of the average student" any method is dangerous. In view of Professor Hathaway's illustration, I do not feel called upon yet to "revise my eulogy on infinitesimals." E. A. BOWSER.

Rutgers College, New Brunswick, N. J., Mar. 2,

A Question of Evidence.

IN a recent number of *Science* I ventured to express the hope that a new era was dawning in American archæologic science, and that the department of geologic archæology especially would experience a needed renaissance. I laid particular stress upon the deceptive and meagre nature of the evidence already on record and ventured to point out the demands of the future with respect to certain lines of research. Some of my statements relating to the character of the evidence have given rise to sharp comment on the part of defenders of the paleolithic theory. I strongly deprecate personalities in scientific discussion and hesitate to refer in a critical way to the legitimate work of other investigators, desiring to restrict myself to such criticism as is absolutely necessary for sifting the evidence and getting at the truth; but the generalized statements by means of which I attempted to describe the old archæology are not sufficiently trenchant to be effective; more definite and detailed characterization must, it seems, be given. This can best be accomplished by means of illustrations drawn from the writings of those defenders of the faith who make most vociferous claim to superiority of knowledge and profundity of research. Numerous illustrations are at hand, but I will refer only to the work of those who have unfairly reviewed or offensively referred to the positions taken by me. Attention has been called in Professor Wright's work, "The Ice Age," to a number of papers bearing on the paleolithic question, written by Mr. H. W. Haynes of Boston. In these papers, twelve in number, I have carefully sought references to original observations on the glacial archæology of this country, and find to my surprise that they are limited to two lines and a quarter of text. These lines include, also, reference to the discoveries of Professor Wright, Dr. Abbott, and two others present on the occasion. The record reads as follows: "Several *implements* were taken by the others, *either from the gravel, or the talus* on the river bank, in my presence, and *I found five myself.*"[1] The italics are my own, and call attention to essential features of the finds and to the fact that Mr. Haynes's investigations are expressed in five words — quite sufficient no doubt for the presentation of the matter, since the articles found were probably all modern pieces from the talus. Now, any one could find these objects in the talus at that day, and no one now attaches any value to such finds save three or four advocates of the paleolithic theory in America who hesitate to acknowledge, or fail to see the shortcomings, of their early work. The chances are a hundred to one that all talus finds and all the finds made by Mr. Haynes are Indian shop-rejects left by native workmen who utilized the argillite bowlders and masses that outcropped in the face of the bluff. But whether they were from the talus or not, I would call attention to the fact that the language used by Mr. Haynes in describing the discoveries indicates practical "ignorance" of the only essential points of the discussion of fossil man. In the first place had he known that the things he picked up "either from the gravel or the talus," as he states it, correspond exactly with the ordinary modern quarry and shop-rejects of the Trenton region, he would certainly not have ventured to class them with European paleolithic implements and to build a monument to American antiquity and to himself upon them; and, in the second place, had he known that the only legitimate proof of the antiquity of such specimens in America is geologic proof, he would not have failed to properly discriminate between those articles obtained from the gravels in place — if there were such — and those obtained from the talus. From his language it is evident that at that time he had no comprehension of the real problems involved, and could not have appreciated the necessity of the discriminating observation now considered essential by scientific men; consequently, his observations made in archæologic obscurity and geologic darkness amount to naught, and no subsequent patching-up can redeem them.

[1] Haynes, H. W. Proc. Boston Soc. Nat. Hist. Vol. XXI., p. 132.

Professor Wright, who is vigorously championed by Mr. Haynes, does not claim to have found any relic of art in the gravels, and hence probably knows nothing, from his own observation, favoring the glacial age of man in America, and I was led, in a review of portions of his published work, to question his judgment in writing so much on the finds of others, and accepting all statements that came to hand without apparent attempt at discrimination. Mr. Haynes has been more successful in his finds, having added five unverified turtlebacks to the long list of "paleolithic" strays. He may not have broken Professor Wright's record in number of papers published, but he has been less discriminating in the use of unsound data. Having little knowledge of native art and less of geology, he has rarely touched the subject of glacial man without adding to its obscurity. His most pronounced shortcoming is, however, in the line of original research; when the three lines recording his complete achievements in the American field are cut down to five words, as quoted above, and these words reduced to their *real bearing* upon the question of glacial man in America, we have only the punctuation left! It would be difficult to find within the whole range of scientific writing three lines containing less of science or evincing a greater degree of incompetence to treat of the subject discussed, than these.

Another example of "that half wisdom half experience gives" may be cited. In a recent publication, Mr. Haynes avers that I have rashly and wrongly characterized the work of other investigators; yet a hurried glance into his part of that work convinces me not only that I shall be acquitted of this charge, but that I may now safely venture farther. I am constrained, therefore, to suggest that perhaps Mr. Haynes's investigations of paleolithic man in Egypt — in the only field in which he can possibly lay claim to having added a single new fact of importance to the data of archæologic science — will not require more than five words for their proper record. A brief summary of these researches may be given.

Scattered over the surface of the ground in the valley of the Nile he found several implements of supposed St. Acheul type and numerous examples of other flaked objects of ordinary and extraordinary shapes. We learn, however, in his own words, that "Quaternary deposits do not occur in the Nile valley, so far as I am aware, though they have been found in various parts of the Sahara."[2]

The "implements" of St. Acheul type are assumed to be paleolithic because of their looks. This is the "evidence" of the ordinary paleolith hunter, and it does not appear of the least consequence to him that the quaternary deposits which alone could furnish the only real element of proof of antiquity — the geologic element — are not found in the Nile valley at all, but are said to exist somewhere in Sahara. These enormous leaps from meagre data to full-blown conclusions are characteristic of the past archæology, and awaken feelings of amazement in the minds of practical students to-day. Even if analogies of form in implements are allowed to have a definite value in cultural or chronologic correlations in Europe and adjoining lands, it must be insisted that in America, until types of flaked objects other than those found commonly in Indian shop-refuse heaps are established, the test of antiquity shall be a geologic test.

The two illustrations given serve to indicate my reasons for raising the question of competency with respect to the evidence relied upon to establish a paleolithic glacial man in America. Observations of the class cited, howsoever greatly multiplied, can never amount to proof, demonstrating rather the lack of it. My position with respect to this point need not be misunderstood: when a single artificial object is found that can be fully and satisfactorily verified geologically, I shall gladly join hands with other students in making it a nucleus about which to arrange all that are clearly fellows with it. Then, and not till then, will uncertainty become certainty, and not till then can the question of the grade of glacial art be taken up and profitably studied. I only ask that the evidence relating to glacial man be properly scrutinized, and that meanwhile paleolithic man in America shall bide his time.

[2] Haynes, H. W. "The Fossil Man," Popular Science Monthly, Vol. XVII., p. 358.

While awaiting the discovery of new evidence tending to establish a glacial man in America, I have undertaken to analyze the old testimony as embodied in the writings of investigators of the American questions, and short papers covering part of this ground will soon appear. I had not anticipated this present diversion, however, as I had thought of Mr. Haynes only as a convenient verifier of that large class of unfortunate "paleoliths" whose pedigree happens to be shaky. My work was intended to bear only upon that of real investigators, such as Abbott and Cresson and Metz, who have for years sought earnestly, if not always effectively, for the evidence that is to make symmetric the culture development of two hemispheres. Those writers who undertake to use, and defend the evidence collected by, these students, will do well to remember that they shine by borrowed light, and should for much-vaunted modesty's sake, if not for science sake, keep well within reach of its limited ray.

If my "rash" assertions, hitherto made, respecting the nature of the testimony relied upon to establish a glacial, paleolithic man in America, lead finally to a just estimate of the real evidence and to the establishment of a firm basis for future operations in this great field, I shall feel amply repaid, notwithstanding the storms of sharp words and the streamlets of doggerel the publication of these views seems destined to call forth.

W. H. HOLMES.

Washington. D.C.

The Neanderthal Skull.

IN reference to Professor Haynes's observation in *Science*, Feb. 24, p. 107, that, not having seen the report of Professor Virchow's address, he could not judge "how far Dr. Brinton may have been misled by his authorities," I beg permission to furnish both him and other readers of *Science* the opportunity of judging, by quoting Virchow's precise words about the place and surroundings of the Neanderthal skull. They are as follows: —

"Für die Beurtheilung dieser Gebeine ist es von Wichtigkeit zu erwähnen dass dieselben aus keiner Höhle herstammen; auch hat man sie nicht an ihrer Lagerstätte aufgefunden, niemand hat sie ausgegraben, sie sind in Bezug auf die geologischen Verhältnisse, unter denen sie sich befanden, nicht Gegenstand der Beobachtung gewesen. Sie wurden gefunden in einer Schlucht, die zunächst eines Bergabhanges sich gebildet hatte; durch diese Schlucht waren Wasser herabgekommen und hatten allerlei herausgespült; wo die einzelnen Stücke früher gelegen hatten, wusste niemand. Darunter befanden sich auch das Bruchstück des Schädels."

Professor Haynes refers to the finder, "Dr. Fuhlrott" (evidently meaning Fullroth). This person's statements are seriously questioned by Professor Virchow, apparently from information derived from Mrs. Fullroth, who imparted it in unsuspecting innocence of the grave decisions involved; as the Professor gleefully narrates. Virchow's earlier report will be found in the Verhand. der Berliner Anthrop. Gesell. for 1872.

D. G. BRINTON.

Philadelphia, March 1.

Aerial Bubbles.

THE account of "snow-rollers" in your recent issue recalls an atmospheric phenomenon which was beheld here by two witnesses of unimpeachable character several years ago, of which no account has ever been published. Towards sunset, late in April, 1886, on a warm, thawing day, the snow rapidly disappearing, two men, Capt. John E. Hetherington and Mr. Marcus Sternberg, as they rode down the long hill towards this village from the east, saw what appeared to be innumerable spherical bodies floating in the air like soap-bubbles. Both men saw and wondered at the appearance for some moments before either spoke. Capt. H. then said, "I wonder whether I am dreaming?" The other rubbed his eyes and echoed the sentiment. "Well," said the captain, "I wonder if you see what I see; what do you see?" They questioned each other, and both agreed as to their impressions. An orchard lay along the lower and northwesterly side of the road, and all in among the apple-trees were thick, gently-descending multitudes of these bubbles, pretty uniform in size, say, 8 or 9 inches in diameter, apparently; none less than six; no small ones being observed.

The two observers state that they carefully fixed their attention on particular bubbles, in order to compare notes, and saw them seem to rest on the bough of a tree, or the top board of the fence, and then gently roll off and disappear or go out of sight. The sun was sinking and dropped below the opposite hills as they reached the foot of the long descent and entered the village, and the appearance came to an end. But up to this time the air seemed to be filled with these transparent floating spheres. The position of the observers with regard to the light seems to have made some difference as to seeing well this or that large aggregation or swarm that one or the other pointed out. The bubbles were highly colored, iridescent, gave the same sort of reflections as soap-bubbles, and apparently vanished individually in much the same way. All these points I have ascertained by repeated conversations.

Captain Hetherington (Lieuteant Colonel by merit) is widely known for his extensive apiaries, the largest in the country, and is an exceptionally good observer. Mr. Sternberg also is a gentleman of intelligence and careful observant character.

The only theory I have been able to form to account for such a phenomenon is, that if a certain kind of dust floated off in the air, each particle composed of some sort of saponaceous envelope, enclosing a highly expansible centre or core, like ammonia, — particles of this character expanded by the warm air, and at the same time moistened, might, under very nice conditions, produce such an effect.

I will add, *apropos* of snow-rollers, that Mr. Sternberg states that, years ago, he once saw, in Schoharie County, what he called "auger borings" of snow; which he described as spiral rolls, about two inches in diameter, and broken into fragments of various sizes, like the borings turned out by an auger.

HENRY U. SWINNERTON, Ph.D.

The Parsonage, Cherry Valley, N.Y.

Hardy Towhee Buntings.

HAVING noticed the effect of the recent severe weather on the crows near Washington, which Dr. Ridgway gives an account of in *Science* of Feb. 10, I was greatly surprised to find the towhee bunting (*P. erythropthalmus*) evidently wintering here. During the second week in January last, I observed two individuals and heard the notes of others. As the towhee seems to get most of its food upon the ground, its presence during deep snows and severe cold rather surprised me. The authors of the U. S. National Museum Bulletin, No. 26 (Avi Fauna Columbiana), say of the towhee: "Chiefly a spring and autumn migrant. A few breed with us, but none remain during the winter." It usually makes its appearance here in the first warm weather in March, and I have found it to breed quite abundantly in suitable localities. During the same cold snap I picked up numbers of dead goldfinches, juncos, and native sparrows, evidently victims of the weather. The turkey vultures (*C. aura*) also suffer from the cold and are sometimes found unable to fly, their plumage being coated with snow and ice. In order to prevent the extermination of the bob-white during the past winter, a Virginia sportsman's club furnished quantities of wheat-screenings to any persons who would place the same in localities frequented by the birds.

ALBERT B. FARNHAM.

Bennings, D.C.

The Speech of Children.

THE paper in *Science* of March 3, having the above title, by Mr. A. Stevenson, has much interested me. In the fifth paragraph, on page 120, the author says: "The child apparently regarded himself only as object and not at all as subject." This conclusion is reached by the child's use of the third person in speaking of himself. It seems to me inconceivable that a conscious being should regard himself other than as subject. The peculiarity of expression — a common enough one in children — I believe to exist, first, because the child hears himself constantly referred to