stances are associated with cells containing reserve materials. In the case of the carrot the reserve materials are utilized by the plant of the second year, and in the case of the buttercup they are utilized in the development of the akene.

4. The feeding of plants with chemicals for the purpose of controlling color, as certain iron, aluminum, potassium and other salts as well as certain organic acids, has not so far, in the author's experiments with carnations, roses and violets, produced any marked changes in the colors of the flowers, only some slight effects being noted which might be attributed to other causes. Knowing that plants have a certain individuality and certain inherent qualities or tendencies, other than negative results could hardly be expected. On the other hand, the plant is a rather plastic organism, and for this reason experiments along the line indicated are more or less justified.

5. Experiments in supplying plants and cut flowers with vegetable coloring matters and aniline dyes showed that none of the vegetable color substances were taken up and that only a comparatively few of the aniline dyes would color flowers. The fact that of thousands of dyes or color substances, only a few are carried as high as the flower, would tend to show that only certain chemicals or substances would be taken up by the plant, and thus exert an influence on the coloring matter in the flower. If such profound changes occur in plants as are provided by the mutation theory, is it too much to suppose that certain definite changes might be produced by means of which we have knowledge or control?

Dr. Kraemer's remarks were illustrated by a hundred or more freshly cut flowers such as carnations, roses, hyacinths and callas, which had been artificially colored in the few hours preceding the demonstration by placing the stalks of the flowers in solutions of certain dyes. Numerous dried specimens of artificially colored flowers of various plants were also exhibited.

Dr. Rusby showed fresh fruits of the so-called 'tree-tomato,' a species of *Cyphomandra* native to South America.

Marshall A. Howe,
Secretary pro tem.

THE ST. LOUIS CHEMICAL SOCIETY.

The regular meeting of the society on March 12 was devoted to a consideration of matters connected with the organization of the society.

April 9. The paper of the evening, communicated by the secretary, was entitled 'Phosphorescent Zinc Sulphide.' The paper dealt with phosphorescent zinc sulphide prepared by Mr. John Esmaker. The method followed was detailed briefly, with a view to emphasizing the slight changes in procedure, which result in failure to obtain the phosphorescent variety of zinc sulphide. The changes in the method were so slight, that apparently they should have had no influence on the character of the result. The remainder of the paper, and the general discussion which followed, dealt with phosphorescence and similar phenomena, and endeavored to assign some reason for the observed effects.

C. J. Borgmeyer,
Corresponding Secretary.

DISCUSSION AND CORRESPONDENCE.

A PLAN TO ENSURE THE DESIGNATION OF GENERIC TYPES. AN OPEN LETTER TO SYSTEMATIC ZOOLOGISTS.

Probably no other single factor has caused so much confusion in systematic zoology and nomenclature as has the failure on the part of some authors to definitely designate the type species for the new genera they have described. Such failure, indeed, so frequently produces confusion, that the suggestion has been made that a rule be inserted in the International Code of Nomenclature to the effect that no new generic name proposed after a given date, say December 31, 1909, may claim recognition unless its author definitely designates its type at the time of the publication of the name in question. A rule of this nature, extreme though it may appear to some persons, seems to be fully warranted in view of the experience zoologists have had with genera proposed without types. It seems somewhat doubtful, however, whether the International Congress would see its way clear to adopt the proposition just referred to.

Another plan has occurred to me by which practically the same result may be obtained, without recourse to the adoption of the pro-

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.