semblance in all particulars which it bears to those unquestioned palæolithic implements [which he exhibited beside it] of the Old World." This implement is not a "reject," but is a finished implement, with the secondary chippings all around the edge. The cuts, reproduced from photographs, on pages 252 and 253 of my volume on "Man and the Glacial Period," perfect as they are, by no means do the implement justice.

I promptly gave an account of this discovery in *The Nation* in its issue for April, 24, 1890, and repeated it in substance with some additional particulars on page 620 of the third edition of my volume on "The Ice Age in North America." The account in my later volume is still more condensed. The detailed evidence is published in Tract No. 75 of the Western Reserve Historical Society, Cleveland, Ohio, which contains the report of the meeting when Mr. Mills was present and gave his own testimony. This was held Dec. 12, 1890.

The facts are these: There is a glacial gravel terrace in Newcomerstown at the mouth of Buckhorn Creek, where it enters the larger valley of the Tuscarawas River. There can be no question about the glacial age of this terrace. It is continuous up the river to the terminal moraine. Its surface is about 35 feet above the flood-plain of the Tuscarawas; it consists of stratified material, containing many granitic pebbles and much granitic gravel. The deposit at Newcomerstown extends over many acres, having been protected from erosion in the recess at the mouth of Buckhorn Creek. Through the middle of this deposit the railroad has cut its road-bed, and for years had been appropriating the gravel for ballast.

Mr. Mills is an educated business man, who had been a pupil in geology of Professor Orton of the State University, and had with him done considerable field-work in geology. Mr. Mills's character and reputation are entirely above suspicion. In addition to his business he took a laudable interest in the collection of Indian relics, and had in his office thousands of flint implements, collected by him and his associates in the vicinity, who had been organized into an archæological society. His office was but a few yards' distant from the gravel pit from which I have said the railroad had been for so many years obtaining ballast. The perpendicular face of this bank of gravel as it was exposed from time to time by the excavations of the railroad men was frequently examined by Mr. Mills, not with special reference to finding implements, for that thought had not entered his mind, but for the sake of obtaining specimens of coral, which occasionally occurred in the gravel. While engaged in one of these rounds on the 27th of October, 1889, he found this specimen projecting from a fresh exposure of the perpendicular bank, 15 feet below the surface, and, according to his custom, recorded the facts at the time in his note-book. There was no lack of discrimination in his observations, or of distinctness in his memory. There is no possibility of any doubt about the undisturbed character of the gravel from which Mr. Mills took the implement with his own hands. The photograph of the bank, to which I refer in my volume, is not, as I say, of the same one from which this implement was taken, but it is so like it that it illustrates the character of the problem just as well. I will, however, speedily prepare an illustration from photographs of the terrace at Newcomerstown.

These facts, submitted at the meeting of the Western Reserve Historical Society referred to, were fully detailed upon the spot to myself and a party of gentlemen, consisting of Judge C. C. Baldwin, E. A. Angell, Esq., Wm. Cushing, Esq., all lawyers of eminence, and Mr. David Baldwin, who accompanied me in a visit to the place on the 11th of April, 1890. We had all the opportunity to question and cross-question that could be desired. Now this is only one case, but it comes in as cumulative evidence with other cases; that of Dr. Metz of Madisonville being almost equally good. I will only make a further passing reference to the evidence at Trenton. Dr. Abbott is not the only competent person who has discovered implements at Trenton in undisturbed gravel. In addition to those mentioned in my communication for Nov. 11, Mr. Lucien Carr has specifically stated in two different meetings of the Boston Society of Natural History (see their Proceedings for Jan. 19, 1881) that he, in company with Professor J. D. Whitney, found several implements at Trenton, one of which was in place "under such circumstances that it must have been deposited at the time the containing bed was laid down."

I submit that this evidence is neither "chaotic" or "unsatisfactory," but is as specific and definite and as worthy to be believed as almost anything any expert in this country, or any country, can be expected to produce. If the public cannot be convinced by such evidence, it is doubtful if any expert will be able to convince them. "If they believe not Moses and the prophets, neither will they believe, though one rise from the dead."

No one will have any objections to Mr. Holmes beginning the investigations anew, but many will object if, when he makes discoveries of relics of man in glacial deposits, he shall claim that they are the first discoveries of the kind which have been made in America.

G. FREDERICK WRIGHT.

Oberlin, O., Jan. 27.

Palæolithic Man in North America.

IF the weight of opinion may be considered as having settled any question, the fact that in some part of the world man once existed in so low a stage of culture as to have possessed only implements rudely chipped out of stone may be regarded as established. If this so-called "palæolithic man" existed anywhere else, why may we not suppose that he has lived on this continent also? To hold the contrary is to imply that this part of the world was not peopled until mankind had developed into the neolithic stage of culture. With such an *a priori* probability, therefore, of finding proofs of his existence here as well as elsewhere archæologists have applied themselves to the task of searching for such evidence in this country. But when archæologists make use of the term "implements rudely chipped out of stone," they have in mind certain well-known and perfectly defined objects. They do not mean pebbles showing the marks where certain portions have been casually detached by blows. By the term "palæolithic implement" the instructed archæologist intends certain definite and fixed types of chopping or cutting utensils, which have been found in large quantities, more especially in western Europe, both in gravel beds of ancient quaternary rivers and sealed up in caverns by overlying layers of stalagmite. These chipped implements have a *facies*, or family likeness, that is unmistakable, and they are accompanied by the remains of certain extinct animals, which furnish a guarantee of their great antiquity. They are implements perfect, complete, and finished in themselves, and not merely objects rudely blocked out to a general outline of the shape intended to be given to them by subsequent toil. They are entirely unlike those rude beginnings of implements which were intended to be perfected by being ground down to a polished surface. Such unfinished articles are quite as common as the polished stone axes themselves, both in Europe and in this country, but no competent archæologist would ever confound one with the other. The general appearance of a series of palæolithic implements and of a set of unfinished, chipped, neolithic implements is entirely different. Thus the term "palæolithic implement" has become a perfectly established technical term, and archæologists, understanding well its full meaning, have accordingly sought for examples of it in the river-gravels of North America. They have confidently asserted that they have found such, not in large quantities, it is true, but sufficiently to establish the fact that palæolithic man lived here also, as well as in Europe, Asia, and Africa.

But quite recently there has been put forth by a little knot of men, principally connected with the U. S. Geological Survey, the claim that this conclusion is entirely wrong; that no palæolithic implement has ever been discovered in this country, and that those objects which are claimed to be such are merely "rejects," or imperfect or unfinished articles left behind by the natives who were found in possession of this continent, and who were then living in "the age of polished stone."

"With that half-wisdom half-experience gives" these geologists, whose archæological studies have been limited to our native Indian tribes and their remains, have had the assurance to maintain that the so-called "palæolithics" of this country are nothing more or less than what are sometimes styled "turtle-backs," or those unfinished polished celts, one of whose sides has had less

material detached from it than the other. This is the whole question in a nut shell; certain Washington geologists claim to know everything about palæolithic man, and that those who disagree with them are utterly ignorant of the subject. But they have put forward this preposterous claim in the most offensive and contemptuous manner possible, using language in regard to those who differ from them such as no gentleman would employ, and wrapping up their conceited ignorance in a cloud of fustian, which appears to pass for philosophical writing in the atmosphere which surrounds them. That this style of "argument" is confined to a very limited circle would seem to show either that the word of command has been given out from some autocratic source, which they dare not disobey, or that they are actuated by jealousy at the success that has crowned the labors of those who maintain the existence of palæolithic man in North America.

Only a jury of the acknowledged pre-historic archæologists of the world is competent to pronounce judgment upon this question.

HENRY W. HAYNES.

Boston, Mass., Jan. 24.

Criticism of the U. S. Geological Survey.

THE frequent complimentary notices and encomiums upon the U. S. Geological Survey that have appeared in *Science* without any adverse criticisms, might lead one not conversant with the subject to suppose that the Survey reflects the geological learning of this country, or that it is rapidly discovering the resources, or in some other way is giving *quid pro quo* for the money expended.

Looking upon the Survey as a public matter, it is a proper subject of criticism, by any citizen, and among those who have given it any attention, with whom I converse or correspond, not one expresses satisfaction, and generally they have only words of severe condemnation.

The Director has called special attention to it by his article in *Science* of Jan. 13, and stated his claims for the work accomplished. He says:

"When the bureau was instituted, in 1879, it was found at the outset that there were no adequate maps of the regions selected for survey; and it soon became evident that the geologic work could not be carried on without maps showing the relief of the land as well as the hydrography and culture. Accordingly, topographic surveys were instituted in each of the regions selected for examination. At first these surveys were planned to meet immediate needs, and the methods of mapping were not systemized or unified; the scales were diverse and the methods various; the areas were selected by geologic needs and were not fitted to a general scheme for the geologic map of the country, and the resulting maps were discordant in their conventions. At this stage the topographic surveys were executed under the direction of the chiefs of the geologic divisions. After two or three years of trial this form of organization was found unsatisfactory, and the topographic surveys were separated from the geologic work and assigned to a geographic division, which has ever since been maintained."

In short, he says, at the outset, it soon became evident that the geological work could not be carried on without maps made by a topographical survey and accordingly the topographical surveys were instituted, but after two or three years of trial this form of organization was found unsatisfactory, and the topographical surveys were separated from the geological work. I will agree with him that, for the first two or three years, "the methods of mapping were not systemized or unified," and I am willing to believe they were of little or no geological value, and I am willing to agree that after two or three years of experience and study he ascertained that a topographical survey belongs to geographical work; but there are two matters arising from his statement that are not exactly clear, viz.:

1. If it was evident, at the outset, that geological work could not be carried on without a topographical survey, why was it necessary, within two or three years, to separate the topographical surveys from the geological work?

2. Was there, at the outset, any intelligent geologist or geographer, in the United States, not connected with the U. S. Survey, who did not know that topographical surveys belong to geographical work?

We do not desire any play on words and, therefore, come at once to the question, What geological work has been done by the Survey that is of any general benefit to the science, or that is of any economical value, or that is of any general application to the stratified rocks of the continent? For my part, having examined nine of the Annual Reports, and observed nothing of general scientific value or utility (excluding a few definitions of fossils), I would answer this question negatively. And if there is work that might possess some geological value as a preliminary reconnoissance, such work is more than destroyed by inexcusable provisional names for the groups, without characterizing them or stating the fossils by which alone their places in the geological column are to be determined. (I do not use the word "group" in the sense in which it is used, generally, in the survey, but I use it in its established geological sense.)

A lawyer in any State can go into any court in any other State or into any of the courts of the United States or into those of Canada or England and hear and understand the technical words of the science. No word will be used by any judge or attorney with which he is not familiar and it will be used in the exact legal sense in which he learned it and used it at home. More law books have been published than belong to all the sciences of natural history combined, but no one in centuries has proposed a substitute or provisional word for any technical one in use, though it cannot be denied that more expressive or euphonious words might, in some instances, be proposed. Blackstone made his fame by abstracting the technical definitions from the opinions of the courts, as written in the books, with full references and citations to his authorities, and it is for that reason alone that the use of his commentaries can be justified in any law school in this country. The whole value of precedents and court reports is in the fixity of the technical words used and their established definitions. What the science of geology demands is fixity in the names of the subdivisions of the stratified rocks, and the accurate determination of the fossils that characterize each subdivision, for by the fossils alone can the subdivisions be determined. And these demands have been wholly disregarded and set aside by the U. S. Survey since 1879, and we have synonym after synonym for equivalent rocks, vague and worthless definitions, and what seems to me the culmination of absurdity if not crime against the progress of geological knowledge, the pretension that they are developing a "New Geology."

This matter of nomenclature alone, in my opinion, will everlastingly condemn the Survey, so far as it deals with stratigraphical geology, and make students of the science wish there had been some power to suppress the publication even if it was necessary to expend the appropriations. It would have been better to have given the money to the printer and consigned the stratigraphical manuscript to the flames.

But, aside from the questions of nomenclature, that are so intimately connected with learning, and so vital to the understanding of any subject, there are numerous fundamental errors. If any one will turn to page 372 of the Seventh Annual Report, under the head of "Paleontological Characters as a Basis for Classification," he may read pages in consecutive connection where every idea expressed will be recognized as absolutely erroneous by any competent paleontologist. I will quote only a single sentence. He says:

"We have now constantly to remember that paleontology is based wholly upon stratigraphy, and consequently that the conclusions that we would draw from our fossils must constantly be checked by stratigraphical observations."

This statement is made, in the face of the fact, that no species in the great Subkingdom Echinodermata is known to have a vertical range of 500 feet, in the palæozoic rocks of North America; that not one is known to cross the line subdividing the groups of rocks recognized in the Geological Surveys of New York, Pennsylvania, Illinois, Indiana, or Canada; and in the face of the fact, that science has not recognized a group of rocks within the