been appointed dean of the school of pharmacy of Northwestern University, to succeed the late Oscar Oldberg.

DEAN DAVID KINLEY, of the graduate school, University of Illinois, has been elected vice-president of the university for one year beginning July 1, 1913, at the meeting of the trustees on July 2. He succeeds Dr. T. J. Burrill, who retired from active duties last year.

ALEXANDER GEORGE MCADIE, professor of meteorology in the Weather Bureau and director of the California climate section, has been elected director of the Blue Hill Observatory and professor of meteorology at Harvard University.

DR. F. J. ALWAY, head professor of agricultural chemistry in the University of Nebraska and chemist of the Nebraska Agricultural Experiment Station, has been appointed professor of soil chemistry and chief of the division of soils in the University of Minnesota. Dr. Fred Upson, of the University of Chicago, has been appointed to succeed Dr. Alway in the University of Nebraska.

DR. JAMES R. NYDEGGER, of the United States Public Health Service, has been elected professor of tropical medicine in the University of Maryland.

MR. W. G. FEARNSIDES, fellow and lecturer in natural sciences at Sidney Sussex College, and demonstrator in petrology in the University of Cambridge, has been appointed to the Sorby chair of geology at Sheffield University.

DISCUSSION AND CORRESPONDENCE

NOMENCLATURE IN PALEONTOLOGY

TO THE EDITOR OF SCIENCE: I ask the courtesy of your columns to explain certain allusions in a recent contribution which seem to have been somewhat misunderstood by my good friend Dr. Peale. In criticizing a prevalent custom in vertebrate paleontology of identifying as to genus and species very fragmentary material which is not really exactly identifiable, I spoke of its having "sadly misled" him into presenting as conclusive evidence of identity in age a correspondence in fauna (*i. e.*, in the fauna as listed) that was really no evidence at all. The criticism was in no wise directed at Dr. Peale, as he seems to suppose, nor at individual vertebrate paleontologists, but at a prevalent custom in this branch of science which I think ought to be amended. Naturally, Dr. Peale is perfectly justified in depending upon the published lists (*if* they have not since been criticized or amended or new and better evidence secured); and vertebrate paleontologists are presumably justified in following the customs of their tribe. But this is a vicious custom, and the fact that it misled so eminent a stratigrapher was cited as an instance of the harm it does.

Dr. Peale finds it "interesting to have a vertebrate paleontologist make the statement that 'correspondence in fauna is not conclusive evidence of identity in age.'" Well, I am not so rash as to say that it *is*, without making a number of reservations as to adequacy, presentation and interpretation of the evidence, etc. (for certain other considerations see article in *Bull. Geol. Soc. America* for 1913, p. 283). But I did not make the statement he attributes to me, if I understand the meaning of words, and considering the context in which I was using them in the cited article. I was discussing faunal lists based upon specimens too fragmentary for exact identification. Such a "correspondence in fauna" is *not* conclusive proof of identity in age. That does not mean that vertebrate paleontology has no place in stratigraphic geology. Fossil vertebrates, provided the material is adequate and the identifications correct, afford a much more exact geological timepiece than do invertebrates or plants. But the material is always scanty and often inadequate, and the degree to which this is true must in each case be taken into consideration in interpreting their evidence. Furthermore, owing partly to the greater exactness of our timepiece, we are conscious of certain normal deviations from accuracy—if one may so speak—regional, environmental, etc., which although their effects upon the existing flora as well as fauna are obvious enough, are not always considered by paleobotanists and stratigraphers.

It should be noted that my criticism was limited to the inference that the evidence from vertebrate paleontology as cited was conclusive in this problem. I have expressed no opinion as to the validity of Dr. Peale's conclusions in regard to the age of the Judith River fauna, chiefly because the subject is under investigation and the evidence is not all in yet. Mr. Barnum Brown has spent four or five months of nearly every year from 1899 to the present date, in collecting vertebrate and other fossils for the American Museum from the Lance, Hell Creek, Judith River, Ojo Alamo, Edmonton and Belly River beds, most of which are or have been included under the broad designation of the Laramie Group.[1] He has secured a large amount of fine material, made extensive observations on the stratigraphy, and kept accurate records of the location and level of his finds. Certain other parts of the problem are under investigation by Messrs. Granger and Sinclair in New Mexico and Wyoming. Until these data have been compared, studied and coordinated with those previously published, it seems better to retain an open mind in regard to the tenor of the evidence from fossil vertebrates on the Laramie question.

W. D. Matthew

American Museum of Natural History,
July 1, 1913

MENDELIAN FACTORS

To the Editor of Science: The alternative interpretation proposed by Dr. Henri Hus[2] for ratios found in F_2 crosses between sweet and waxy varieties of maize, suggests the question whether we are to use Mendelian factors merely as a form of notation to aid in the orderly arrangement of certain facts of heredity, or go further and insist that they have a real existence. The observed ratio of 9 horny seed, 3 waxy seed and 4 sweet seed was represented as resulting from the interaction of two factors, a factor S for sweet endosperm and a factor X for waxy endosperm. The presence of both S and X was assumed to result in horny endosperm. In the self-pollinated progeny of a sweet-waxy hybrid, both S and X would be present in 9 out of every 16 seeds and this was the number of horny seeds observed. X alone would occur in 3 out of 16, the ratio in which the waxy seeds occurred. S would also occur alone in 3 out of 16 seeds, but the number of sweet seed was found to be 4 instead of 3 out of 16. On this hypothesis, therefore, the one seed out of every 16 which would have neither X nor S was included with the sweet seeds.

Dr. Hus's proposed changes are in effect to substitute W for our X, H for our S, and to add a common factor called S to all the members involved.

To the writer the only object in premising factors at all is that by their use predictions are made possible, and in the present case two factors are adequate for this purpose. To assume a third factor is like adding an unknown constant to both sides of an equation.

The test proposed by Dr. Hus for the reality of the H factor is the same as one of the tests originally outlined as a test for the same factor which we called S. What is needed to prove the superiority of the formula proposed by Dr. Hus is some method of testing the reality of the common basic factor. Until some plant is discovered in which the basic character is absent there appears to be no way of doing this. The presence of a factor can neither be demonstrated nor disproven so long as it is assumed to be universally present.

When sweet and horny were the only alternative kinds of endosperm known the presence and absence of a single factor was adequate to make predictions regarding their behavior. With the discovery of waxy endosperm it was necessary to add a second symbol. But until another form comes to light it is difficult to understand how a third symbol helps us to an understanding of the inheritance of these characters.

If the symbols are taken to represent actual entities it is of course anomalous to have a

[1] Not in the Laramie *formation* as now limited by the U. S. Geological Survey.

[2] Science, June 20, 1913, p. 940.