

1939, died September 13 at his home in Princeton after a long illness.

Selig Hecht, 55, professor of biophysics, Columbia University, and a well-known authority in the field of vision, died September 18 of a coronary thrombosis at his home in New York City.

COMMENTS

by Readers

The British Association for the Advancement of Science, under the auspices of its Division for Social and International Relations of Science, on May 10 held a conference on "The Place of Universities in the Community." The following resolution, proposed by Sir Henry Dale, president of the Association, and passed unanimously, has since been approved by the Council: "This Conference recommends to the Council of the British Association that they should make representations to the competent authorities of all Universities of the British Commonwealth, urging that no contract should normally be accepted by the Scientific Departments of these Universities, if its terms include any restriction on freedom of publication."

Ludwig Silverberg remains unlocated in Europe. A recent search failed to disclose his whereabouts. Dr. Silverberg left Germany in 1936, and until recently his address has been "The Hague, Netherlands." Since further information is desired concerning some of his inventions, any assistance which can be given in locating Dr. Silverberg would be greatly appreciated. Communications should be addressed to: Reed Research, Inc., 1048 Potomac Street, N.W., Washington 7, D. C.

Make Plans for—

American Public Health Association, October 6-10, Atlantic City, New Jersey.

American Association of Cereal Chemists, New York Section, October 7, 2 Park Avenue, New York City.

American Academy of Ophthalmology and Otolaryngology, October 12-17, Chicago.

American Association for the Advancement of Science, 114th Meeting, December 26-31, Chicago, Illinois.

This communication is written in the hope of opening a constructive discussion of the editing of scientific papers. That the subject deserves airing is certain from the continued grumbling we all hear and often swell. As editor of several journals and contributor to many more, I have both sinned and been sinned against. At the moment, several adventures in publication have put me more on the sinned against side, which prompts this note. It will be restricted to the problem of form; content is an independent subject.

Every journal is entitled to insist on certain style uniformities. If a certain spelling of a word (fiber rather than fibre) is preferred, if particular abbreviations are used or rejected (hr. for hour, but not % for percent), if a particular form of literature citation is demanded, the author who objects should submit his paper elsewhere. Good sense should prevent an unyielding application even of such rules—"Hrs. later conduction failed," or even, "After hrs. . . ." would hardly do; but the editor is arbiter of usage. Further, no author will complain about correction of errors in spelling, punctuation, grammar, citation, fact (providing they *are* errors); and few, about *suggestions* of improved sentence structure, or organization, or even the nuances of style. But here the pendulum swings over, and the author, not the editor, has become final arbiter. The editor may only suggest.

An editor may properly reject a paper solely on the basis of obscure or verbose presentation, or may offer to accept it subject to improvement, or may generously detail the revisions demanded for acceptability and others recommended for improvement of the presentation. The decision is then the author's: to submit his paper elsewhere, to sweat over it further (perhaps with the aid of a more literate colleague), or to welcome the editor's proffered help and accept those revisions which seem good to him. There is one absolute essential in editor-author relations that I am saving for the "punch line"; with that operative, the balance indicated can work admirably.

I know editors who have performed minor and major surgery on hundreds of manuscripts; who have, in special cases, as when highly important results were poorly presented, literally rewritten an article completely. (One condensed, by rewriting, a 75-page manuscript, by a man senior in age and achievement, into one of 23 pages.) And no anguished cries resulted. On the contrary, it was generally true that the more drastic the operation, the more grateful the subject. The reason? Adherence to the critical condition.

I know authors who have been awed to find an incorrect page number in an obscure reference questioned by the editorial office; who have been delighted to have headings of comparable tables recast into comparable form, and pleased to note correction of misspellings or of infelicitous constructions; who have been sadly tolerant, mostly, when commas have been deleted or adjectives substituted (for some sprinkle their commas and choose their words with considerable care, and the results are as intended); and who have been, sometimes, extremely annoyed when sentences have been recast, material omitted, style altered, meaning changed. Why annoyed? Because the critical condition was *not* always adhered to.

The editor should have final say as to what appears in his journal. The author should have final say as to what appears under his name. Between reasonable men agreement is readily attained, when adequate opportunity exists for exchange of reasons and preferences. In a case I know of, an invited article in a symposium on a highly controversial issue, the editor made many and serious changes from the original manuscript. The author restored the original form in galley proof and wrote a full explanation of his wish to retain his presentation. This letter was not acknowledged and some of the objectionable editorial changes remained in the published article.

By all means, have the editor "vet" each manuscript. Let him alter it to the best of his ability and his available time. Let him indicate which changes are demanded if

the article is to be accepted, which are judged highly desirable, which are minor suggestions. (All this is also applicable to content of an article, although this is less often altered.) And then, if any question of the acceptability of the changes to the author be possible, *insist that the edited manuscript, not proof, be returned early to the author for his consideration.* (R. W. GERARD, *Department of Physiology, University of Chicago.*)



By the regulated action of sulfuric acid paper is converted into vegetable parchment. Under similar conditions (occasionally in the presence of formaldehyde), cotton is transformed into a variety of Heberlein fabrics. Such treatment of other cellulose fibers, e.g. esparto, flax, jute, sisal, plywood (prior to folding), etc., would lead to a wide range of new materials possessing useful and attractive properties. Increased tensile strength, waterproofing, and general durability should add to the value of thread and plywood and result in a further development of textile and constructional material. In the case of wood, owing to its lignified condition, some preliminary treatment may be necessary, although not so drastic as that employed in the purification of wood pulp. The single fine layers of plywood should respond readily to such processing without impairing its original mechanical structure. (MAURICE COPISAROW, 1 Gildridge Road, Manchester, England.)



Recent reports on the results of glucose tolerance tests in vitamin-deficient animals (O. H. Gaebler and W. E. Ciszewski. *Endocrinol.*, 1945, 36, 227; A. Chesler, E. Homburger, and H. E. Himwich. *J. biol. Chem.*, 1944, 153, 219; S. Banerjee and N. C. Ghosh. *J. biol. Chem.*, 1947, 168, 207) do not refer to a series of papers published by McCay (D. McCay, *et al. Indian J. med. Res.*, 1916, 4, 1-27; 1918-19, 6, 485-549; 1919-20, 7, 22-80, 81-147). I came across McCay's papers fortuitously, and it seems probable that the later authors were not aware of the vast amount of work that had been done on human subjects at an earlier date. Nor have I been able to find any reference to these studies in

books, reviews, or textbooks that might be expected to mention the work. Since the studies were carried out with large numbers of patients and occupy several hundred pages of journal space, with many fully charted case reports, it is obviously desirable that they be called to the attention of modern nutritionists and physiologists.

McCay reported many cases in which patients who were losing 50 to 150 grams of glucose daily in their urine were made aglycosuric within 2 or 3 days. The patients were first restricted to a diet of milk, vegetables, and butter for a few days: "One to two pints of milk, 12-16 oz. of green vegetables, and 2 oz. of butter for 3 days will clear the urine of sugar and the blood of hyperglycemia in 99 per cent of all cases" (*Indian J. med. Res.*, 1919-20, 7, 81-147). The diet was then slowly built up until the caloric content was adequate, and eventually the patients were allowed to increase the rice in the diet. However, it was also stated that "in the treatment of diabetes in India the value of green vegetables cannot be too highly appraised."

One might surmise that the patients McCay studied were deficient in one or more vitamins. Certainly his findings should be confirmed or denied in terms of modern nutritional knowledge. If any assessment has, in fact, been made, no doubt others would like to hear of it. (VAN R. POTTER, *University of Wisconsin Medical School.*)



Egg number is a matter of interest in fishery biology, especially through its relation to mortality rates. The maximum usual number of ripe eggs in an individual of *Ostrea virginica*, estimated at 50,000,000-60,000,000 by W. K. Brooks (*Rep. Comm. Fish. Md.*, 1880, p. 14) and T. C. Nelson (*Bull. N. J. agric. exp. Sta.*, 1921, 351, 8), has more recently been placed at 500,000,000 by P. S. Galtsoff (*Science*, 1930, 72, 97-98). Computation of the volume represented by various numbers of eggs suggests that the order of magnitude of the later estimate may be too high.

Galtsoff's value was based on an estimate of 115,000,000 eggs discharged at a single spawning. The gonad of this medium-sized individual was still 7 mm. thick after this discharge and was considered to contain sufficient ripe ova for

the spawning season's estimated four or five further periods of oviposition. However, 115,000,000 eggs 0.05 mm. in diameter (W. R. Coe. *Biol. Bull.*, 1932 63, 428) would amount to a solid mass of about 8 cc. and would presumably require an ovary of about double this volume.¹ From the data of C. Grave (*Rep. Shellfish Comm. Md.*, 1912, 4, 42) on cubic content of oysters, it would appear that Galtsoff's individual probably had a total body-volume of only about 15 cc. Therefore, it seems likely that the number of eggs discharged was miscalculated, perhaps by misplacing a decimal point.

A layer of gonad 7 mm. thick would be unusually great, since V. L. Loosanoff and J. B. Engle (*Bull. U. S. Bur. Fish.*, 1937, 33, 230) report a maximal average thickness of 9 mm. for any 6-oyster sample on any date and an average of only 4 mm. for 19 samples on the best date. The volume of a gonad 7 mm. thick in an oyster 3.7 inches long seems unlikely to have been as much as 10 cc., and the volume of packed eggs which it could have accommodated would probably not be as much as 5 cc. Therefore, if (as he evidently believed) Galtsoff's oyster had discharged only about one-fifth of its ripe eggs, the absolute quantity discharged might well have been only 0.8 cc., or 11,500,000, instead of 115,000,000.

The solid volume of a half billion eggs would be about 2 cubic inches. About 110 such masses would fill a gallon. Allowing for looser packing in the ovary and for the nonovarian content of the animal, it seems probable that an oyster of size to contain 500,000,000 eggs would be one of some 40/gallon at smallest. The largest recognized commercial category of oyster ("extra large") is defined at 160/gallon. Galtsoff's oyster was evidently a "select" of about 270 count.

It is, of course, possible that, by continued gametogenesis after the inception of spawning, an oyster might during a single season produce eggs in excess of its own volume. However, available reports do not make clear to what extent this might occur in *Ostrea virginica*, and this possibility was evidently not the basis of Galtsoff's estimate. (MARTIN D. BURKENROAD, *Survey of Marine Fisheries of North Carolina.*)

