TABLE 1

VASCULAR VOLUME CHANGES IN THE DOG EAR FOLLOWING INTRA-ARTERIALLY INJECTED ADRENERGIC HORMONES

Hormone	Degree of constriction*			
	Control	After SY 28 (2 mg/kg)	After D.H.O. (0.2 mg/kg)	After both agents
With anesthesia				
Epinephrine	21.2 ± 7.1	7.3 ± 2.1	3.4 ± 2.4	0 ± 0
Nor-epinephrine	13.0 ± 2.8	2.9 ± 2.1	2.0 ± 1	1.0 ± .2
Without anesthesia				
Epinephrine	23.4 ± 6.8	24.3 ± 10.6	22.1 ± 6.8	19.3 ± 7.6
Nor-epinephrine	15.8 ± 14.2	13.7 ± 8.4	18.5 ± 8.2	15.3 ± 2.9

* Each unit represents 0.1 mv change in output of photomultiplier tube.

that the author could determine. Occasionally biphasic responses are obtained in which a slight and brief dilatation precedes the constriction.

The inability of adrenergic blockade to prevent constriction in cutaneous vessels is very striking, and this is true in spite of the fact that the blood pressure response to intravenously injected epinephrine is reversed equally in both anesthetized and unanesthetized dogs.

It seems that the normal body is able to sensitize cutaneous blood vessels to either the constrictor or dilator effects of epinephrine. This control is lost in experiments on denervated vessels and in some experiments on anesthetized animals, where it is also possible to depress the constrictor effect by adrenergic blockade. However, the unanesthetized dog apparently neutralizes the effect of the blocking drug to some extent by sensitizing the vessels to the constrictor effect of epinephrine.

Further study of the vessels in other tissues is being made in order to explain these phenomena.

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Comments and Communications

Cooperation between Systematists and Experimental Biologists

IN THE recent excellent collection of papers making up the Michaelis memorial volume, *Modern Trends in Physiology and Biochemistry* (New York: Academic Press [1952]), produced by the staff of the physiology course at the Marine Biological Laboratory, Woods Hole, there appears a footnote (p. 339) by Dr. Wald which poses a problem and a challenge to those who would like to see a healthy cooperation between experimental biologists and their fellow-workers in taxonomic fields. This footnote, although extremely humorous and to the point, reflects a widespread, although by no means universal, state of mind among experimental biologists, and, indeed, complaints of this sort have of late become as familiar around Woods Hole as the cries of the sea gulls, but not so easily ignored. The gist of the difficulty seems to be that repeated changes in the names of animals long used in experimental work have caused so much confusion that busy physiologists simply can no longer follow them and might as well ignore them. The examples cited of the mandrill and Guinea baboon, and of *Limulus* versus *Xiphosura,* hardly represent contributions by taxonomists to a stable nomenclature, but to conclude from such extreme cases that name changes in general must be deplored would seem to indicate that physiologists are not fully aware of the problems of the systematist, nor of the conventions of zoological nomenclature. It is equally true that on numerous occasions systematists have revised the names of animals in very common experimental (or commercial) use without publishing clearly in journals accessible to experimentalists the reasons for the changes.

The problem expressed by Dr. Wald affects experimentalists and taxonomists alike, and at some risk of being caught in the ensuing cross fire, I shall try to point out certain reasons for the present lack of cooperation, and to suggest a positive step toward a lessening of the existing confusion. Not being a taxonomist, I should make clear that I am interested, not in the oversimplification of genuine nomenclatural problems, but rather in promoting a workable and beneficial relationship between experimentalists and taxonomists.

Experimental biologists should realize that there are two very different aspects of the problem of naming organisms. One is the matter of *nomenclature,* which is at its simplest the task of assigning a name to each distinct species of plant or animal.

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.

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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