutritive value of the diet. This position is recognized in many published articles containing the results of feeding experiments not involving control of food consumption. In 1924, M. L. Mitchell[6] obtained results on mice indicating in a positive fashion that taurine can replace cystine in animal nutrition. In the following year, Beard[7] published a report of feeding experiments on mice that did not bear out this interpretation. He was, however, content to draw the non-committal conclusion that "the results here obtained do not confirm the conclusion of Mitchell that taurine can replace cystine in the diet of mice." Using similar uncontrolled methods, Lewis and Lewis[8] also obtained negative results relative to this metabolic interchangeability, stating in conclusion simply that "no evidence was obtained which indicated that either taurine or cysteinic acid could replace cystine entirely or in part for purposes of growth." However, on the basis of the same sort of evidence, Rose and Huddlestun[9] conclude that "taurine is totally incapable of replacing cystine in the diet for purposes of growth." This appears to be an exaggeration of the importance of negative evidence.

It is a fundamental requirement of rigorous experimentation that two animals (or periods) which are to be compared should differ in only one factor capable of affecting the thing measured, such as rate of growth. The rate of growth of an animal is undoubtedly affected both by the composition of its diet and by the amount consumed. Therefore, if it is desired to measure the effect of the composition of the diet only, some method of equating the food intake must be devised. If the animals are of the same size, equal food intakes may be imposed; if of different size, equal food intakes per unit of weight. In comparing the value of different proteins for growth, Osborne and Mendel[10] have prescribed the condition that the experimental animals must eat "the same amount of food in the same number of days and gain the same amount of weight, the protein factor being the only variable."

The advantages in clarity of interpretation of equating the food intakes of comparable animals may be illustrated from the article of Lewis and Root[11] on the value of nor-leucine as a substitute for lysine in nutrition. Rat 5 in a seventy-day period consumed an average of 3.87 grams of a ration containing gliadin as the sole source of protein and gained an average of 0.14 gram daily. In a subsequent forty-two-day period it received a lysine supplement to this diet and

4 On the same basis, Osborne and Mendel (*J. Biol. Chem.*, 1917, xxxii, 369) might have concluded that soybean proteins were inadequate for growth, since in their first experiments the soybean rations were not readily consumed and little growth resulted. It was later found that the rations were consumed in adequate amounts if the soybean meal were previously subjected to heat in the presence of water. Without changing the nutritive value of the ration to any considerable extent, normal growth was now produced with it.

5 Ackroyd, H., and Hopkins, F. G., *Biochem. J.*, 1916, x, 551; Geiling, E. M. K., *J. Biol. Chem.*, 1917, xxxi, 173; Stewart, C. P., *Biochem. J.*, 1925, xix, 1,101.

6 Mitchell, M. L., *Australian J. Exp. Biol. and Med. Sci.*, 1924, i, 5.

7 Beard, H. H., *Amer. J. Physiol*, 1925, lxxv, 658.

8 Lewis, G. T., and Lewis, H. B., *J. Biol. Chem.*, 1926, lxix, 589.

9 Rose, W. C., and Huddlestun, B. T., *J. Biol. Chem.*, 1926, lxix, 599.

10 Osborne, T. B., and Mendel, L. B., *J. Biol. Chem.*, 1916, xxvi, 1; 1919, xxxvii, 223.

11 Lewis, H. B., and Root, L. E., *J. Biol. Chem.*, 1920, xliii, 79.

gained 1.29 grams daily on a food intake of 5.81 grams per day. Finally, in a thirty-five-day period in which it received nor-leucine as a supplement, its average daily gain was .07 gram and its daily food consumption 4.94 grams. While it may be quite probable that the increased rate of growth in the second period, as well as the increased food consumption, was due to the fact that lysine, but not nor-leucine, supplemented the gliadin in metabolism, this can not be considered a demonstration until the rates of growth on 5.81 grams (or some equivalent amount) daily of the basal ration and of the basal ration plus nor-leucine have been determined. With Rat 6, the difference in supplementing value of lysine and nor-leucine is more clearly shown, because with lysine an average daily gain of 1.19 grams was secured and with nor-leucine one of only .06 grams on food intakes practically identical, *i.e.*, 5.90 and 5.86 grams daily. In the case of comparing the results on the same animal in successive periods, it is undoubtedly better to equate the food intakes not absolutely, but in proportion to body weight, and, as Lewis and Root observe, such an equating of food intakes in the lysine and nor-leucine periods actually resulted in most cases without deliberate control. As a general plan, however, how much better it would be to assure by deliberate control, rather than to leave to chance, such an essential requisite of effective experimentation!

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AMOEBA DOFLEINI (NERESHEIMER) VS. MAYORELLA BIGEMMA (SCHAEFFER) A CASE OF SYNONYMY

E. NERESHEIMER (05)[1] described and fully illustrated a rhizopod to which he gave the name Amoeba dofleini. This description agrees in all the essential details with the diagnostic characteristics of a rhizopod described by Schaeffer (18),[2] as a new species under the name of *Amoeba bigemma,* which was afterwards changed by him (26)[3] to Mayorella bigemma.

[1] Neresheimer, E., 1905, "Über vegetative Kernveränderungen bei *Amoeba Dofleini* nov. sp. Arch. fur Protist," Bd. 6, S. 147–165.

[2] Schaeffer, A. A., 1918, "Three New Species of Amebas: *Amoeba bigemma* nov. spec., *Pelomyxa lentissima* nov. spec., and *P. schiedti* nov. spec.," Trans. Am. Mic. Soc., Vol. 37, pp. 1–18.

[3] Schaeffer, A. A., 1926, "Taxonomy of the Amebas," Carneg. Inst. of Washington, Vol. 24, pp. 1–116.

Schaeffer, of course, before publishing his description, made a survey of the literature and for sufficiently good reasons was unable to reconcile the descriptions of Mereschkowsky (79), Parona (83), Fromentel (74) and others with the rhizopods he described, although with respect to Doflein (07), he says, (26), p. 56, "Doflein's figures as to the character of the pseudopods, the size, etc., of the body, agree closely with the amoeba I described, Schaeffer (18), under the name *bigemma*. . . . Consequently I incline to think that Doflein worked with *bigemma*. . . ." Since Schaeffer does not quote Neresheimer I presume he has merely overlooked his work. As stated above Neresheimer published his work in 1905, and, as the name of his animal signifies, it was named after Doflein, with whom Neresheimer worked. Doflein published his investigation on *Amoeba verspertilio* (Penard), the animal Schaeffer thinks is his *bigemma,* in 1907, two years later. This means that Doflein regarded the rhizopod that he worked on as distinct from the one that Neresheimer dealt with, for he must certainly have been familiar with Neresheimer's animals. If this is correct, then it seems that Schaeffer is possibly at error in thinking that his animals are the same as those of Doflein, provided the evidence given below is sufficient.

Neresheimer's description of the outstanding characteristics of Amoeba dofleini follows in roman type and Schaeffer's diagnosis of *Mayorella bigemma* in italics.

Size: 80–150 microns (the larger size more prevalent).
100–300 microns in locomotion.

Form: Assumed wide changeability of form.
Very changeable.

Pseudopods: Broad, short, broken sac-like pseudopods barely extended from the body.
Numerous, tapering, blunt, never with sharp points.

Surface: Without the characteristic ectosarc folds of *A. verucosa.*
Smooth, no fine folds or ridges.

Endoplasm: Contains bar and rarely dumb-bell-shaped crystals from which hang little spheres.
Usually containing numerous small twin crystals: crystals attached to "excretion spheres."

Movement: Lively, brisk.
Rapid, about 125 microns per minute.

Nucleus: A round or oval vesicle, 20 microns in diameter, firm bodies in the form of little masses gathered together around the periphery of the karyosome.
Single, round or slightly oval, about 12 microns in diameter (sometimes as large as 28 microns), chromatin in small masses clumped loosely together in the center of the nucleus in a nearly spherical mass about 6.5 microns in diameter.