that either the morality or the courtesy of the scientific world is likely to be improved by the renewed exertions on their behalf which are about to be made by Mr. Samuel Butler.

GEORGE J. ROMANES

[This correspondence is now closed.—ED.]

WILL it go any way towards calming Mr. Butler's zeal in the cause of literary honesty to remark that at any rate fifteen years ago, and it may have been further back, Mr. Darwin prefixed to "The Origin of Species" a historical sketch of the progress of opinion on that subject? In view of this it is at least very *misleading* on the part of Mr. Butler to quote the first sentence from the edition of 1859, and then to ask: "What could more completely throw us off the scent of the earliest evolutionists?" as if in those days it would have made a pin's difference to him, or any one else whom he includes in the *us*, whether the scent of the earlier evolutionists lay strong or weak in the track. In these days he should know, if he knows anything of the history of opinion, that these predecessors of Mr. Darwin, with their great though varied merits, had been laughed down, and, for all popular estimation, might be said to have disappeared. To have relied in any way on their authority when Mr. Darwin's book was first published might well have increased the mountain of prejudice against his views without in any way relieving the weight of ridicule that lay upon theirs. When the whole scientific world had been stirred to its foundations and when the whole world almost had been roused into paying attention to science by the awakening genius displayed in the new exposition *de rerum naturâ*, then, when it could best be done, Mr. Darwin turned ridicule into renown, and made all who could even remotely claim to have anticipated or shared his views participators of his fame. Not those who scatter seed at random, but those who cultivate it in chosen ground with indefatigable industry and prevailing skill should, I imagine, be considered the chief benefactors of mankind; and in like manner the fancy that may have fluttered uselessly through many brains becomes at last a fruitful hypothesis or a wide-stretching theory when it falls beneath the cultivation of undaunted genius.

T. R. R. STEBBING

Tunbridge Wells, February 7

"Prehistoric Europe"

WILL you kindly allow me a few words in reply to certain statements made by Prof. Dawkins in his notice of my "Prehistoric Europe." I shall not remark on the perplexing confusion which he gravely puts forward as an outline of my general argument further than to say, in all sincerity, that I fail to recognise in it any trace of what that argument really is. The few observations I have to make shall be confined chiefly to questions of fact.

1. Mr. Dawkins states that I ask geologists to believe that the mammaliferous gravels with Palæolithic implements, which overlie the chalky boulder-clay of East Anglia, were covered by an upper and younger boulder-clay, which latter "has been removed so completely that no trace of it is now to be seen." Now I do not believe that the gravels in question ever were covered by boulder-clay, nor have I written anything which could justify Mr. Dawkins in attributing to me an opinion so absurd.

2. The account I have given of Victoria Cave was written after a careful perusal of all that has been said about it, and my proofs were submitted to Mr. Tiddeman, who reported on the explorations; and therefore I have every reason to believe that my description is correct.

3. The so-called Upper Pliocene deposits at Mont Perrier are described in detail by Dr. Julien, who shows that they are truly interglacial, being younger than the great "pumiceous conglomerate" with its striated stones and blocks, and older than the more recent moraines of the same neighbourhood. Dr. Julien remarks: "La période pliocène supérieure doit disparaître de la science." He correlates the interglacial beds of Mont Perrier with those of Dürnten.

4. The lignites of Leffe and Borlezza, according to Prof. Stopanni, who has carefully studied those closely-adjoining districts, belongs without any doubt whatever to the glacial series; and his observations I have confirmed by a personal examination of the ground. They are generally admitted by Italian and Swiss geologists to be on the same horizon as the lignites of Dürnten.

5. I have not asserted the interglacial age of the so-called Pliocene of Olmo. The newer deposits in the Upper Val d'Arno, which have usually been assigned by palæontologists to the Upper Pliocene, have been shown by Prof. Mayer, after an exhaustive analysis of the evidence (as well stratigraphical as palæontological) to belong to the Pleistocene; and as their mammalian fauna corresponds with the fauna of the lignites of Leffe and Borlezza, I have said that this fact is "significant," meaning thereby that the beds in question may very likely be of the same age as those near Gandino.

6. Mr. Dawkins says that I deal with my subject not with the impartiality of a judge, but as an advocate, and that I have only called those witnesses which count on my side. I am probably as well acquainted with the literature of the subject as my critic, and after many years' careful reading and study must confess that I have not encountered any evidence that contradicts my views. Had it been my fortune to come upon such evidence I feel sure that I should not have been so weak and foolish, or so untruthful as to have ignored it. Doubtless I have met with many forcible statements of opinion by Mr. Dawkins that he does not agree with me; but I may remind him (and not for the first time) that mere expressions of opinion, however emphatic, prove nothing save, as a rule, the sincerity of him who utters them.

7. My critic further ventures the statement that my classification "is based on ice, and ice only." How very far this is from being the case any candid person may see who shall take the trouble merely to run his eye over the "contents" of my book. Geologists rightly refuse to accept classifications which are based upon so narrow a foundation as a single series of phenomena, such, for example, as Mr. Dawkins's attempt to classify the Pleistocene by reference to the mammalia alone—a classification which, while it draws the line that separates Pliocene from Pleistocene at the base of the glacial deposits in England, would carry the same line, in France and Central Europe, through the middle of the glacial series. Or, to put it another way, if we accepted Mr. Dawkins's classification, we should be forced to admit that the Glacial Period attained its climax in France and Central Europe during Pliocene times, but that it did not begin in England until after the Pleistocene had commenced. And this is the classification which, as may be inferred from the tenor of my critic's remarks, I ought to have adopted.

Mr. Dawkins's remarks upon my views in regard to the evidence of climatic changes I am sorry to say I do not understand. All that I am sure of is that he has quite failed to grasp my meaning—that he has attributed to me opinions which I have done my best to refute—in a word, that he has strangely misrepresented me. But I need not attempt to set him right, as those who are sufficiently interested in the matter are not likely, after this repudiation, to accept his travesty for a reliable presentment of my views.

JAMES GEIKIE

Perth, January 7

On Dust, Fogs, and Clouds

A CURIOUS confirmation of Mr. Aitken's theory of fog was brought to my notice a short time ago. A friend of mine residing in Streatham, struck with the perfection of the heating arrangements in American residences, fitted up his house with a similar contrivance. In the basement was a furnace and boiler which warmed pure air that entered from without, and circulated at a regulated temperature throughout the house. A water-pipe that was connected with the boiler became stopped by frost; an explosion ensued, and the house was filled with so-called steam (hot fog, in fact) from top to bottom. Wherever a cold surface (clock faces, metal fixtures, &c.) was found, even in the topmost bed-rooms, the vapour condensed and left behind it black carbon dust. Nowhere else was this dust found.

Again, few persons who have read Mr. Aitken's paper can have noticed the dejected appearance of the late beautiful snow on the first morning of the welcome thaw without thinking of his theory. What on the previous evening was a clean dazzling mass of exquisite white became a sooty speckled heap of dirty snow. As the sparkling crystals liquefied into water which drained away, they left behind the dust and carbon, around which, according to Mr. Aitken, they originally formed, becoming by multiplication molar and visible. In the streets of London the masses of white snow rapidly became, as somebody remarked, like streams of cold *café au lait*. The whiteness rapidly disappeared and left behind mere dirt.

It may interest some of your readers to know that in 1537