Stability can be achieved here, at the level of the species, relatively readily, although there must remain numerous problems of synonymy, inadequate description, misidentification, etc. When Dr. Wald states that, "The most important thing about a name, after all, is that it remain attached to the thing it designates," he is thinking chiefly of the naming of species. But there is a second and more basic aspect to this problem, and that relates to *taxonomy,* or *systematics,* which deals with the evolutionary relationships of organisms. And it is in this respect that stability of nomenclature, as it affects genera and higher categories, cannot be asked for except at the cost of a static systematics. One can no more ask that generic names be stabilized entirely than he can ask that atomic weights or other physical constants be rounded off to integers or be not subject to revision. Major efforts to stabilize nomenclature are currently going on, but absolute stability is neither possible nor desirable.

Actually, most of the name changes which plague the experimental biologist are not changes of specific names, but revision and reordering of genera and higher groups in the attempt to evolve a more natural classification—the same goal as that of the biochemical evolutionist. The change from *Dolichoglossus kowalevskyi* to *Saccoglossus kowalevskyi* is a case in point, representing an advance in the understanding of the group as a whole. If all workers were to use the specific name, *kowalevskyi,* in their papers, and also the name of the describer (A. Agassiz), much of this confusion would be avoided. The basic rules governing such changes are no more complex than, say, those for the naming of organic compounds, and can be (and often are) covered in elementary biology courses or learned in less than an hour. There are, of course, many historically tangled nomenclatural problems requiring study by experts and suspension of the rules for their resolution. The point I wish to make is that *most* of the nomenclatural problems affecting nonsystematists are not so complex and could be explained easily in a three-line footnote. Proper use of specific names in experimental papers is likewise an essential part of the task of keeping confusion at a minimum. It should be realized that an older name can be perfectly understandable and in a sense valid, if stated in proper form. Thus the names *Platynereis megalops* (Verrill) or *Nereis limbata* Ehlers, if applied to animals at Woods Hole, introduce no confusion in spite of the fact that recently some systematists feel these to be synonyms of the earlier-described *Platynereis dumerilii* (Aud. and M. Edw.) and *Neanthes succinea* (Frey and Leuckart), respectively. If the latter, less well-known, names are used, the insertion of a brief note will make the situation clear to the general biologist.

At this point the physiologist may well ask how an experimentalist is to know whom to consult (as among physiologists, there are not only specialists among taxonomists, but good and bad taxonomists as well). How can he be sure of getting a simple, clear, and conservative answer, rather than a lengthy, overdetailed, and pedantic discussion? How can he avoid overhasty or poorly supported name changes? Obviously the taxonomist who advises an experimentalist must have a sense of responsibility in furnishing a succinct and clarifying explanation in cases where confusion exists.

The general problem can be met if those concerned wish to take simple steps to avoid lack of understanding and confusion in the future. Indeed, some positive steps need to be taken at once, if progress in the newer fields of biochemical evolution and comparative physiology is to interact to mutual advantage with advances in the older field of systematics. The step I would propose is twofold: First, for the editorial boards of journals in experimental and general biology to insist that organisms which are the subject of investigations be properly named (including species, if identifiable, and the authority), with a brief footnote clearly stating any outstanding synonymy. This is in line with the common requirements that statistical work be checked. Second, for the Society of Systematic Zoology to recruit small panels of broad-minded systematists who would undertake to verify or to furnish upon request these explanatory footnotes. This young but active society could readily supply the small but vital amount of taxonomic consultation necessary. The general problem is one that could advantageously be made a topic for discussion and action by the American Society of Zoologists at forthcoming meetings. It is not a partisan matter, but a common need of modern biologists.

RALPH I. SMITH

Department of Zoology
University of California, Berkeley

The Atlantic Estuarine Research Society

ONE day in April 1949 a group of 22 young scientists met in Morehead City, N. C. They had received their training in widely separated parts of the United States but now had a common interest—they were engaged in research related to the important fisheries of Chesapeake Bay, the North Carolina sounds, and their estuarine tributaries. Among the group were biologists, working chiefly with the oyster, the blue crab, the shad, and the croaker, and physical and chemical oceanographers, occupied with problems concerning the circulation of these semienclosed bodies of water, and with the exchange of water and dissolved substances between the rivers and the sea.

At these informal discussions it was generally agreed that the objects of the diverse investigations were ecological in nature. Furthermore, it was apparent that many unique problems were represented, for the fishery resources of this region are exploited almost entirely within estuarine waters. In almost no other region in the world do estuarine waters produce so much protein food.

Concerned with the scarcity of knowledge of the

chemistry, physics, and biology of such enclosed waters, and faced with the need for discussion of mutual problems, the group organized the Atlantic Estuarine Research Society. Enthusiasm, informality, and active participation by all members are keynotes of the organization. Its growth has been rapid, partly because research activities have been expanded in the area, and also because others outside the states of Maryland, Virginia, and North Carolina have become interested.

The stated purpose of the society is to exchange ideas and knowledge, and to stimulate free and informal discussion on estuarine ecology. Membership is restricted to scientists, whatever their field of interest, who are carrying on active research on estuarine problems. Meetings are held twice yearly, in spring and fall, and are restricted to Maryland, Virginia, and North Carolina.

The informal atmosphere is fostered by meeting at the various research laboratories. The membership believes that the strength of the organization lies in its local character, relatively small membership, and frequent meetings. Evidence that these restrictions do not impose a lack of breadth on the society is revealed by the biographies of the 73 active and 8 honorary members, who hold degrees from many different colleges and universities in North America, from the Pacific to the Atlantic coast, and from Canada to the Deep South.

JAY D. ANDREWS
Virginia Fisheries Laboratory, Gloucester Point

Use of Sodium Metabisulfite as a Preservative for Grass Silage[1]

SULFUR dioxide has been used extensively as a preservative for grass silages during the past few years, apparently with considerable success. Satisfactory preservation of the nutrients of the grass is obtained by addition of 5 lbs of gaseous SO_2/ton of fresh material, producing highly palatable, good quality silage at a considerably lower cost than that resulting from the use of other preservatives.

When sulfur dioxide is used in the preservation of foods, it is generally applied as an acid sulfite of calcium or sodium, in powder or crystalline form, which is more convenient to work with than gaseous or liquid SO_2.

With this in mind, preliminary experiments were conducted during the summer of 1951 to investigate the use of a concentrated water solution of sodium metabisulfite as a preservative for grass silage. This material is available in large quantities, at approximately half the cost of liquid SO_2 (per unit of SO_2). A variety of grasses and legumes were ensiled in 1-gal glass jars fitted with rubber stoppers and water traps arranged to allow seepage and escape of gases and to exclude air. In every case the silages preserved with $Na_2S_2O_5$ kept at least as well as comparable materials treated with SO_2 gas at equivalent levels, and much better than untreated control samples.

In addition, a small experimental silo ($3' \times 8'$) was filled with third-cutting alfalfa (pure stand), to which sodium metabisulfite was applied in water solution at a rate of 8 lbs/ton of fresh material. The product removed from the silo at the end of 3 months was an apparently excellent quality silage, of a color similar to that of the fresh alfalfa, possessing a clean, acid odor. This silage was eaten with considerable relish by a group of sheep over a period of more than a month, at a rate of about 8 lbs/day/100 lbs live weight.

It is recognized, of course, that these results are too meager to warrant recommendation of this product for use by farmers as a general practice. However, the experiment resulted in adequate preservation of the forage crops treated. Further work is planned to investigate the use of sodium metabisulfite in powder form under different conditions.

R. L. COWAN
J. W. BRATZLER
R. W. SWIFT
Department of Animal Nutrition
The Pennsylvania State College

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The Evidence for Mitotic Spindles in Bacteria

IN a recent report in this journal, of a meeting of the National Academy of Sciences, DeLamater (*1*) puts forward a claim to describe typical mitotic spindles in bacteria. This work was published simultaneously in a variety of journals, for the most part with identical photomicrographs (*1–5*). It is particularly worthy of notice, in view of the positive nature of DeLamater's claims, that all of these papers are illustrated by the same photomicrograph of what is alleged to be a metaphase spindle, and that in nearly all of them it is the only example shown. This figure is compared with others, published by other workers, of the same genus (*Bacillus*) in Fig. 1: 1–5, showing clearly that DeLamater's interpretation is fallacious. The supposed "centrioles" are merely the strongly stained granules at the junction of the cell wall and cross-walls of the bacillus; these cross-walls and septa, with which bacilli of this type are liberally provided, are clearly described in three standard monographs upon bacterial cytology (*6–8*). It is remarkable to be obliged to record that DeLamater, despite repeated quotation of these studies in his list of references, makes the elementary mistake of regarding *Bacillus megatherium* as a single, multinucleate cell. The present writer has repeatedly drawn attention to the misconceptions that have arisen from failure to recognize this multicellularity in bacteria (*6, 9, 10*).

DeLamater's failure to recognize the true nature of the structures he describes is due to his exclusive employment of a technique of dehydration in freezing alcohol which, he claims, preserves the material unaltered. He provides no controls of undehydrated