amplitude is to my mind incompatible with innumerable observations of *what did not happen* in Charleston. I admit the difficulty of the problem, but think it easier to account for large displacements by successive movements of small amplitudes.

I must also dissent from the opinion expressed as to the value of stopped clocks as a means of determining the time of the wave-transit. Is it not likely that most of the inconsistencies which appear on a comparison of such data arose out of the fact that many of the clocks were not correctly regulated to 75th meridian time, or that their errors were not known? The man whose clock or watch is 'just right' is met with at every turn, especially after an earthquake; but to most people this means that the error is not greater than a minute or two.

If all of the stopped clocks in the area disturbed had been in exact agreement before the shock, I do not think the errors would have been very great; except, perhaps, in the immediate vicinity of the source. The stopping of all clocks at any considerable distance probably occurred at the transit of the same great wave. Of course, a properly adjusted seismoscope with a clock attached is infinitely better, but I do not have great confidence in the 'observer with watch in hand.' Most intelligent observers in this country must be classed as inexperienced: the watch is not generally in his hand until after he is convinced that the something which has happened is an earthquake, and then it is very likely to have a large and unknown error. Should the disturbance be so considerable as to threaten to be destructive, the skill of the observer in 'measuring a part of the shock and estimating the beginning' is tolerably certain to be overshadowed by his disposition to seek a place of safety. The position and environment of the observer at the time of the occurrence will greatly influence the character of the phenomenon. As an illustration, I may compare my own observations with those of Professor Newcomb, when the Charleston earthquake was felt in Washington City.

I was seated in my library on the second floor of a three-story brick building, about four squares from the state, war, and navy building. As soon as the disturbance was felt, the time was noted. In a moment the motion became very strong. My small boy, who had been awakened out of a sound sleep, rushed into the room; and the family quickly decided to do what it had often done before under such circumstances, and found its way to the street. By the time this was accomplished all was quiet; and in two minutes from the beginning we were again seated in the same room, discussing the shock. In a few minutes, about five from the beginning, another shock occurred, much less violent than the first.

Professor Newcomb "observed a duration of perceptible tremors, with two maxima lasting about five and one-half minutes."

There is, of course, no doubt but what these tremors were felt, but it may be a question whether they were prolonged vibrations of the building in which Professor Newcomb was, or real earth-movements. I am pretty sensitive to earthquakes, and I can say with certainty that they were not felt by me or by my family.

Everybody, I am sure, will agree that it is highly important to establish a large number of observing-stations, equipped with the best instrumental appliances which can be obtained. Even so small a number as ten or twenty such stations, well distributed over the area disturbed by the Charleston earthquake, would have put us a long way in advance of our present knowledge of seismology. It is greatly to be hoped that the able and interesting discussion of the subject, which Messrs. Dutton and Hayden have evolved from the mass of observations which they have gathered with so much industry, will serve to direct the attention of intelligent people to the importance of such a system of observing-stations, and that in the near future the director of the geological survey will be enabled to establish it.

T. C. M.

Terre Haute, June 1.

Museums of ethnology and their classification.

The remarks of Dr. Boas and Professor Mason on the classification of ethnological material raise questions which must occur to every one who has before him unclassified material. As both views include a part of the truth, the decision on the course to be adopted must depend upon the amount of material to be handled, the space available for its exhibition, and the purpose most at heart in the organization of the museum considered as an agency for effecting a purpose.

The ideal way, if all circumstances were favorable, would be to have a double series,— one representing the culture of each people as an ethnic unit; and the other a comparative collection illustrating the relations to a common standard of the items making up each tribal aggregation. In ninety-nine museums out of a hundred, this would be impracticable, owing to the expense involved, the exhibition space required, and the difficulty of obtaining sufficient duplicate material for two series. The decision must therefore depend on the object to be attained. Is this to show the manner in which tools, weapons, dress, etc., have been elaborated, under the operation of the environment, by the human mind in varying stages of development, or is it rather to convey to the observer the resultant of all the forces acting in and on a comparable series of ethnic types or units, each complete in itself? In either case the object is a worthy one, and to be attained in its particular manner. Neither is likely to be completely attained under the existing conditions of museums in this or any other country; but, as attempted in different collections, we may regard them as complementing each other. In the one case, as very truly observed by Dr. Boas, we are helped to a knowledge of what problems exist; and it is no little matter to have a rational sailing-direction over a trackless ocean, though the accurate chart is still to be made. In the other, we have the equivalent of the monographic study of the specialist who surveys in detail, and for all time, a gulf or harbor forming a small part of the oceanic coast.

To conclude, for the people at large and the majority of those who profit by public museums, I believe the greatest amount of satisfaction and instruction is to be obtained rather from an ethnic arrangement than from the organic method; but this is merely an expression of my individual preference.

WM. H. DALL.

Washington, D.C., June 4.

Prof. Otis T. Mason's reply to my remarks on his views of the methods of ethnology is mainly a justi-

fication of his plan of arranging the collections of the national museum. As this plan is the outcome of his philosophical view of the problems of ethnology, we must scrutinize these in order to judge as to the merits of his system.

His principal object is the study of each and every invention among peoples of all races and countries. I am well aware that this idea was and is shared by many scientists; and at this very moment I read with interest Mantegazza's proposal of erecting a 'psychological museum,' i.e., a museum of ethnological objects arranged according to the ideas to which they belong. Professor Mason's rank among American ethnologists, however, and the weight he can give to his opinions by the arrangement of the large collections of the national museum according to his theories, induce me to criticise his views more particularly.

My view of the study of ethnology is this: the object of our science is to understand the phenomena called ethnological and anthropological, in the widest sense of those words,—in their historical development and geographical distribution, and in their physiological and psychological foundation. These two branches are opposed to each other in the same way as are biology and the so-called systematic 'organology,' or, as I have called it in another place (*Science*, ix. No. 210), when treating on the study of geography, 'physical science and cosmography;' the former trying to deduce laws from phenomena, the latter having for its aim a description and explanation of phenomena. I tried to show that both branches are of equal scientific value.

Let us inquire which method must be applied to carry on ethnological researches of either kind. Ethnological phenomena are the result of the physical and psychical character of men, and of its development under the influence of the surroundings: therefore two problems must be studied for attaining scientific results. The preliminary study is that of the surroundings: the final aim of the researches is the knowledge of the laws and history of the development of the physiological and psychological character of mankind. 'Surroundings' are the physical conditions of the country, and the sociological phenomena, i.e., the relation of man to man. Furthermore, the study of the present surroundings is insufficient: the history of the people, the influence of the regions through which it passed on its migrations, and the people with whom it came into contact, must be considered. All of these are phenomena which may directly be observed by a well-trained observer, or may be traced with greater or less accuracy by historical researches.

The second part of ethnological researches is far more difficult. The physical and psychical character of a people is in itself the result of the action of the surroundings, and of the way in which the present character was attained. Each stage in the development of a people leaves its stamp, which cannot be destroyed by future events. Thus it appears that the elements of the character of a people are extremely complex. There are two ways of treating this problem.

One of the remarkable features of such problems is the occurrence of similar inventions in regions widely apart, and without having a common origin. One method of studying them — and this is Professor Mason's method — is to compare the phenomena, and to draw conclusions by analogy. It is the deductive method. The other method is to study phenomena arising from a common psychical cause among all tribes and as influenced by their surroundings; i.e., by tracing the full history of the single phenomenon. This is the inductive method. For this method of study, the tribal arrangement of museum specimens is the only satisfatory one, as it represents the physical and ethnical surroundings.

I will explain these ideas by giving an example. It has frequently been proposed to establish a museum illustrating the adaptation of organisms to surroundings. The aim of this study is to find the physiological laws or the combination of causes which have the effect of causing these adaptations. The classification and arrangement must, of course, be made according to surroundings, in order to show their influence on different kinds of organisms.

An ethnological collection is analogous to this. The objects of study are researches on psychology. The method of researches is a study of the surroundings. The surroundings are physical and ethnical: therefore the arrangement must also be physical and ethnical, as this is the only way to show the single phenomenon in its peculiar character and surroundings.

It has been the tendency of science to confine the domain of deductive methods more and more, and not to be content with arguments from analogy, which are the foundation of most errors of the human mind, and to which may be traced the religious and other ideas of man in a primitive state of culture, and, to a certain degree, even in a state of advanced civilization. Science is constantly encroaching upon the domain of the argument from analogy, and demands inductive methods.

Nevertheless the psychological and scientific value of the argument from analogy cannot be overrated: it is the most effective method of finding problems. The active part it plays in the origin of philosophical systems and grand ideas which sometimes burst upon scientists is proof of this. But, as far as inductive methods can be applied,—and we believe that their domain will continue to increase,—induction must scrutinize the ideas found by deduction. Therefore I should call Professor Mason's system a suggestive one, but not fit for scientific researches, as it does not allow the application of the inductive method.

But even this acknowledgment must be limited. The technological idea, which Professor Mason has made the leading one in the arrangement of the collection of the national museum, is only one side, and a very limited one, of the wide field of ideas which must be leading in a 'psychological museum,' as Mantegazza calls it.

The rattle, for instance, is not merely the outcome of the idea of making noise, and of the technical methods applied to reach this end: it is, besides this, the outcome of religious conceptions, as any noise may be applied to invoke or drive away spirits; or it may be the outcome of the pleasure children have in noise of any kind; and its form may be characteristic of the art of the people. Thus the same implement belongs to very different departments of a psychological museum.

Furthermore, let us inquire what is the psychological principle upon which Mason's system is founded. The leading idea is technology. The foundation of technics is the faculty of acting suitably: consequently the purpose of the implement must be made the principle of division. For in-

stance, all kinds of cooking-pots and other arrangements for cooking would belong to one class. The mere fact that certain pots are made of clay would not justify the establishment of a pottery department. This quality of being made of clay is incidental, and does not agree with the psychological basis.

There is one point of view which justifies a classification according to inventions in a psychological museum. This is the extent to which each invention is used by a people: for instance, in what branches of life pottery is made use of, which may be limited in one tribe, very wide in another. But in this case the purpose of the object will not be the principle of division, but the principal invention applied in its manufacture; and thus the specimens would not be arranged according to Professor Mason's system, objects serving widely differing purposes belonging to one class. Therefore I cannot consider it justifiable to make technology, in the sense Professor Mason does, the basis of arranging ethnological collections.

One reason ought to make us very cautious in applying the argument from analogy in ethnology as well as in other sciences of similar character; biology, for instance. Former events, as I have already said, leave their stamp on the present character of a people. I consider it one of the greatest achievements of Darwinism to have brought to light this fact, and thus to have made a physical treatment of biology and psychology possible. The fact may be expressed by the words, "the physiological and psychological state of an organism at a certain moment is a function of its whole history;" that is, the character and future development of a biological or ethnological phenomenon is not expressed by its appearance, by the state in which it *is*, but by its whole history. Physicists will understand the important meaning of this fact. The outward appearance of two phenomena may be identical, yet their immanent qualities may be altogether different: therefore arguments from analogies of the outward appearance, such as shown in Professor Mason's collections, are deceptive. These remarks show how the same phenomena may originate from unlike causes, and that my opinion does not at all strive against the axiom, 'Like effects spring from like causes,' which belongs to that class of axioms which cannot be converted. Though like causes have like effects, like effects have not like causes.

From my statement it will be understood that I cannot content myself with Mr. Dall's remark, in the letter contained in to-day's issue, that both standpoints contain part of the truth. I have expressed in another place (*Verh. Ges. für Erdkunde*, Berlin, 1886, No. 7) my opinion on Dall's ethnological method, and emphasized, as I have here also, the necessity of studying each ethnological phenomenon individually.

In conclusion I have to add a few words on the practical side of the question upon which Professor Mason and Mr. Dall touch. In regard to this question, I concur with Mr. Dall, and believe that the public will be much more benefited by the tribal arrangement of ethnological collections.

I cannot agree with Professor Mason's proposal of arranging the cases like a checker-board. In ethnology all is individuality. We should be compelled to leave long rows of cases empty, as certain phenomena occur but in very few tribes. It would be almost impossible to show in this way all important ethnological phenomena, the historical development of tribes, the influence of neighbors and surroundings, etc. It is my opinion that the main object of ethnological collections should be the dissemination of the fact that civilization is not something absolute, but that it is relative, and that our ideas and conceptions are true only so far as our civilization goes. I believe that this object can be accomplished only by the tribal arrangement of collections. The second object, which is subordinate to the other, is to show how far each and every civilization is the outcome of its geographical and historical surroundings. Here the line of tribal arrangement may sometimes be broken, in order to show an historical series of specimens; but I consider this latter point of view subordinate to the former, and should choose to arrange collections of duplicates for illustrating those ideas, as it were, as an explanation of the facts contained in the tribal series. Of course, it is generally impossible to do this, on account of the lack of specimens, or, more frequently, on account of the lack of our knowledge; but it is my ideal of an ethnological museum. I wish to state here again that I am not at all opposed to Mantegazza's psychological museum, which will be very suggestive and important for the development of science, but I consider the ethnological museum indispensable for controlling the ideas suggested by the analogies shown in the psychological collection, and as the only means of showing the state of culture of man.

DR. FRANZ BOAS.

Correlation of the geological structure of the maritime province of Canada with that of western Europe.

I take the liberty to send a corrected abstract of a paper read by me before the Royal society of Canada, and which may perhaps be of interest to some of your readers: —

As early as 1855, in the first edition of 'Acadian geology,' the author had indicated the close resemblance in structure and mineral productions of Nova Scotia and New Brunswick with the British Islands, and in subsequent editions of the same work further illustrations were given of this fact. Recent researches by Bailey, Matthew, Fletcher, Ells, and others, had still more distinctively indicated this resemblance, as well as the distinctness of the maritime geology from that of the great interior plateau of Canada and the United States. In short, as argued by the author in his recent address before the British association, the geology of the Atlantic margins of America and Europe is substantially the same, and distinct from that found west of the Appalachians in America and in central and eastern Europe. In this fact has originated much of the difficulty experienced in correlating the geological formations of eastern Canada with those of Ontario, of New York and Ohio, as well as similar difficulties in Europe which have led to much controversy and difference of classification and nomenclature. One object of the present communication was to show that the system of classification of paleozoic sediments employed for the interior plateau of the American continent requires very important modifications when applied to the Atlantic coast, and that neglect of this has led to serious misconceptions.