know as yet all the factors involved in the problem, but considering the relative scantiness of the food supply on the island at the present it is safe to say that the experience thus far gained speaks in favor of continuing the policy of sparing the female fox.

LEONHARD STEJNEGER.

U. S. NATIONAL MUSEUM,
August 11, 1902.

TYPES VERSUS RESIDUES.

TO THE EDITOR OF SCIENCE: My recent note under the heading 'Zoological Nomenclature in Botany' was not intended as a contribution to a running controversy, but was merely a plea of 'not guilty' to the horrible charge of having continued in botany the discussion of a tiresome question solved long ago in zoology. Historical differences in the development of the two biological sciences were taken to be at least a partial explanation of the fact that zoologists had managed, though not without considerable effort of casuistry, to keep their barge afloat in spite of shoals which would bring the more heavily laden botanical craft firmly aground. That the framers of the zoological chart to which botanists had been referred had not sounded all the difficulties of the problem of nomenclatorial stability is rendered even more obvious by Dr. Dall's two letters.*

It is not to be expected that the merits of any suggestion in so old and intricate a subject as nomenclature can be made plain by desultory argument, but the possibility that somebody may wish to examine the matter further may justify the notice of such of the new specifications of the second letter as seem calculated to obscure the question of permanent generic types. I am quite unable to understand why Dr. Dall should represent me as objecting to 1758 as the initial date for zoological nomenclature, or as favoring vernacular names.

Under the method of types systematists who agree to the validity of a generic group will not differ as to the name to be applied to it, while under the method of elimination such differences are frequent and necessary. This absurd provision for perpetual confusion has appeared unavoidable to DeCandolle and to many eminent systematists of later date because they persist in the pre-evolutionary fallacy of regarding genera as definitions or concepts instead of taking advantage of the evolutionary right to treat them as groups of species, to one of which the generic name may be as directly and fixedly attached as the specific name itself. And since by means of an evolutionary axiom we can escape the Doubting Castle of mediæval casuistry and much unproductive labor of antiquarian research, Dr. Dall's objection to so simple and practical an expedient can scarcely be understood except as an unwillingness to come out—a nomenclatorial Prisoner of Chillon, as it were.

To attach generic names to type species certainly renders nomenclature far more effectively separate from classification than when they are made to pertain only to residues which vary with every individual opinion. Taxonomy as a whole is, however, but a means for scientific ends, and is not studied merely to preserve the Linnæan or the DeCandollean traditions. The taxonomic problems of to-day are very different from anything contemplated by Linnæus, and if the system of nomenclature popularized by him could not be modified to serve practical purposes it would undoubtedly be discarded, as occasionally threatened already by physiologists and ecologists impatient at once of the complexity of organic nature and the fickleness of systematists.

To have types for 'modern genera' will yield no 'definite stability' while the ancient names are free to roam over the face of nature, though to tether each of them securely in a particular place must disappoint all except one of the claimants for possession. Nevertheless it would seem that those who have made hundreds of changes of names in accordance with rules which do not produce stability are scarcely in a position to object to measures better calculated to secure permanence.

The only 'upsetting' advocated in this connection is that of a rule which causes, perpetuates and legalizes confusion and instabil-

* SCIENCE, N. S., XV., 749, May 9, 1902; XVI., 150, July 25, 1902.

ity in the application of generic names. The changes incident to the execution of such a reform are few and harmless in comparison with those perpetually necessary where names depend on residues. An excellent example of the workings of the method of elimination is appropriately revealed in the mazes of the recent *Mephitis-Chincha-Spilogale* discussion. Several prominent zoologists propose to settle this by a new rule* which eliminates elimination and yields definite types for a very small percentage of genera, but which constitutes a significant admission of the essential instability of residues. The new rule leads in the right direction, though it is but a short step on a long journey.

Obviously, the method of elimination lacks the definite and 'necessarily arbitrary' features without which, as Dr. Dall well says, 'stability is hopeless.' It may be true that the zoological laws 'are intended to * * * bring about stability,' but it is plain that the intention has not been rendered effective by adequate formulation. Systematists who appreciate stability may differ on details of the legislation needed to inaugurate the method of types, but they should not use stability as an argument for residues.

O. F. Cook.

Washington, Aug. 7, 1902.

SHORTER ARTICLES.

Nature of the specific bacterial toxins.†

After it had been demonstrated beyond any controversy that certain bacteria cause certain diseases, quite naturally the question was asked, How do bacteria cause disease? Several answers to this question have been offered. If the liver, spleen or kidney of a guinea-pig which has died from experimental anthrax be sectioned and examined under the microscope, the blood vessels of these organs will be found to be filled with bacilli. In many places the germs have grown so abundantly that they distend the smaller vessels. It was suggested that the anthrax bacillus causes disease and death by mechanically interrupting the functions of certain organs. This is known as the mechanical interference theory. It has no support in any other disease than anthrax, and consequently it cannot be accepted as a satisfactory answer to the question, How do germs cause disease? Another theory offered supposes that the bacteria cause disease by consuming the proteids of the body and thus depriving it of its sustenance. It is known that the proteids are necessary for the building up of cells, and it is also known that microorganisms feed upon proteids. This theory is untenable in the first place because many of the infectious diseases destroy life so quickly that the fatal effects cannot be presumed to be due to the consumption of any very large amount of proteid substance. Secondly, the distribution of the germs in the body is such in many diseases that they do not come in contact with any large percentage of the proteids of the body, and thirdly, the symptoms of the majority of the infectious diseases are not those which would be produced by withdrawing from the various organs their food supply. A third theory supposed that the germs cause the symptoms of the disease and death by depriving the red blood corpuscles of their oxygen. This theory was suggested by the resemblance between the symptoms of anthrax and those of carbon dioxide poisoning. More extended observation soon demonstrated the fallacy of this theory, especially inasmuch as it was shown that the amount of physiological oxidation going on in the bodies of animals sick with anthrax was not diminished by the disease. In this way the theory that germs destroy life by depriving the blood of its oxygen has been shown to be not applicable to anthrax, and in fact not to any known infectious disease. Next it was suggested that bacteria cause disease by forming chemical poisons. This is the theory which has found general acceptance and which is now generally believed to be the true explanation, although none of the specific toxins has been isolated in a state of chemical purity. The elaboration of chemical poisons by bacteria may occur in either of two ways: In the first place the bacterium, either

* Science, N. S., XVI., 114, July 18, 1902.

† Abstract of a paper presented by Dr. V. C. Vaughan before the Research Club of the University of Michigan.