employed, ranging between 2 and 4 years of age, show an especially large number who are underweight. In children 5 and 6 years old, the differences are not so marked.

Nature reports that at a recent meeting of the Industrial Advisory Committee of the Ross Institute, London, reports were received of the over-seas activities of the institute. Seven research centers in Assam and northern Bengal have been opened, and anti-malarial work and the testing of new drugs for the treatment of malaria have been pursued there and in Rhodesia and East and South Africa. In the Assam tea gardens, anti-malarial work has resulted in much improved health, for in 1930 among a population of 13,248 the admissions to hospital were 23,226, but in 1932 with a slightly larger population the admissions were reduced to 15,141. A standard oil mixture for killing mosquito larvae has been devised in conjunction with the Burma-Shell group. The health among lead miners in Yugoslavia was investigated and a health scheme was formulated and is now in operation. At the conclusion of the proceedings, Mr. Still and Sir Malcolm Watson addressed the meeting on the subject of yellow fever. Now that travel by aeroplane is so rapid, the grave danger that infection may be carried from the yellow fever zone in West Africa to East Africa and Asia, which would be followed with disastrous consequences, was emphasized.

DISCUSSION

ZOOLOGICAL NOMENCLATURE

PROFESSOR A. S. PEARSE, of Duke University, has recently contributed to SCIENCE[1] a number of thought-provoking reviews of recent text-books of zoology.

Professor Pearse makes, rather dogmatically, two statements in his reviews that to the taxonomist stand out as though they were printed in red ink and, though they are of distinctly minor importance, are as startlingly incongruent with the established formalities of nomenclature as red ink would be on a page of SCIENCE.

Professor Pearse makes the statement that "the scientific name of an animal consists of the genus, species *and the name of the author.*"[2] (The italics are Professor Pearse's). Had Professor Pearse said that the name of the author should be *appended* to the scientific name of an animal at least once in the publication using the name, one could agree with him, and might even walk with the International Commission on Zoological Nomenclature and add that the date of the proposal or some other clue to the original use of the name might also appear to advantage.[3]

Article 2 of the International Rules of Zoological Nomenclature states that "the scientific designation of animals is uninomial for subgenera and all higher groups, binomial for species, and trinomial for subspecies." Article 22 reads in part as follows (there is no need of quoting it in full): "If it is desired to cite the author's name, this should follow the scientific name without interposition of any mark of punctuation;—"[4] If the author's name is a *part* of the scientific name, as Professor Pearse contends, how is it possible for it to *follow* the scientific name? How can the part follow the whole?

Professor Pearse at another place[5] makes the following comment: "Evidently the writer disapproves of the modern tendency to begin even generic names with small letters, —" Let us refer to the Code. Article 8 reads: "A generic name must consist of a single word, simple or compound, written with a capital initial letter—."

Of course, a word may be used in more than one sense. The word felis, for instance, may be used as a formal scientific name, in which case it is properly capitalized. Also it may be used as a common noun, just as is its English equivalent, cat. In the latter usage it is not capitalized. Can it be this that Professor Pearse means? If so, he fails to make himself clear and has no need to appeal to a non-existent "modern tendency" for support of what has always been good usage.

Any tendency, however trivial, to flout the International Rules, the only hope we have for ultimate stability in zoological nomenclature, should bring a vigorous protest, even though the protestee is one as eminent and respected as Professor Pearse. Indeed, the more eminent the offender, the more necessary the protest.

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[1] SCIENCE, n. s., 77: 169–172.

[2] *Loc. cit.*, p. 170, first paragraph.

[3] See resolution of the International Commission on Zoological Nomenclature at Budapest, 1927, as published in U. S. Public Health Service, Public Health Reports, Oct. 28, 1927, pp. 2639–2640.

[4] International Rules of Zoological Nomenclature; Proc. Biological Society of Washington, volume 39, pp. 75–104.

[5] *Loc. cit.*, p. 170, second column, first paragraph.

THE INVOLVED GENETICS OF FISH

THE recent paper by Hubbs and Hubbs on "Apparent Parthenogenesis in Nature, in a Form of Fish of Hybrid Origin"[1] is of great interest to any one concerned with the extremely puzzling phenomena of ichthyological genetics. The matter is puzzling for the reason that although some crosses behave in normal Mendelian fashion, other crosses, like those

[1] SCIENCE, n. s., 76: 628. 1932.

recorded by Hubbs and Hubbs, refuse to conform to anything that the geneticist seems able to explain in the light of his studies with fruit-flies, birds or mammals.

For years the writer has bred tropical fish as a hobby and now has aquaria and indoor heated pools totaling 2,500 gallons capacity, with a number of sorts of hybrids under observation. Some of the facts connected therewith are worth recording.

First it should be stated that the commonest cross attempted in domestication is that of a male platy (*Platypoecilus*) of one or another of the developed color phases, with a female swordtail (*Xiphophorus helleri*). In the absence of live food, at least, the young hybrids are not produced as freely as those of pure lineage, but the former are all different from either parent, with size of mother and with color approaching or surpassing that of the father. These hybrids are known as "black helleris," "red helleris" or similarly according to color. At least when a female black helleri is mated to a pure male swordtail, approximately three quarters of the resultant young will be colorless like the father and one quarter black like the hybrid mother. The genetics of some of these various hybrids are extremely difficult to investigate, for many of them are sterile, and it is even claimed that males of some of the crosses are always so.

Hubbs and Hubbs found that when males of either *Mollienisia latipinna* or *M. sphenops* are mated with females of a hybrid between the two, which is a form currently known as *M. formosa,* all the offspring are always *formosa* in type. In other words, the hybrid characters are inherited by the young from the mother in a block, without respect to dissimilar paternity. And the offspring are invariably female. In this connection, however, the authors state that "the consistent and abundant production of purely matroclinous and constantly female offspring by this hybrid form of fish finds its most plausible explanation as parthenogenesis. It is apparently not a spontaneous parthenogenesis, since many controls, unmated, have shown no indication of becoming pregnant. We provisionally assume that we are probably dealing with a case of gynogenesis . . . a condition recorded as naturally occurring among certain invertebrates." Comment upon the above may be divided into three headings as follows:

(a) It is unwise to intrude the suggestion of parthenogenesis, even of a modified sort, into vertebrate literature. The phenomenon is so at variance with what is known and believed about vertebrate development that I am sure no vertebrate morphologist would admit for a moment that the natural development from egg to sexual maturity of an individual vertebrate without direct inclusion of the male element is within the realms of probability. Certainly extremely convincing cytological evidence would be necessary and the experiments verified several times.

(b) The fact that all the hybrids of *Mollienisia* produced were female appears to be ascribable in this instance, perhaps, to a sex-linked lethal factor acting on male eggs, or possibly to some type of nondisjunction. In this connection it may be mentioned that a yellow (mutant ?) strain of *Xiphophorus,* developed in domestication and known as the golden swordtail, consistently produces two or three times as many females as males, and the case of the *Mollienisia* may be an instance of this factor carried to an extreme.

(c) The matroclinous character of the offspring can involve no argument in favor of parthenogenesis, for I can cite reverse evidence, of a patroclinous nature, as follows: I have a strain of hybrids produced by crossing a male black platy (*Platypoecilus*) with a female of the golden swordtail strain of *Xiphophorus,* which are large, spectacular fish, mostly black but with many spots and blotches of green and orange. Apparently all individuals of this hybrid or at least a large proportion of them, are fertile, and when bred to one another they behave as a true species. In other words all the young of the second generation resemble their parents rather than either of the grandparents. So far so good, with nothing very remarkable. But when a male of this hybrid strain is mated with a female of pure strain green swordtail (*Xiphophorus helleri*), none of the young will be of dull color like the mother but all blackish like the hybrid father. This patroclinous inheritance has been tested in a number of individuals and litters. I have no doubt that it will hold throughout successive generations, but for proof or disproof of this assumption sufficient time has not yet elapsed.

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NOTE ON THE LIFE-CYCLE OF ECTOCARPUS SILICULOSUS DILLW.

THE generally accepted view that the plurilocular sporangia of *Ectocarpus siliculosus* produce gametes is based primarily on the classical work of Berthold[1] at Naples. His results have been confirmed by Oltmanns, Hartman, and, more recently, by Knight.[2] Knight demonstrated also that all the plants of this species that she found at Naples had a haploid soma.

[1] G. Berthold, "Die geschlechtliche Fortpflanzung der eigentlichen Phaeosporen," *Mitt. a. d. Zool. Stat. z. Neapel,* II. 1881.

[2] M. Knight, "Studies in the Ectocarpaceae. II. The Life-History and Cytology of Ectocarpus siliculosus, Dillw." *Trans. of the Royal Society of Edinburgh,* Vol. LVI. 1929.