two degrees of declination. The corrections are given for the epoch 1755, when they depend on Auwers-Bradley; 1875, when they depend on Pulkowa; 1880, depending on Greenwich; 1885, on Pulkowa; and finally, 1890, depending on Greenwich. The corresponding corrections for 1847, which is the mean epoch of Boss' system, are taken as zero. From the fact that these corrections to Boss do not vary uniformly with the time, Prof. Newcomb draws the conclusion that Bradley's observations must be inconsistent with the truth, which seems to imply that they are to be accorded no weight in forming a normal system. Yet we may well ask whether the numbers given by Prof. Newcomb are accurate enough to furnish any information of reliability. In his zone A the correction to Boss for 1755 is $-2''.23$. But the ten numbers of which this is the mean have a range of no less than $4''.00$. So we can hardly escape the conviction that the whole conclusion may be vitiated by a large error in a particular star. That this has occurred is not altogether impossible. For zone B the corresponding mean is $0''.27$, with a range of $2''.30$ in the ten numbers whose mean has been taken. We cannot regard conclusions based upon evidence so discordant as final. It is to be noted also that only one of the twenty stars used by Prof. Newcomb is to be found in Boss' mean system. The other nineteen stars are among those taken by Prof. Boss from the catalogues which were not used in forming the mean system, but which were reduced to the mean system by the aid of systematic corrections. Indeed in all researches with Boss' system we are met at every step by the insuperable difficulty that his original mean system does not contain stars enough to get rid of casual errors in individual stars. While therefore we agree with Prof. Newcomb's final conclusion that the system of Auwers cannot be regarded as definitive, and that it requires revision, we wish to point out that the same is true of the Boss system. And finally we wish to repeat our former statement that it is not at present practically possible to employ the Boss system, because the reductions to that system for the recent accurate catalogues have not been published. This has been done with care for the Auwers system, and until it has been done for the Boss system astronomers wishing to deduce for any purpose the most accurate declination of a star from all the catalogues will have to use the Auwers system.

H. J.

THE AGE OF THE PHILADELPHIA BRICK CLAY.

In Prof. Salisbury's last excellent report on the Surface Geology of New Jersey some of the most important points are likely to be overlooked by reason of the different names applied to the same formation by successive investigators. Fully to appreciate the light which Prof. Salisbury's investigations shed upon some of the points recently under discussion, it is necessary, after the manner of the mathematicians, to substitute in one equation its equivalent in another.

What was formerly referred to as the 'Philadelphia Brick Clay' was later correlated with the 'Columbia.' This, however, is now properly described by Prof. Salisbury in the New Jersey report (from its place of greatest development in that State), under the name of 'Jamesburg,' of which he says there can be no doubt that it corresponds to the Columbia. This deposit as developed on the Pennsylvania side of the Delaware River, from Philadelphia to Trenton, was very carefully studied fifteen years ago by the late Prof. Carvill Lewis, his views regarding it being embodied in various papers published about that time and finally in the last chapter of Abbott's 'Primitive Industry' (pp. 524–527), published in 1881. His conclusions were "that this clay may be assigned to a period when the land stood 150 feet or more below its present level, and when the cold waters from the melting glacier bore ice rafts which dropped their boulders."

After going over much of this field with Prof. Lewis, I adopted these views and incorporated them into my various references to the subject. (See especially Proc. of the Boston Soc. of Nat. Hist., Jan. 19, 1881, p. 141; Ice Age in North America, p. 523, and later in Am. Jour. Sci., March, 1894, pp. 180, 181.) It is gratifying to see that Prof. Salisbury's studies upon the New Jersey side of the river lead him to substantially the same conclusions. First, in opposition to Mr. Upham, he now holds that (p. 126)

"it seems certain that the formation (Jamesburg) was produced during the submergence of the area which it covers;" secondly (p. 128), that "the period of submergence must have been short;" and thirdly (p. 129), that "the amount of erosion accomplished since the deposition of the Jamesburg is slight. This is shown * * * by the undissected flats of this material, even where in close association with considerable streams. * * * Either the formation is very recent, or conditions since its development have been most unfavorable for erosion * * *. The small amount of erosion which it has suffered seems hardly consistent with its correlation with the earliest glacial epoch."

In order to understand the distinct advance here made, one has but to refer to Prof. Chamberlin's article in the *American Journal of Science*, for March, 1893, pp. 191, 192, where he enumerates among the features which he thinks 'may be accepted as demonstrative,' first, that "an older fluviatile deposit (the Philadelphia Brick Clay) is to be associated in age with the old glacial drift," and "that after the formation of this older river deposit, which took place at a low altitude and a low gradient, there was an epoch of elevation and erosion, during which the Delaware cut its channel down to the depth of 200 or 300 feet below the upper old terrace." It would seem now that this interpretation must be abandoned for the Delaware, as a similar interpretation had to be abandoned for the gravel terraces near the junction of the Conewango and the Allegheny Rivers two years ago. Mr. Salisbury is undoubtedly correct in believing that these high level gravel and clay deposits in the Delaware Valley, in the vicinity of Trenton, are of comparatively recent deposition. They are not older, but younger, than the erosion of the rock channel of the Delaware.

I may say in conclusion, also, that the investigations of Prof. E. H. Williams, in the Lehigh Valley, which have been too little noticed, seem positively to show that the river channels of that whole region had been worn to nearly their present depth of rock bottom before the earliest period of glaciation. I trust that renewed attention will be attracted to this difficult problem concerning which so many facts have now been accumulated.

G. FREDERICK WRIGHT.

OBERLIN, O., January 29, 1896.

ANCIENT MEXICAN FEATHER WORK AT THE COLUMBIAN HISTORICAL EXPOSITION AT MADRID, 1892.

TO THE EDITOR OF SCIENCE: Under the above title a contribution of mine has appeared in the recently issued Report of the U. S. Commission on the Madrid Exposition, Government Printing Office, Washington, 1895. Owing to the fact that the proofs were not sent to me for revision, my paper contains several typographical errors, three of which particularly demand correction. It being too late to rectify these errors by any other means, I have adopted the present method of doing so, with the hope and earnest request that possessors of copies of the report will duly note them therein, in order to prevent future misunderstandings. On page 332 read that I identified the shield 'of Phillip II.' at the Royal Armory, Madrid, as being of Hispano-Mexican workmanship, in 'October, 1892,' instead of '1893,' as printed.

On page 335 read the 'tiny,' instead of the *wing* feathers * * * that grow on the heads and breasts of tropical humming birds.

On page 337 read Mr. Phillip Becker instead of 'Bectier(?)' I need scarcely state that, in my original text, the name of my late, highly esteemed friend, is correctly given and is not followed by an interrogation point.

Thanking you, in advance, for kindly affording me the opportunity to do myself justice.

Yours truly,

ZELIA NUTTALL.

JANUARY 14, 1896.

SCIENTIFIC LITERATURE.

NEW DATA ON SPIRULA.

Zoölogy of the Voyage of H. M. S. Challenger: Part I., XXXIII. Report on Spirula. By T. H. HUXLEY and P. PELSENEER. VIII., 32 and 12 pp. 4°, and six plates. 1895.

The eighty-third and last part of the zoölogical series of reports on the scientific results of the Challenger expedition could not be issued in one of the zoölogical volumes on account of delays in its preparation. These delays were