It rarely happens that a scientific organization of national character is instituted with so general support and so complete harmony as was displayed at the founding of the American Anthropological Association.

W J M.

DISCUSSION AND CORRESPONDENCE.

BLUE FOXES ON THE PRIBYLOF ISLANDS.

The account of the 'Blue Fox Trapping on the Pribylof Island,' by Lembkey and Lucas in Science, Vol. XVI., pp. 216–218, is highly interesting in many respects, but while the authors seem to regard the experiment of sparing the females as of doubtful success, I am of the opinion that the result has amply justified it.

It is quite true that the table of foxes trapped on St. George Island, 1897–1901 (p. 216),* apparently indicates a surprisingly small increase in the females caught, but several causes have probably conspired towards this result. In the first place, the experience on the Commander Islands seems to indicate that the females are more cautious than the males and are not so easily caught. Thus in 1896 there were taken in steel traps on Copper Island 515 males and 452 females or 63 males more than females. If this represents the normal ratio between the sexes caught then it will be seen that on St. George Island in 1900–1901 there should have been taken only 539 females to 614 males under normal circumstances. As 690 females were really taken it would seem that the normal excess of females was 151 instead of 76.

It will be observed that during the previous three years a large number of females have been trapped on St. George Island, which were released after having been 'marked' or 'branded.' Is it quite probable that all these females have allowed themselves to be caught over and over again? The blue fox is a stupid creature compared with his red brother, and I know that the same animal has repeatedly been trapped. But from this to conclude that all the females are thus caught and that none of them have learned by experience to keep out of the traps seems little probable, especially if it is true that the females are more cautious than the males. It appears to me even highly probable that a large number of the females avoided being taken again, and that we have here a valid explanation of the comparatively low number of females in the table on p. 216.

The writers of the article in question think it probable 'that there has been some slight gain in the number of foxes.' Apart from the above considerations I think it can be shown that the gain has been great and almost unexampled.

Statistics covering a long period of years (1847–1891)* show that on the Commander Island,† as a rule the successful fox hunt of one season is followed by a tremendous drop in the yield during the next year. Thus on Bering Island the number of foxes killed in 1852 was 1,900; in 1853 the number dropped to 547, or more than two thirds. In 1859 the harvest was 1,233 foxes, while in 1860 only 584 were caught. In 1871 870 blue foxes were killed, in 1872 only 580. In 1875, 1,087, in 1876 only 573. In 1881–2 the number was 1,477, in 1882–83 only 872. A series of figures such as we have them from St. George for three consecutive years, viz., 867, 955, 1,304, is therefore highly encouraging.

It is therefore greatly to be hoped that the authorities on the Pribylof Islands may not lose heart even if the actual returns may not come up to the figures of the table which is intended to show what the increase ought to be theoretically. It is evident that we do not

* 1902 in the table quoted is probably a misprint for 1901.

* See my 'Asiatic Fur Seal Islands,' 1898, p. 43.

† A corresponding table relating to the Pribylof foxes during part of the same period ('Fur Seals and Fur Seal Islands N. Pacif.,' III., 1899, p. 340) taken from I. Petroff's census report does not show similar conditions on the Pribylof Islands. Without knowing the source of these statistics this difference is not easy to explain, but I would suggest that the list in question may only be a record of the number of skins *shipped* during the respective years but not showing the number of foxes actually killed in the year to which they are credited. The company probably required a certain number of skins shipped each year to satisfy the demand of the market, hence the remarkable uniformity.

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.

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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.