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 If we admit the *obligation* to be governed by his unpublished actions, we tie the hands of subsequent workers, who can not know what he may or may not have done in manuscript.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

subject, and that source is unreliable, then we have a queer situation. . . .

Professor Jennings waited two months and then wrote: "I felt that I had little or nothing to add that would help you, so that I have not hurried about replying."

The words "nothing to add" are a categorical acknowledgment that on March 14, 1923, Professor Jennings did not know of a thing on the face of the earth, other than my work, which was a proper answer to the question asked. When I gave out my quotations, I said that they were "simply those parts of the letters received which answer the questions asked—irrelevant matter being omitted."

The same letter of inquiry was also sent to the National Research Council, the Bussey Institution and to several other places. None of the other replies referred to Kammerer's work, or indicated any other person who had investigated the inheritance of "mental and physical benefits" obtained by training. As Professor Conklin put it: "I am sorry to have to tell you that there has been no satisfactory research of this character." No matter how unsatisfactory my work may be to any one, he can not deny that it is directed to the particular thing asked about in the question. Under these conditions I do not see any reasonable ground for the complaint of Professor Jennings in SCIENCE for January 11, 1924. I gave everything which was relevant, and the form of giving it did not change the meaning in the slightest degree.

CASPER L. REDFIELD

CHICAGO, ILL.

SINCE sending in my note in SCIENCE of January 11, the material there discussed has been published by Redfield. With relation to the above communication, two matters of fact require to be made clear:

(1) My letter to Herdman cited the paper (by Detlefsen) given at the scientific meetings in Toronto, but did not mention the author's name.

(2) Any one competent to discuss the inheritance of acquired characters knows that the experiments of Kammerer do deal with those effects of the environment that take the form of responses by the organism, including activities as well as structures. The same is true of the book by Semon, which was likewise cited.

The method employed in asserting that the words "nothing to add" are "a categorical acknowledgment" of what Redfield affirms, is a precious sample of the illuminating methods referred to in my former communication. One is at liberty to hold any opinions that suit his fancy, but to publish them over the signature of another who considers them preposterous is not scientific. The matter is without interest save as a study in the methods, reliability and competence of a man whose pronouncements on a difficult biological problem have in certain otherwise well-informed quarters been taken seriously.

H. S. JENNINGS

THE PROBLEM OF THE MONKEY AND THE WEIGHT

IN this *Journal* on February 15 last, page 164, Carl Hering states the problem in these words: "A supposedly weightless rope passing over a frictionless pulley has a 10 pound weight hanging on one end and a 10 pound monkey on the other. What will happen when the monkey climbs the rope?

As the proposer himself does not give a solution of his problem, it may be of interest to mention the practical test to which the writer put it a little over six years ago. A clockwork monkey driven by a spring and weighing 240 grams was counterpoised over the nine-inch pulley of a fine Atwood's machine. When the thread holding the last wheel in the train was burned, it climbed 80 cm in a minute, while the counterpoise remained stationary. An account of this was published in *School Science and Mathematics* in December, 1917, xvii, 821.

This statement remained unchallenged for two years, when in the same journal in December, 1919, xix, 815, Wilbert A. Stevens asserted that friction was to blame for the fact that the counterpoise did not rise with the monkey. This appeared rather doubtful because one fifth of a gram was sufficient to destroy the equilibrium, and when the pulley was replaced by a balance, 10 milligrams deflected the beam when the monkey and its counterpoise were attached. When the monkey started to climb, the counterpoise did go up for a moment, but it came down again and oscillated about its zero position with decreasing amplitudes.

The experiment was then repeated with the clockwork monkey climbing ten times as fast as before, and then both monkey and counterpoise went up together. This result was published in the same journal in February, 1920, xx, 172.

WILLIAM F. RIGGE

IN the discussion of this problem on page 164 of this volume of SCIENCE, a necessary condition has been omitted from the statement of the problem; that is, the relative distances of the monkey and the weight from the pulley.

Whatever this ratio may be, the effect of the action of the monkey in passing the rope through his hands must be precisely the same as that which would be produced by a shrinkage of the rope. That is, the tension will be the same in all parts of the rope. If the weight is five feet and the monkey ten feet from