I give drawings representing the corresponding parts of these two bones. The determination is not difficult; in each bone, enough of the lower border is preserved to guide one with certainty to the identification of right and left parts. In both sides the lower hinder angle of the parietal bone is broken away, but although not fractured in exactly the same manner, the lowest point in both cases may be taken as in strict correspondence. In this reconstruction then the lower border of the right parietal occupies a position nearly half an inch higher on the right than on the left side. I think that discrepancy must be due to an error in reconstruction.

I have not entered into a discussion on the markings which indicate the middle line of the skull for this reason. A very considerable experience in attempting to reconstruct ancient and modern skulls from fragments has convinced me that if a wrong bearing is taken—if one misidentifies any point in the middle line—unless it be a very slight error, that misidentification will find the reconstructor out, and his task will be brought to a halt by the development of a degree of asymmetry. If, on the other hand, points are rightly recognised—often it has to be by repeated experiment—then the parts fit easily together, provided there is a sufficiency of them, and in the case of Piltown there is an ample sufficiency. I look upon the problem of rightly reconstructing a skull as similar to that of replacing the fragments of a broken vase of symmetrical design. Given the fragments of the greater part of one half and a part of the other, there cannot be two reconstructions. All the parts may be got together except one fragment. The remaining fragment is evidence that the task has not been accomplished. I know very well that my friend Prof. Elliot Smith is searching for a true representation of the brain-state of the very earliest human form that can claim any direct relationship to modern men; I hope I may claim the same spirit for myself. I also admit that he has gone a considerable way towards what, in my opinion, must have been the original form. The points on which we disagree are now apparent, and I am content, having had an opportunity of presenting my case, to leave the final decision to the future.

ARTHUR KEITH.

Royal College of Surgeons, W.C.

Work of Natural Forces in Relation to Time.

IN the notice of the "Origin and Antiquity of Man" (*NATURE*, October 9), the remark that I have "returned to the manner of thinking which was prevalent before the days of Lyell" calls for some comment. It would be nearer correct to say that I have adopted the manner of thinking occasioned by the facts which have come to light since Lyell's day, and which may be succinctly described as that of regarding nature, not as a "uniformity," but an "evolution." Lyell's habit of regarding nature as a progression by infinitesimal steps has been corrected by later observations which reveal, at times certainly, a much more rapid rate of progress than he and his followers have been wont to admit. Lyell certainly failed to appreciate the activity even of the present forces of nature.

For example, in 1842, after a cursory examination of Niagara Falls, he put forth the estimate that their recession could not have amounted to more than one foot a year, and probably one foot in three years, thus making the age of the cataract at least 35,000 years, and probably 100,000. But at his suggestion Dr. John Hall made a trigonometrical survey of the front of the falls and set up monuments so that the rate could be eventually determined by actual measurements. After seventy years, surveys show that the

falls have receded, during the entire period, at a rate of about five feet a year.

Again, Darwin, adopting Sir Charles Lyell's methods in the first edition of his "Origin of Species," estimated that the erosion of the Wealden deposits in England required the work of 306,662,400 years, which he called "a mere trifle of geological time." But on having his attention directed to the activity of sub-aerial erosive agencies acting over the entire surface at all times, concerning which a great mass of evidence had recently been gathered, he confessed in a second edition that he had made a rash statement, and in subsequent editions withdrew it entirely. The facts accumulated concerning the activity of present eroding forces show that instead of the immense period originally assumed by Darwin, the whole removal of the Wealden strata would be accomplished in a few million years.

But it is in respect to the rapidity of glacial movements that the slow rates assumed by Lyell and his followers are pre-eminently misleading. Those whose studies of glaciers have been limited mainly to the Alps, have not readily appreciated the facts concerning the movement of glaciers in North America. For example, it was in 1886 that I made the first extended observations upon the great Muir Glacier in Alaska. This glacier presented a water front one mile in width, rising 300 ft. above the water, and descending 700 ft. below the water. From examination of various lines of evidence I was able to show that, when Vancouver surveyed the region 100 years before, the Muir Glacier with various others coming in to Glacier Bay had united to project the ice twenty miles farther south, with a thickness of two or three thousand feet. The correctness of this inference has been abundantly corroborated by subsequent observers.

But now comes the confirmatory evidence in the fact that the Muir Glacier has receded seven miles and a half in the twenty-five years that have elapsed since my first observations. Moreover, the ablation from the surface has been such as to lower it 700 ft. In short, we have here from actual observation in a glacial field larger than that of the Alps, evidence of greater changes in twenty-five years than some of those for which Prof. Penck has demanded many thousand years.

The word uniformity as applied to the action of natural forces, both in geology and biology, is unfortunate and misleading. There is, indeed, continuity. But this permits varying rates of movement according to evolutionary laws, so that, as Huxley observed, all that Darwin had to do to adjust his theory to the recent moderate estimates of geological time was to assume a more rapid rate of variation. Neither need the Darwinian be afraid of recognising paroxysms in nature, since they naturally follow the slow accumulations of strain which finally culminate in some sort of fracture or interruption of the ordinary course of events. I am not a pre-Lyellian, but a post-Lyellian.

G. FREDERICK WRIGHT.

Oberlin, Ohio, October 23.

The United States Territory of Hawaii.

DR. J. STANLEY GARDINER, in his appreciative notice of "Fauna Hawaiensis," in *NATURE* of September 25, just received, has used a name against which I must enter protest. I thought it a possible misprint, but it appears several times as *Hawaiiia*.

We shall probably have to bear Cook's name, Sandwich Islands, from our conservative English friends for some years longer, although the Hawaiian kingdom was independent many years, and never officially used that name, although having diplomatic and com-

mercial agents in England as elsewhere. Its successor, the United States territory of Hawaii, now administers the affairs of the late kingdom. Neither kingdom nor republican territory has ever sanctioned such a barbarous name as your reviewer gives—Hawaiiia.

WM. T. BRIGHAM.

Bernice Pauahi Bishop Museum, Honolulu, H.I.,
October 24.

THE first article in the "Fauna Hawaiiensis" is entitled "Introduction, being a Review of the Land-Fauna of Hawaiiia." Dr. Brigham's quarrel is hence with the writer of that article, and with the editor of the fauna, not with me. I should have expected "Hawaiiia" to meet with his approval as against the rather cumbrous title, "United States Territory of Hawaii," a title taken from the name of the largest island. The islands from Niihau to Hawaii stand on an isolated plateau in the ocean, and represent a geographical group; the name "Hawaiiia," I consider, may quite usefully be applied to them. A name will also have to be adopted for the islands between Nihoa and Lisiansky, which form a similar group; these I frequently find in maps included in the Hawaiian Islands.

Presumably the aboriginal inhabitants had no name for the islands in question, as they knew no other lands, and certainly the Spanish navigators established no name for them. Cook's name, "Sandwich Islands," dates from 1778, and clearly has priority, a fact which should appeal to American—I hope Dr. Brigham will pardon this incorrect adjective being applied to his countrymen—biologists.

J. STANLEY GARDINER.

November 14.

INTERNATIONAL CONFERENCE ON THE STRUCTURE OF MATTER.

THE first International Conference in Brussels on the Theory of Radiation in 1911 (see NATURE, vol. lxxxviii., p. 82) owed its inception to Mr. Ernest Solvay, and proved a great success. Shortly afterwards, Mr. Solvay generously gave the sum of one million francs to form an International Physical Institute (NATURE, vol. xc., p. 545), part of the proceeds to be devoted to assistance of researches in physics and chemistry, and part to defray the expenditure of an occasional scientific conference between men of all nations to discuss scientific problems of special interest. In pursuance of this aim the second International Conference or Conseil International de Physique Solvay, was held in Brussels this year on October 27–31, under the able presidency of Prof. Lorentz. On this occasion the general subjects of discussion were confined to the structure of the atom, the structure of crystals, and the molecular theory of solid bodies.

Reports were presented by the following:—The structure of the atom, Sir J. J. Thomson; Interessenscheinungen an Röntgenstrahlen hervorgerufen durch das Raumgitter der Kristalle, Prof. Laue; the relation between crystalline structure and chemical constitution, W. Barlow and Prof. Pope; some considerations on the structure of crystals, Prof. Brillouin; and Molekulartheorie der Festen Körper, Prof. Gruneisen.

Among those present at the meeting were Prof. Lorentz, Kamerlingh Onnes, Sir J. J. Thomson, Barlow, Pope, Jeans, Bragg, Rutherford, Mme. Curie, Gouy, Brillouin, Langevin, Voigt, Warburg, Nernst, Rubens, Wien, Einstein, Laue, Sommerfeld, Gruneisen, Weiss, Knudsen, Hasenöhr, Wood, Goldschmidt, Verschaffelt, Lindemann, and De Broglie.

An interesting and vigorous discussion followed on all the papers presented to the congress. Special interest was taken in the report of Laue on the interference phenomena observed in crystals with x -rays. A valuable contribution was made by Prof. Bragg on selective reflection of x -rays by crystals, and on the information afforded by this new method of research on crystalline structure. The report of Mr. Barlow and Prof. Pope on the relation between crystalline structure and chemical constitution was illustrated by a number of models, and was followed with much interest. A report on the papers and discussions at the Conference will be published as promptly as possible.

The arrangements for the meeting, which was successful in every way, were admirably made by Dr. Goldschmidt. All the members stayed at the same hotel, and thus were afforded the best of opportunities for social intercourse and for the interchange of views on scientific questions. During the meeting, the members were very hospitably entertained by Mr. Solvay and Dr. Goldschmidt, while a visit was made to the splendid private wireless station of the latter, which is one of the largest in the world, capable of transmitting messages to the Congo and Burmah.

The committee of the International Physical Institute, who were present at the conference, held meetings to consider the applications for grants in aid of research, made possible by the sum set aside for this purpose by Mr. Solvay at the foundation of the institute.

It was arranged that the next meeting of the Conseil de Physique should be held in three years' time at Brussels, when there will be a new programme of subjects for discussion. In order to extend the scope of the congress, and to make it as representative as possible, it has been arranged that the original members will retire automatically at intervals, while their place will be taken by new members, who will be specially invited to take part in discussion of definite scientific topics.

E. RUTHERFORD.

ALFRED RUSSEL WALLACE.

THE last link with the great evolutionary writers of the mid-nineteenth century—the men who transformed the thought of the world—is broken. How can I best speak of the long, happy, hard-working, many-sided life that has just come to a close? The history of Wallace's contributions to science and the details of his career have been long known, and are now rewritten and epitomised in the Press of the world. I propose to speak of the man himself as he was revealed to his friends.