posal mentioned, namely, by inducing journals and publishing societies to refuse publication to papers containing new genera for which the authors fail to designate types. This plan, unbeknown to me at the time, had already been adopted by the Washington Biological Society before I began to advance it. I have now brought the proposition before several organizations, all of which have agreed to insist upon the designation of a type for every new generic name submitted to them for publication, and instructions have been issued to the general effect that papers not complying with the rule will not be accepted for publication. The organizations which have notified me of the adoption of this general plan are as follows:

U. S. Fish Commission.
U. S. Geological Survey.
U. S. Department of Agriculture.
U. S. National Museum.
U. S. Public Health and Marine Hospital Service.
Smithsonian Institution.
Biological Society of Washington.
Entomological Society of Washington.
American Museum of Natural History, New York.

It is my intention to communicate with other organizations in the hope of inducing them to adopt this same plan. Such a movement, however, when dependent upon the efforts of one person, is necessarily somewhat slow. On this account I take the liberty of addressing the systematic zoologists, through SCIENCE, and of asking them to join in the movement by bringing the matter before any publishing organizations to which they belong and by urging its adoption not only by societies, academies, surveys, etc., but also by zoological journals.

I shall be under obligations if zoologists will notify me of any societies, journals, etc., which have already adopted this rule, or which adopt it in the future.

CH. WARDELL STILES.

CERTAIN PLANT 'SPECIES' IN THEIR RELATION TO THE MUTATION THEORY.

AT the last congress of the American Ornithologists' Union I presented a short paper on the 'Applicability of the Mutation Theory to Birds.' My conclusions were entirely in accord with those of Dr. C. Hart Merriam as presented in his most interesting address before the American Association in New Orleans.[1]

There is one point, however, not touched upon by Dr. Merriam which I brought forward as probably influencing de Vries or at least others who share his views. This is that we seem to have among plants certain forms which are, so far as their differential characters are concerned, comparable to subspecies among terrestrial vertebrates, but which are not restricted to any definite geographic life area or climatic zone, as is always the case with the latter.

Any one at all in touch with modern botany is aware of the tremendous number of forms which are now being described as species. In order to learn something of the nature of these forms and their possible correlation with subspecies of birds and mammals, I selected the acaulescent violets and spent several years studying their variations in the neighborhood of Philadelphia.[2]

I found it possible to recognize a number of quite distinct forms, and yet every year I discover others of intermediate character; while every new section of country yields allied forms which do not fit exactly into any of my previously prepared diagnoses; yet each of these forms is reasonably constant in its own patch or neighborhood.[3] These are certainly not *species*, neither are they subspecies as we understand them in vertebrates. Moreover, it is hopeless to begin to 'lump' them, for we soon find ourselves forced to combine species of long standing and ultimately to have only one species of acaulescent blue violet[4] and one white one!

Just what these 'forms' are and what their

[1] SCIENCE, XXIII., p. 241.

[2] Cf. Stone, *Proc. Acad. Nat. Sci.*, 1903, p. 656.

[3] Cf. Burgess, 'Biotian Asters,' *Mem. Torrey Bot. Club*, XIII. Also Brainard, *Rhodora*, VI., 213; VIII., p. 6 and 49, where hybridism on a large scale is advanced as the explanation of these forms.

[4] Exclusive of *V. pedata* of course.

origin may be I am not prepared to say, but they have no counterpart among birds and with a few possible exceptions none among other vertebrates.

I may say that I do not regard them as the result of mutation as the followers of de Vries apparently do, but think it likely that they may be due to the action of immediate local environment, the exact nature of which it is practically impossible for us to detect. The extremely sedentary nature of plants, especially some groups, and the ease with which isolation may affect them would tend to emphasize the effect of local environment in producing differentiation.

In terrestrial vertebrates we find among snakes certain forms with peculiar coloration occurring as colonies here and there within the range of the species which do not conform to any definite geographic habitat, and in some fossorial or semifossorial mammals similar extremely local forms occur, as '*Geomys colonus*' Bangs surrounded by the range of *G. floridanus* and *tuza* and in just the same sort of environment so far as we can see; also '*Microtus rufidorsum*' Baird, which occurs in colonies within the range of *M. pennsylvanicus*.

These may be parallel cases to those exhibited in *Viola, Cratægus, Aster, Panicum,* etc., and their sedentary nature seems to point similarly to elements in the immediate local environment as the probable cause of their differentiation.

Witmer Stone.
Acad. Nat. Sciences, Philadelphia.

ISOLATION BY CHOICE.

To the Editor of Science: The recent discussion of isolation in Science reminds me of a popular article I wrote for *The Outlook,* emphasizing the psychic factor in evolution[1] —the part that choice plays. We must, it seems to me, not forget the various factors that work together at the same time in producing species. A fish with weak eyes would naturally prefer cave life, and thus isolated breed with others similarly equipped, physically and mentally. Those that have the physical variation without the mental would soon feel the effect of their want of sense.

[1] February 18, 1898.

The same principle applies in protective coloration. One may easily conceive of two habitats where the protective coloration would be quite different, and it is easy to see that the survival of those sensible enough to stick to the habitat best suited to them might quickly lead to intensification both of the tendency to seek the one habitat and of the coloration that adapts them to it. Indeed sexual selection may come in. Those having a willingness to accept mates with an erratic tendency to the other habitat or less protective coloration would have progeny less liable to prosper. Thus we may easily imagine two color races, species, arising, separated by a hereditary preference for different habitats, and for mates with all the peculiarities that those habitats have produced, while yet there is no physical barrier preventing the crossing, which may indeed go on to some slight extent.

Alfred C. Lane.

LARVAL CONGER EELS ON THE LONG ISLAND COAST.

The occurrence of larval conger eels in great abundance on the Atlantic coast has, as far as I am aware, not been recorded; accordingly the following note may be of interest.

On May 27, 1905, the 'Leptocephalus' of a conger eel appeared in great numbers at Easthampton, on the south shore of Long Island, about twenty miles from Montauk Point. They were washed up by the waves, literally, in thousands, and continued to come ashore in greater or less quantity—being especially abundant again on June 3—for about a fortnight. It was evident that this interesting harvest was due in some measure, at least, to a local storm and change of currents, which also brought in a number of bottom forms—*e. g., Natica* and its eggs.

The larvæ were all of a uniform length of about four inches, and in a few cases appeared to be in normal condition; most, however, were found to be either dead or dying. Dr. Bashford Dean, who has seen my specimens, tells me that they are probably *Leptocephalus*