interest, therefore, surrounds this remote period. History is silent about it, and archæology alone can guide us. This wondrous science reveals two diverse civilizations in that area during the early iron age, separated probably rather by a few hundred years of time than by a few hundred miles of space.

The first is represented by the remarkable cemetery of Hallstatt, near Salzburg. This locality discloses a people skilled in working bronze, gold, and iron, manufacturers of richly decorated and gracefully formed pottery, lovers of ornaments of amber, glass, and agate, and accustomed to cremate their dead. We may place them 500–800 B.C.

The late iron age is the La Téne period, one or two centuries before the Christian era, deriving its name from a station in western Switzerland. By that time the working of iron had reached a singular perfection; glass, gold, silver, and precious stones were frequent; the dead were buried in stone coffins, and a local coinage was for the first time issued in metallic pieces, now popularly known by the name "rainbow-keys."

Recent studies on this period are those of Dr. Jakob Heierli of Zurich, in the December number of the Proceedings of the Vienna Anthropological Society, who describes a La Téne station in eastern Switzerland; one by Dr. L. Niederle, in the Report of the International Congress of Pre-History at Moscow, discussing the age of iron in Bohemia; and an address by Von Troltsch before the German Anthropological Society with reference to it in southern Germany.

Enigmatical Stone Implements.

In *Science*, Jan. 6, Mr. Walter Hough describes a form of polished stone implement with grooved surfaces, and suggests that these utensils were employed in beating out fibrous bark for clothing, paper, etc. This suggestion is not improbable, and has been accepted by some curators. In the Trocadero Museum, Paris, these stones are labeled "Armatures de maillet à battre les fibres d'agave." In the University Museum, Philadelphia, one bears the label, "Pounder said to have been used in pounding the agave in making pulque." There is no doubt of the correctness of this identification. The Mexicans called these implements *amatequini*, paper beaters, from the verb *amauitequi*. Mr. Hough is also right in surmising that the Mexican paper was not made from the agave alone. Other materials were the bark of the "Cardia," a tree of the family Boraginaceæ, and palm leaves, *hojas de palma*, which Boturini says made the finest of all. An article on the *amatequini* may be found in *La Nature*, Dec. 15, 1888.

Another strange implement or ornament is the stone yokes or collars which are found in eastern Mexico. In the *Internat. Archiv für Ethnographie*, 1892, Dr. Ernst of Caracas has an interesting article on these. He believes them to be memorial tokens of great individual achievements and worn as signs of power and dignity, on certain ceremonial occasions. Mr. Strebel, who wrote an article some years ago on the same subject, entertained a similar opinion. As they are quite heavy, often weighing about sixty pounds, some have supposed they were intended to fasten the victim to the sacrificial stone, the *techcatl*. They are evidently not adapted for this, however. I would suggest that they were the stones used in the game of ball, *tlachtli*, described by the early writers, enclosing the aperture through which the ball was to be driven. Some are closed with an armature, one of which is figured by Dr. Ernst. They are to be distinguished from the stone yokes from Porto Rico.

Recent Researches in South American Ethnology.

South America offers as large an unexplored region as Africa, and one with as promising possibilities. Strange that it has not attracted more attention from adventurous travellers! One of these, M. Henri Coudreau, has accomplished three expeditions, at the instance of the French government, into the far interior of Guiana. His general results have appeared in various works, as "La France Equinoxiale," "Chez Nos Indiens," etc. Lately, his linguistic collections have been edited by the competent hand of M. Lucien Adam, in a volume forming Tome XV. of the *Bibliothéque Linguistique Américaine*, published by Maisonneuve, Paris. It contains ample and carefully prepared vocabularies of the Ouayana, Aparai, Oyampi, and Emerillon dialects The first two are shown on abundant evidence to be members of the Carib stock, while the two latter are Tupi dialects.

Ernesto Restrepo Tirado is a young and active archæologist of the Republic of Colombia, equally enthusiastic in field-work and in historical studies; as is well shown in his "Estudios sobre los Aborigenes de Colombia," the first part of which, a volume of 180 pages with a good map, was published in Bogota last year. It begins with an extraordinary list of the tribes who occupied the territory at the time of the conquest, largely drawn from the epic of Juan de Castellanos. That Mr. Restrepo had the courage to read the 110,000 verses which compose this epic is reason enough to entitle him to our profound respect. Of course, a great part of his study refers to the Chibchas, who had the highest culture of any Colombian tribes. They were, however, not the most skilful workers in gold. This honor belonged to the Quimbayas, upon whom he has written a long essay, separately published. As their wealth led to their early and complete destruction by the Spaniards, their ethnic affinity has not yet been determined.

The University of Zurich possesses the rare treasure of five skeletons of members of the Alakuluf tribe of Tierra del Fuego. It seems these wretched islanders were taken to Europe to show in museums, and by some strange fatality all died at Zurich of pneumonia. Dr. Rudolph Martin has worked up their osteology and published his results in the *Vierteljahrsschrift der Natur. Gesell.* in Zurich. He finds the skulls well shaped, mesocephalic, with relatively large cubical capacity, 1590 cubic centimetres, and the horizontal circumference greater than that of the modern Parisians, as reported by Broca. The torsion of the humerus was less than in Europeans, and two of the humeri showed perforation of the fossa of the olecranon. The study is an exact and an interesting one.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

A Reply to Professor Hathaway.

I HAVE just read the note of "praise and criticism" on my books by Professor Hathaway in *Science* of Feb. 17. Kindly allow me a few words in the way of reply. Passing over the first part of his note, and thanking him for any praise of my books which he has given them, I come to what he calls his "illustration of my treatment and use of the method of infinitesimals." He says: "Thus, by trigonometry,

$$\sin(x+dx)+\cos(x+dx)=\sin x\sqrt{2}\cos\left(\frac{\pi}{4}+dx\right)$$
$$+\cos x\cos dx+\cos x\sin dx$$
$$=\sin x+\cos x+\cos x\,dx,$$

since $\sqrt{2}\cos\left(\frac{\pi}{4}+dx\right)=1$, $\cos dx=1$, $\sin dx=dx$,

Hence $d(\sin x+\cos x)=\cos x\,dx$, a false result."

Of course, it is a "false result"; who would expect anything else when the work in it is false? But this is Professor Hathaway's work; not mine. His statement, made above, that "$\sqrt{2}\cos\left(\frac{\pi}{4}+dx\right)=1$," is *not true*. For,

$\sqrt{2}\cos\left(\frac{\pi}{4}+dx\right)=1-dx$, as any mathematician can see.

Therefore, $d(\sin x+\cos x)=\cos x\,dx-\sin x\,dx$, a *true result*.

Professor Hathaway has given the above illustration, as he says, to show how I "establish the differentials of the trigonometric functions"; though I should have never known it if he hadn't told me; and I deny that I should ever have taken this roundabout way. I hope that Professor Hathaway will not give the credit of his "false result" to the infinitesimal method, which he says "is at best a dangerous one, even in the hands of the masters, let alone the average student." I think, on the contrary, that the method is a safe one, when well understood. "In the hands

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.