are very poor and have no diagnostic value as they are very incomplete. In the introduction, which has been overlooked by the majority of workers interested in this matter, appears the statement that this is a preliminary work, written for circulation among entomologists and would be followed by a more complete work. This indicated that Meigen did not wish these names to be used. In 1803 Meigen published a complete work on Diptera, but used none of the names he proposed in his 1800 paper. The 1800 paper was forgotten, but there is evidence that other workers of the time knew of it.

In 1908 F. Hendel[2] reprinted, in part, Meigen's paper of 1800. Hendel said he was able to recognize the 1800-genera by comparing the 1800 diagnoses (in French) with the diagnoses of the 1803 paper (in German) until he succeeded in pairing them off. He explicitly states that before he tried this method he was unable to recognize them from their diagnoses alone.

In 1909, J. M. Aldrich took steps to have the International Commission on Zoological Nomenclature act on the validity of the Meigen 1800 names. He sent them a paper, first asking if the 1800 names were valid and then listing reasons why they should not be, with the hope that the commission would declare that the names could not be or were not to be used. In 1910, the commission gave Opinion 28, which did not answer Aldrich's question, but stated that the 1800 paper had been published and, therefore, the names were available if found valid under the International Code. The majority of dipterists did not use the 1800 names, because they believed them invalid since they could not be recognized from the original description. Thus the matter rested until 1932, when F. W. Edwards of the British Museum published a "Questionnaire" in the *Entomologist* (65 (1932), pp. 13–14) and the *Entomologist Monthly Magazine* (vol. 68) (1932) pp. 1–3). The questions were as follows:

1. Do you consider that the names in the Nouvelle Classification should be accepted?
2. Do you consider that the omission of specific names renders the Nouvelle Classification names invalid?
3. Do you consider that, whether or not the Nouvelle Classification names are valid under the International Code, they should be annulled?

The results of the questionnaire were as follows (*Ent. Mo. Mag.*, vol. 68 (1932), pp. 255–258):

1. Affirmative, 13 per cent; no. of votes, 11.
2. Affirmative, 58 per cent; no. of votes, 58.
3. Affirmative, 74 per cent; no. of votes, 63.

The results show that the great majority of dipterists were definitely not in favor of the Meigen 1800 names.

[2] Verhandl. zool.-bot. Wien. Vol. 58 (1908), pp. 43–69.

In 1944, the commission issued Opinion 152, which stated, "The generic names in the Order Diptera (Class Insecta) first published in 1800 by J. W. Meigen in his 'Nouvelle Classification des Mouches à Deux Ailes' are to be treated as having priority as from that date." This opinion was issued in spite of the fact that the majority of dipterists were against using the Meigen 1800 names. Does this mean that the International Commission knows more about Diptera nomenclature than men who have spent their lifetime studying the subject? Secondly Opinion 152 shows that either the commission did not see a copy of the original 1800 paper or ignored Opinion 46, because it is impossible to recognize species from the generic descriptions as given in the 1800 paper.

The purpose of the International Commission on Zoological Nomenclature is to bring order to zoological nomenclature, but because of the ambiguous rules and opinions they have made, much of our nomenclature is no better than before the commission was formed. This matter of the Meigen 1800 names is an excellent example. So far they have given two opinions and neither has answered the question which was submitted for their consideration. They have simply said that it is up to the dipterist to interpret the meaning of their opinions. If it is up to the specialist to interpret the opinions why does the commission continue to publish ambiguous opinions? Are they afraid to offend some scientific worker by disagreeing with him?

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SCIENTIFIC PAPERS FOR EUROPE

SCIENTIFIC institutions in Europe are, as we all know, greatly in need of the technical literature which has appeared during the war, as well as older works to replace those lost owing to the war. Individual scientific workers can do a good service by sending their papers and others which they are able to obtain. I thought to make a beginning by sending a package of papers to the Congo Museum at Tervueren, Belgium, but it was returned to me as not complying with the necessary requirements. I was not told what these were, but in "News from Belgium," May 19, published by the Belgian Information Center in New York, I read:

> The printed-matter service is restricted to:
> (a) Periodicals and newspapers mailed directly by a publisher in this country to a publisher, an agent or a subscriber in Belgium.
> (b) Other articles conforming to the conditions applicable to printed matter, mailed directly by a publisher or commercial firm.

Forwarding or remailing any article of printed matter for Belgium is prohibited. Publications containing technical data must comply with the licensing requirements of the Foreign Economic Administration.

These rules seem to be obstructionist and devoid of sense under present circumstances.

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SCIENTIFIC BOOKS

MANKIND

Mankind So Far. American Museum of Natural History Science Series. By WILLIAM HOWELLS. 319 pp. Illustrated. New York: Doubleday, Doran and Company, Inc. 1944. $4.50.

THE book comprises three parts of about 100 pages each: Animals and the Coming of Man, Man and the Coming of Homo Sapiens and, finally, Homo Sapiens and the Coming of Races. In the words of the author, "Of all animals we men are the only ones who wonder where we came from and where we will go." Science has given much serious attention to the successive appearances of man-like creatures, patiently collecting the evidence upon which a reconstruction of his lost history could be based to the end that much new data have been accumulated during the half century now closing. Hence there is need for a clear logical attempt at such reconstruction. Until the appearance of this volume, there was no high-class treatise which the anthropologist could recommend to the scholar as well as to the intelligent lay reader seeking a reliable summary of the subject.

As may be expected, the first part of the book deals with the predecessor of man and could be consistently labeled "from fish to man," a favorite headline in current museum exhibits. The common saying that "scientists agree on nothing" is not strictly true, since paleontologists and anthropologists usually agree that no creatures properly designated as *Hominidae* are known before the Pleistocene Epoch, the régime of the Ice Ages, when, in the words of the author, "the weather was certainly something to talk about." In this section of the book, the author follows closely the broad generalizations of paleontology, but not blindly, since now and then he questions traditional interpretations. In his remarks about fire and cooking, he reveals an honest doubt that fire was so necessary for warmth as to force its invention and use since caves are usually warm enough, but that cooking and a liking for novelty may claim priority as a motive. In more general terms, the author suspects that "fire, flintworking, speech and society" served to increase man's numbers rather than to change his anatomy. However, he looks favorably upon the idea that the increase in numbers due to revolutionary inventions, such as fire, the bow, domestication, etc., might increase individual and group variation, thus leading to the wide range of race diversity in modern mankind. He rejects as unscientific the idea that man somehow sensed a career upon the ground as more desirable than one in the trees. Nevertheless, he imagines there was a great hazard in such a shift because the evolution of a foot was necessary, as the lack of it dooms the chimpanzee and the gorilla to extinction.

The over-all puzzle in the story of the several known species of the genus *Homo* is to reconcile the disappearance of all these species save one, *sapiens,* with the wide variations conceived as entities which we speak of as races. Yet the author reminds us that we are prone to grossly exaggerate these differences, to the end that we really think of "our races" as species, many of us unconsciously running amuck by giving them genus rank. He shows us that anthropologists favor one or the other of two opposing theses: (a) that none of the species of *Homo* save one, *sapiens,* was able to survive the selective rigor of the Pleistocene, but that even his grasp upon the continuity of morphological form was so shattered that he survived in a number of variations of near-species grade; (b) that few, if any, of these early species really disappeared, but that after an interval began a rapid transition by convergent evolution (by no means ended) accelerated by what we call "race mixture," so far leaving us a few sharply marked varieties of man no longer worthy of being rated as separate species. Of contemporary anthropologists, Weidenreich leads in support of *b,* whereas the author of this book bids for leadership in the *a* group. He contends that among the many objections to accepting theory *b* are that it violates the principles of genetics and is out-of-step with comparative morphology, but holds that neither view should be taken too seriously, because the number of available fossils of man is contemptibly small in contrast to the millions of examples of *Homo sapiens.* If new finds of fossil man are made, a less puzzling answer may be forthcoming.

We come now to the third section of the book. Since it is the custom to speak of the varieties of *Homo sapiens* as races, the author accepts that term with the title "Homo Sapiens and the Coming of Races." Beginning with the three great primary strains, White, Yellow and Black, there follow another