lagging and ejected material. Moreover, true plasmosomes are present in the spermatocytes and spermatids of *D. melanogaster*. Finally, the conduct of the lagging and ejected material is precisely that found in many known hybrids.

It is thus clear that *D. melanogaster* has so many of the characteristics of a hybrid that only its duplication by the experimental crossing of recognized species could supply additional evidence in this direction. It is further indicated that it is time that we had surcease of speculations on the part of geneticists as such on the general problems of evolution and the origin of species. No amount of Mendelian moiling with the stabilized variants of a mutable species is likely to throw any permanently valuable light on the question of the origin of species. The real problem is clearly that of the origin of mutability in modern types of plants and animals, a matter upon which the conscientious and even contentious elaboration of the laws of Mendel throws no light whatever. Cytology and the experimental crossing of species are obviously destined to lead the way to new and fundamental advances in our knowledge of the origin of contemporary species of plants and animals.

IN a recent number of this journal[3] Professor Huettner passes some criticisms on my work on *Drosophila melanogaster*. These are largely expressions of the dis-esteem of himself and his group of Drosophilists for my work on this species and includes some quite erroneous statements in regard to my qualifications. As these matters are of little scientific interest, it will be well to refer to the only significant feature of his paper, namely, the question he raises as to the lagging of the chromosomes in the species under discussion. He commends strongly in this connection the use of Feulgen's reagent for the identification of the real chromosomes. Putting aside the question as to the extreme abnormality of the reduction division in *D. melanogaster* as in itself a suspicious circumstance, my critic is referred both to the article cited above and more particularly to a recent paper by Woskressensky and Scheremetjewa on spermatogenesis in *D. melanogaster* published in the *Zeitschrift für Zellforschung und mikroskopische Anatomie*. The latter authors use the method of Feulgen, and their figures show a very large amount of lagging in the chromosomes of the species as diagnosed by this method. Further, their account agrees with that of the present writer in respect to the multiplication in number of the chromosomes beyond that to be expected from the somatic conditions. The tide in regard to the interpretation of *D. melanogaster* has apparently definitely turned, as the newer literature has abandoned the contemptuous attitude which has been in general adopted by Drosophilists on this side of the Atlantic.

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[3] SCIENCE, 71: 241, February 28, 1930.

SCIENTIFIC NAMES IN ZOOLOGY

A VERY considerable number of interested zoologists believe that real progress is being made in zoological nomenclature. A sane use of the highly valuable principle of priority is being made, despite the unwillingness of an ultra-conservative group to adopt any change from the nomenclature it is accustomed to and therefore regards as the proper one, and despite the objections of another group now also being forced into a conservative position by the progress which is being made. This second group would insist on the resurrection of any available name which has priority, no matter how much inconvenience the change would introduce. The International Commission on Zoological Nomenclature is in general steering a median course, by using its plenary power very wisely in eliminating from the rule of priority those occasional names which would clearly create a really wide-spread confusion and instability, and in consistently refusing to pass favorably on cases in which the replacement of the established name affects only a few systematists or in which the desirability of the replacement is clearly debatable or in which the proposed change involves taxonomic judgment rather than the application of the rules.

Those of us who view the progress in nomenclature more or less optimistically are perturbed by the expression of views which can only serve to obstruct the advance which is being made. We feel that such an article as that of Professor Needham[1] is particularly reactionary. Since this account is well and boldly written and since a considerable weight of authority is carried by the author, the remarks of Professor Needham are meeting in some quarters with a reception which we believe to be unjustly favorable. We feel constrained to attempt a reply.

Needham's "smouldering impulse first burst into flame" over some modern names to which he objects. Of course not all names are of equal euphony or brevity, and it is possible to search out some which are flagrant examples of poor style. Zoologists alone are not responsible for such unpleasant productions. They blight the terminology of other sciences. Indeed, some words in common use are not simple.

The discussion of these long names of the day by Dr. Needham gives a very unfair picture. Many modern names are a joy to the zoologist and the classicist alike; many old names are badly constructed and long. But that two names of record length "are far worse than anything pre-Linnaean" is not the truth. The first name quoted belongs to a set which the In-

[1] SCIENCE, 71: 26–28, 1930.

ternational Commission has recently specifically outlawed, in Opinion 105 (published June 8, 1929, with preliminary notices in 1927)—a fact of which our critic was either ignorant or which he ignored. The second name is further elongated by Needham himself by the insertion into its middle of three more letters, thus carrying it so far from bad to worse that the senior author of the name may perhaps be happy that Needham added a letter to his name also. Some of the names which Needham holds up to ridicule apparently sounded and looked bad to him because of their Slavic form, and to this degree his course is surely unjustified.

The impression is given by Needham that such awkward, bestretched names are very general: "If any one thinks that such monstrosities in names are isolated cases let him read the four pages of generic names derived from personal names in Palmer's 'Index Generum Mammalium.'" But the very names Needham cites are notorious exceptions; in fact, they are the very ones I myself have quoted in objecting to such names. We therefore can not regard such abuses as being at all common. The International Commission has refused to validate a group of crustacean generic names of which the one quoted by Needham is an example. On account of the circumstances of the case, I think the commission was justified in this arbitrary action, the first of its kind ever taken. But to outlaw inelegant names except in most unusual conditions would be dangerous and unjust, and would discredit the commission out of the authority it now commands among systematists.

It is amusing to note that men like Raffinesque a century ago replaced names which appealed to them as too long (or too short), but for such an action to be advocated to-day is an unpleasant anachronism.

Needham's critical attack appeals to us as essentially destructive. He does, however, make some suggestions, of very unequal promise. The general proposal for the erection of a new body to pass on taxonomic as well as nomenclatorial evidence and so build up a "standard name list" strikes us as very unwise and dangerous. Such a body would certainly command at most very limited and ephemeral authority. Active systematic zoologists as a whole would certainly not tolerate an effort to subjugate individual scientific judgment to an ordained system of names.

We trust that Dr. Needham will not deny that scientific judgment does enter into systematic work. We trust that he knows that even supposedly very well-known species are often found to be complexes of two or more species, or to be wrongly classified. We trust that he appreciates that accurate species identification is essential to sound comparative work in almost any field of zoology. We trust that he realizes that the fine distinction of forms is especially conducive to new interpretations in such fields as ecology, zoogeography and economic zoology. To have a fixed system of names for others than systematists would make the advances of systematists unavailable to other zoologists and would inhibit broad cooperative progress in the whole field. The suggestion for a name-fixing body seems to us little better than the discarded scheme for numbering species.

The proposal to submit our problems of nomenclature to psychologists will certainly hold little interest. The proposal for botanists and zoologists to cooperate may be worth considering, when the botanists among themselves agree to one code of rules. The proposal for "members-at-large in the name-choosing body, to secure a measure of uniformity" expresses a poor appreciation of the function which an International Commission on Zoological Nomenclature should (and has) assumed. The proposal to have members of the name-choosing body serving only for the groups in which they are taxonomic specialists, to bring to bear a working knowledge of the group and of its literature and tradition, would involve a commission of unwieldy size and overlooks the fact that the present commission ordinarily does submit cases to specialists in the group involved. The specific "tradition" of one group ought not to bear on questions of nomenclatorial rules, which are rightly the primary concern of the International Commission. The proposal for "Some better method of obtaining the opinion of zoologists than the *viva voce* vote of the crowded sessions of an international congress" is accompanied by no mention of the fact that the secretary of the International Commission makes strong efforts to bring proposed cases before the general zoological public for its opinions.

That the existing system of zoological nomenclature is in many ways crude and in need of revision is undeniable. That scientific names are so confused, absurd or appallingly long as to dull the interest of the pupils of conscientious teachers is a very minor pedagogical objection, if true. That "good judgment, expert knowledge, human sympathy, hard labor and long patience" will be required "to find a way out" is granted, but we need not completely ignore the fact that some degree of these noble qualities has already been mixed with the almost fiendishly egotistical passion of systematists (as pictured to us), in building up the nomenclature and the rules which hold to-day.

Needham is of course not alone in longing for a simple system of nomenclature. Most of us do. How to obtain such an end is a common question. The

solution is obviously, as Needham now admits, not to be found in any such device as numbering species. The use of quadrinomials I also object to; it is confined to few authors and to few groups of animals, and is not recognized by the International Rules, to which Needham makes too little reference. The use of trinomials to designate incompletely differentiated forms most systematists believe to be sound and unavoidable, and when logically applied leaves the binomial specific name available for the use of those who do not need or those who do not care or those who are unable to split the subspecies of the species in question.

Thus the evil, or virtue, of subspecies "splitting" need not worry those who long for a simple nomenclature (even if the simplicity be artificial). The splitting of species, when sound, unavoidably alters the scientific name and can be ignored only through ignorance or arrogance. The splitting of genera into subgenera need not worry the worshipers of brevity, for there is no need for quoting the subgenus in the scientific name. But the splitting of genera into smaller genera does alter the name. So does the transfer of species from one genus to another. The Rules of Nomenclature have no primary application to such taxonomic changes. These changes are at the base of an ever-increasing proportion of the unfortunate shifting of names. Fewer and fewer alterations are due to the uncovering of overlooked available names or to alteration of the species interpretations.

It is becoming increasingly clear that these name changes, due to genus splitting or shifting, are the chief concern of those who long for a stable nomenclature. It is unfortunate that the changes in genus concept should alter the scientific name of an animal. The fault lies in the binomial system of nomenclature. This system confounds classification, which ought to be flexible, with nomenclature which should be fixed. A uninomial system of animal names would divorce classification from nomenclature and would presumably emphasize the fact that the species is the most natural and objective of all systematic groups. It would certainly shorten animal names. The uninomial system has been found workable in mineralogy, chemistry and astronomy, and would have many advantages in zoology.

I do not propose the present adoption of any uninomial system of zoological nomenclature. I do emphasize, however, the facts that the tendency to split has continued, despite occasional set-backs by lumpers, from the time of Linnaeus until the present; that in some groups the splitting of genera has gone so far as to produce a high percentage of monotypic genera; that for such groups there is a tendency, in conversation or in general works or in the frequent repetition of the name in technical papers, to allow the generic name to stand for the whole scientific name. We are to this degree now heading toward a uninomial nomenclature of animals. That this system will be gradually and eventually adopted I venture to predict. If the uninomial system is not accepted, or until it is, I see no hope for ever arriving at a really stable nomenclature. In the meantime we can devise ways of surviving without this stability.

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SEA-LEVEL CHANGE NEAR NEW YORK

IN *Bulletin* of the National Research Council, Number 70, just issued, there is an erroneous statement. On page 35, paragraph D, it is stated that "Tidal observations at Fort Hamilton extending over a period of 35 years indicate no appreciable change in sea-level at that point during the period of observations."

As a matter of fact, the probable change in sea-level at Fort Hamilton between 1893 and 1927 is at the average rate of a rise of one foot in 214 years (by the least square method 0.0047 feet a year $\pm$ 0.06). Though the probable error of this result is great, it is more likely to be at the rate of 0.6 feet per century (.006 feet per year) as suggested by J. R. Freeman than to be with "no appreciable change."

Curiously, taking the last twenty-five years, from 1903 to 1927 inclusive, the rate would be .0055 feet a year.

The whole question deserves further consideration which we hope it will receive. For instance, M. R. Campbell's suggestion that meanders in streams flowing essentially at and below tide level are indicative of drowning, Bull. G. S. A. (1927) pp. 537–555, has a bearing.

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ASTRONOMY IN SOUTH AFRICA

THE paragraph quoted from *Science Service* in the issue of December 20, 1929, headed "Astronomy in South Africa," contains several inaccurate statements.

The large refractor of the Radcliffe Observatory has an aperture of twenty-four inches, not eighteen inches. The University of South Africa does not possess an observatory, and there is no observatory in Cape Town other than the Royal Observatory. The twenty-four-inch photographic refractor of this observatory has an eighteen-inch visual refractor on the same mounting.

Neither the University of Michigan nor the Yale University has branches in the grounds of the Union