THE NAME OF THE SPOTTED FEVER TICK

OPINION 78 of the International Commission on Zoological Nomenclature, just issued by the Smithsonian Institution, is so extraordinary that it should not be allowed to pass without comment. The summary states that "the commission is of the opinion that *Dermacentor venustus* dates from Marx in Neumann, 1897, type specimen Collection Marx No. 122 (U. S. National Museum), from *Ovis aries*, Texas." This is not quite the same as the statement on page 13: "*Dermacentor venustus* Marx in Neumann, 1897, belongs to a form," etc. The latter statement may be regarded merely as an indication of the facts; the former, professing to be a summary, commits the commission to the proposition that the publication by Neumann is valid from the standpoint of nomenclature.

What did Neumann actually do? He reported, wholly without diagnosis, certain ticks from Texas and New Mexico which he found labelled *D. venustus* by Marx. In his opinion these specimens belonged to the old species *D. reticulatus*. It is impossible (as Horváth states in his dissenting opinion) to regard this as a valid publication. The name *venustus* was not applied as a substitute for *reticulatus*, or to any part of the genuine *reticulatus*, but to undescribed specimens associated by the author with that species. The position is better understood if we suppose the matter to still stand where it stood then. There would be no possibility of knowing anything definite about the so-called *D. venustus*, except the localities. A name which can not be understood or interpreted on the basis of what has been published is not validly published in the sense of Article 25 of the Code. On the other hand, it is questionable whether private type designation ought to overrule the action of a subsequent reviser of a composite "species." This is a matter of great importance, which ought to be dealt with in an authoritative manner. The following position is at least worthy of consideration:

(1) The standing of a species depends on the statements published by the author. If we admit the *obligation* to be governed by his unpublished actions, we tie the hands of subsequent workers, who can not know what he may or may not have done in manuscript.

(2) If the supposed species is composite, the first reviser who detects the fact is compelled to select part of the aggregate to carry the name, and this should stand if there is no distinct indication to the contrary in the original account.

(3) Whether the supposed species is composite or not, if several localities are cited, but no type locality, the first reviser should have the right to select a type locality from among those originally given.

Hence, unless the *description* of Banks distinctly contradicts it (and no one appears to assert that it does), although the valid name *D. venustus* dates from Banks, 1908, Stiles in 1910 was at liberty to restrict the name to that part of the composite represented by the material on sheep from Texas, this having been definitely cited by Banks.

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ACQUIRED CHARACTERS

ON January 2, 1923, Wm. J. Herdman, of Toronto, addressed the following letter to Johns Hopkins University:

> During the war, England, Canada and the United States gave their soldiers more than a year of intensive physical training before sending them to the front. The medical men were much interested in the effects produced by this training, and it is reported that the American Medical Association recommended universal military training for all men for the mental and physical benefits obtained thereby.
>
> Has there been any scientific research on man or the lower animals to determine whether such benefits are inherited by offspring?

This letter of inquiry also contained some further explanations to distinguish between environment and animal activities. The reply was written by Professor H. S. Jennings, and in it he referred to Kammerer's work on "the effects of changed environments" and said: "He reports inheritance of these environmental effects, throughout." Professor Jennings also referred to my books and articles, the experiments of Guyer, and a brief article in SCIENCE for December 15, 1922, by Griffith. Detlefsen was not mentioned.

To this Herdman replied:

> Since writing my letter of January 2d I have secured a copy of Redfield's "Human Heredity" (1921). As far as I have been able to discover, this, and his previous books which you mention, represent the only work bearing upon the question which I asked in my previous letter. I have not examined the German reference which you mention, but if those experiments relate to actions of the environment, as you say, then they are not what I was looking for. I was looking for results produced by actions of the animal, and not those produced by actions of the environment.

After giving some explanations of why the work of Guyer and Griffith did not answer the question asked, Herdman said:

> Redfield seems to be the only person who has furnished any information bearing on the effects of physical training continued for enough years to amount to anything, yet, from what you say, he seems to be under a cloud. . . . If there is only one source of information on a particular