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.

character represented by the absence of all factors. But in avoiding this anomaly, calculation is made more difficult and the only object gained is to lend an unwarranted appearance of reality to what is merely a convenient formula for expressing the observed relations.

G. N. COLLINS

WASHINGTON, D. C.,
June 30, 1913

SWEDENBORG

TO THE EDITOR OF SCIENCE: At the top of the second column of page 100 of SCIENCE for January 17, 1913, I note the following statement by one of your correspondents: "But Swedenborg would be laughed out of a modern court of science."

I find in a brief Life of Swedenborg, by J. Stuart Bogg (Frederick Warne & Co., London and New York, 1911), that Swedenborg was a wide traveler, a friend of learned men, a student of astronomy, metallurgy and anatomy, an inventor, a practical-minded, useful member of the Swedish House of Nobles, assessor in the Royal College of Mines and an author of numerous scientific works. Among his inventions were a plan for a submarine boat and a plan for a flying machine based on the now known principles of heavier-than-air machines. He declared that a very slight force would be sufficient to keep such machines up, but he knew nothing, of course, of gasoline motors. In the domain of astronomy he originated a method for finding terrestrial longitude by means of the moon. In the House of Nobles he took an active interest in such matters as the finances of the country, the liquor traffic and the mines. Among his scientific publications were works on chemistry, metallurgy, astronomical methods, observations connected with the physical sciences, and the economy of the animal kingdom. Until he was fifty-five years of age he was wholly occupied in these scientific and practical pursuits and was respected by scholars and patrons of learning at home and abroad.

In a prospectus which lies before me of a new edition of Emanuel Swedenborg's Scientific Works, I see that "Swedenborg's discoveries and theories in various departments of science have awakened an increasing interest among specialists during the past century," that they led the Royal Swedish Academy of Sciences to appoint a Swedenborg committee in 1902, and that this academy had in 1907 already published Vol. I. of the new edition in the original Latin and Swedish.

In view of these facts it seems strange to me that any one should affirm that "Swedenborg would be laughed out of a modern court of science." Is it possible that those who would laugh him out have never read his scientific works at all? If so, perhaps they could profitably reflect on the following quotation from Herbert Spencer:

> There is a principle which is a bar against all information, which is proof against all argument, and which can not fail to keep a man in everlasting ignorance; this principle is contempt prior to examination.

ANDREW H. WARD

A NEW VARIETY OF JUGLANS CALIFORNICA WATSON

THERE recently appeared in these columns a brief note by N. B. Pierce entitled "A New Walnut." It included a very brief general description which could not be accepted as a diagnosis in the usual meaning of that term. Yet Dr. Pierce stated that he thought it desirable to give the new form a name at that time and that he intended to publish a full description later. But Dr. Pierce did not see fit to cite the diagnostic description of this form which was published (but without reference to a scientific name) in Jepson's "Silva of California."[1] Had he done so the name he proposed would stand, even though unsatisfactory to one who has studied the form carefully.

However, I take it that *Juglans quercifolia* Pierce is a *nomen nudum* and that it still remains to publish a scientific name and diagnosis *together*. Therefore, I take pleasure in recording the same as follows:

New Variety: Juglans californica var. *quercina*. Diagnosis by the undersigned in

[1] Jepson, W. L., "Silva of California," Univ. Calif. Memoirs, Vol. II., 1910, p. 54.