

of L-arabinose and methylamine, in absolute ethanol with anhydrous liquid hydrogen cyanide led to the ready crystallization of N-methyl-L-glucosaminic acid nitrile—m. p. 113°, $[\alpha]^{25}_D -17.5^\circ \rightarrow -21^\circ$ (50 min.) $\rightarrow -8.3^\circ$ (final, water); pentacetate—m. p. 132–134°, $[\alpha]^{25}_D -38^\circ$ (chloroform). On hydrolysis of the nitrile with acid followed by base there was obtained, on acidification, N-methyl-L-glucosaminic acid—m. p. 236° (dec.), $[\alpha]^{25}_D -4.6^\circ$ (water). From the mother liquors of the nitrile there was isolated an amorphous product which on hydrolysis led to a crystalline acid now under further investigation.

Full details will be communicated at a later date.

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The Rh System in the Chimpanzee

A. S. Wiener and M. Wade (*Science*, 1945, 102, 177) have shown that the erythrocytes of chimpanzees do not absorb the Rh agglutinins from the human antisera, anti-Rh₀, anti-Rh', and anti-Rh'', but do absorb the agglutinin from the anti-Hr serum. As this parallels the behavior of the human Rh- cells, it is concluded that chimpanzees are Rh-.

A chimpanzee died recently in the Gardens of the Zoological Society of London, and through the kindness of Prof. E. Hindle and Col. A. E. Hamerton we were provided with a sample of the blood. Absorption tests with the chimpanzee blood were clear cut and confirmed the findings of Wiener and Wade; that is to say, in the language of Fisher's theory (cited by R. R. Race. *Nature, Lond.*, 1944, 153, 771; R. A. Fisher and R. R. Race. *Nature, Lond.*, 1946, 157, 48), anti-D, anti-C, and anti-E agglutinins were not absorbed, but anti-e was. We found, however, that the chimpanzee cells failed to absorb the anti-e agglutinin, recently discovered by one of us (A. E. Mourant. *Nature, Lond.*, 1945, 155, 542).

The antigen e is present in double dose on human Rh- red cells, which strongly absorb anti-e. In terms of Fisher's theory the failure to absorb either anti-E or anti-e means that the chimpanzee possesses neither of the antigens determined by the E-e locus in man. Either the chimpanzee possesses a third antigen determined by the same locus or, more probably, the locus is absent altogether.

The apparent separability of this group of Rh antigens would seem to support Fisher's belief that they are, in fact, controlled by a separate locus.

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Misuse of the Linnaean System of Nomenclature

A. Byron Leonard (*Science*, 1946, 104, 17) has recently criticized under this same heading certain nomenclatural usages of E. E. Dickerman and in doing so has himself so misused the Linnaean system of zoological nomenclature (as codified in the Règles Internationales de la Nomenclature Zoologique) that a protest is called for.

We have not examined Dr. Dickerman's paper, because the reference given by Dr. Leonard is so incomplete that previous knowledge is required to identify it. Since we are neither parasitologists nor helminthologists, we will not presume to discuss Dr. Dickerman's usage that is criticized by Dr. Leonard, but will merely comment on Dr. Leonard's conclusions from his stated premises.

In general terms, of course, Dr. Leonard is correct in his position that in Linnaean nomenclature all growth stages of a single species must be called by the same name for taxonomic purposes. Furthermore, the choice of names is governed by priority of publication and not at all by the ontogenetic stage named (Règles Internationales, Art. 27 and 28, not Art. 26 as stated by Leonard). But the reasoning by which Dr. Leonard arrives at a conclusion that is correct in general terms (whether these terms are applicable to Dr. Dickerman's usage or not) is erroneous. In the first place, Dr. Leonard seems to assume that the names *Cercaria* and *Proterometra* were both proposed by Dickerman as new generic names, for he argues that priority between them could be determined by page precedence in Dickerman's work. This is far from being true, for *Cercaria* was first published by Mueller in 1773 and *Proterometra* by Horsfall in 1933 (A. S. Neave. *Nomenclator Zoologicus*, 1940). Furthermore, Dr. Leonard would have realized that the question of priority must have been settled long ago unless he had thought that the names were newly proposed. Priority of publication, not page precedence, would require that *Cercaria* be employed, unless the Règles were suspended by a special act of the International Commission or unless there was some other special circumstance. For example, there is some outside evidence that helminthologists in general, and probably Dr. Dickerman, do not regard *Cercaria* as the name of a genus but of a collective group treated for convenience as if it were a genus (see Art. 8 of the Règles). If this were the case, there might be no nomenclatural conflict, since only one of these names would be available as a generic name under the Règles.

But let us pass this over and assume that both names did originate in Dickerman's paper of 1945 as Dr. Leonard seems to believe, or at least that different growth stages of the same species were called for the first time variously *Cercaria sagittaria* Dickerman (p. 37) and *Proterometra sagittaria* Dickerman (p. 39). Dr. Leonard argues that by virtue of page precedence *Cercaria sagittaria* Dickerman is the "correct name" for the species described. This is fallacious, and it is this argument by Dr. Leonard to which we wish to take vigorous exception. It is particularly important to protest this because of the widespread misapprehension on the subject that probably stems from the fact that page precedence was an important criterion in several other codes of nomenclature long ago superseded by the Règles Internationales.

In the Règles, the code under which zoologists have operated for about 50 years and which Dr. Leonard undoubtedly thinks is the authority for his position, page precedence has no role at all for determining priority