same time that they often, by an unconscious process of approval and persuasion, help to exaggerate bad qualities and develop worse.

## LYELL'S "ANTIQUITY OF MAN"

*The Geological Evidences of the Antiquity of Man, with an Outline of Glacial and Post Tertiary Geology, and Remarks on the Origin of Species, with special reference to Man's First Appearance on the Earth.* By Sir Charles Lyell, Bart., M.A., F.R.S. Fourth Edition Revised. Illustrated with Woodcuts. (London: John Murray, 1873.)

SINCE the first volume of "The Principles of Geology" appeared—now more than forty-three years ago—Sir Charles Lyell has put forth an uninterrupted series of new works or new editions, and we have now arrived at the 11th edition of the "Principles," the 7th of the "Elements of Geology," and the 4th of the "Antiquity of Man." A most striking feature of these works is, that they give the fullest and most accurate scientific details, and the most philosophical discussion of principles and results, without for a single page ceasing to be interesting to any well educated and thoughtful man. Perhaps no author has attained in so perfect a degree the art of making science popular without ever attempting to popularise it, or has produced a series of works which are equally acceptable to the experienced geologist and to the general reader.

The present edition of the well-known "Antiquity of Man" will fully sustain the author's high reputation, since it is not a mere corrected reprint of former editions, but, in several important respects, a new work, embodying all the most recent discoveries and researches on the various subjects of which it treats, while several discussions of temporary or personal interest have been omitted. Almost every chapter contains either important new facts or new results derived from a more careful study of old ones; while some are almost wholly rewritten, as, for example, chap. xii., in which the most recent researches on the climate of the Crag period is very fully given; and it would need a very acute critic to discover in these any lack of that lucidity of arrangement and vigour of thought which have always distinguished Sir Charles Lyell's writings.

The most striking additional facts bearing directly on the Antiquity of Man are so well known and have been so often before the public, that it is unnecessary to enumerate them here; but it may be advisable to remark briefly upon a theoretical point of some importance on which the author's views seem open to question; and there are also a few matters connected with the general subject which seem worthy of attention.

Although Professor Gastaldi, of Turin, after a careful study of the Italian Alps, has adopted Professor Ramsay's view of the excavation of alpine lake basins by ice, Sir Charles Lyell is still strongly opposed to that view. He maintains that they have been produced by changes of level in valleys, producing depressions which have been preserved during the glacial epoch by being filled with ice, while at all other times they were either soon filled by *débris*, or their lower barriers were cut down as fast as they were formed. He thus accounts for the fact that lakes only occur in any abundance in glaciated districts. He further maintains that the erosive power of glaciers, as indicated by the muddy torrent that always issues from them has been overrated, because "the flour of rock" thus produced is due, not solely to the wearing down of the floor of the valley, but, "to a considerable extent," to the grinding up of the stones which fall upon the glacier and are engulphed in its crevasses.

There are doubtless many difficulties in Prof. Ramsay's theory, and much remains to be done to verify it, but it does seem to cover a larger portion of the facts than that now opposed to it. There is no evidence before us to show how much of the glacier mud is respectively due to the two sources above referred to, but the enormous bulk of many of the old moraines, where they have not been destroyed by subsequent denudation, seems amply sufficient to account for the *débris* which falls upon a glacier; while the wide extent of glaciated surfaces, and the manner in which the very hardest upturned strata are often planed off or *moutonnées*, is equally convincing proof that large masses of rock have been ground down by glaciers. The evidence of this is very remarkable also, in the case of the Loess, a deposit which covers an enormous extent of country, and in some parts of the valley of the Rhine reaches a thickness of near 1,000 feet, and which Sir Charles Lyell himself considers to be undoubtedly glacial mud. It is difficult to conceive how such an enormous amount of mud could have been formed except by a grinding power capable of producing most of the effects imputed to it by Prof. Ramsay. It is considered to be one of the most powerful arguments against the ice-erosion theory that no lakes exist in certain valleys which were undoubtedly filled with enormous glaciers; but the answer to this is, that a lake will only be produced when the erosion is considerably greater at one part of the valley than at another, and this inequality may be caused either by unequal hardness of the subjacent rocks or by the piling up of the ice to a greater thickness in certain spots by the convergence of several branch glaciers, as must have been notably the case over the site of Lago Maggiore, which received the icy streams descending from near 100 miles of the loftiest Alps. It must also be remembered, that at such points of convergnce the rate of motion of the glacier will be much more rapid than elsewhere, in order to discharge the accumulated ice-streams; and we shall thus have a double cause of increased grinding in such positions. A difficulty of a somewhat similar nature, and which cannot be so easily overcome, besets the unequal-subsidence theory, which can hardly be made to account for the thousands and tens of thousands of lakes so thickly scattered over the lowlands of Northern Europe and America.

It is somewhat remarkable that notwithstanding the numerous researches in post-tertiary caves and gravels in all parts of Europe, no human remains have been discovered which can be proved to be older than those found by Dr. Schmerling more than forty years ago in the caverns near Liége. After many years' labour this gentleman, a skilful anatomist and palæontologist, published, in 1833, a detailed account of his researches, copiously illustrated. It is curious to see, from Sir Charles Lyell's account of this work, how completely its author anticipated all the more important results of modern cave exploration, and how thoroughly he had worked out that doctrine of the antiquity of man which the great majority of geologists so long attempted to put down. Such wholly independent researches as those of Schmerling in Belgium, McEnery in Devonshire, and Boucher de Perthes in France, made by careful and conscientious observers, and all converging to the demonstration of one fact, were for many long years laughed at or ignored, solely because they clashed with preconceived opinions. When this occurred with the students of a science which had already fought and won many hard battles against popular and theological prejudice, and whose whole course of study should have taught them how to interpret the evidence adduced, we are bound to deal tenderly with the less unjustifiable prejudices of those who have had no such training.

Notwithstanding the lesson these long-ignored facts should have taught them, some geologists still exhibit a strange fear or hesitation in facing the whole results of modern inquiries on the subject. How is it that, whenever any estimate is made of the lapse of time (expressed in years) since any human remains or works of art were deposited, the lowest possible estimate is almost always chosen? One would think that, having once got beyond the traditional six thousand years, the period of man's past existence would be a matter of purely scientific inquiry, to be arrived at by careful estimates in a variety of ways. But how can we possibly arrive at the truth by always taking the lowest estimate? we might just as reasonably always take the highest. Is there any merit in arriving at a false result so that the figures are small? Is it really the "safe" side so to calculate that we shall almost certainly be wrong? Astronomers do not think those observations most likely to be correct which give the smallest distances and sizes of the heavenly bodies and it would be more dignified and more scientific if geologists, whenever any data exist on which to found a calculation, should insist on taking the mean result of various impartial estimates as that most likely to be the true one. From this point of view it may be interesting to give a summary of the more important attempts which have yet been made to determine the antiquity of human remains or works of art.

From observations at the delta of the Tinière and on the lakes of Neufchatel and Bienne, the bronze age in Europe has been determined with approximate accuracy to have been from 3,000 to 4,000 years ago, and the stone age of the Swiss Lake dwellings at from 5,000 to 7,000 years and an indefinite anterior period. The burnt brick found 60 ft. deep in the Nile alluvium indicates an antiquity of about 20,000 years, taking, from a calculation by Mr. Horner, the estimate of $3\frac{1}{2}$ in. per century as the rate of deposit of the mud. Another fragment found at 72 ft. deep is estimated by M. Rosière to be 30,000 years old. Some human bones found in a lacustrine formation in Florida have been considered by Agassiz, after a careful examination of the locality, to be at least 10,000 years old. A human skeleton found at a depth of 16 ft. below four buried forests superposed upon each other, has been calculated by Dr. Dowler to have an antiquity of 50,000 years.

These latter estimates may be very uncertain, but we have no reason to think them improbable, from what we know of the great changes of physical geography that have undoubtedly taken place since man existed. Kent's Cavern at Torquay furnishes a good example of these, since the whole drainage of the surrounding country must have been very different when the great thickness of cave earth was deposited by floods rushing through the cavern which is now situated in an isolated hill. We have here indications of an immense antiquity from various sources. The upper stalagmitic floor itself marks a vast lapse of time, since it divides the relics of the last two or three thousand years from a deposit full of the bones of extinct mammalia, many of which, like the reindeer, mammoth, and glutton, indicate an arctic climate. It has been remarked that the varying thicknesses of the stalagmitic floor, from 16 in. to 5 ft. and upwards, closely correspond to the present amount of drip in various parts of the cave, so that the cave itself with its various fissures and crevices does not appear to have been materially altered since the stalagmite was deposited. It is true that the drip may once have been greater, but it may also have been less, and we do not know that a more copious drip would necessarily produce a more rapid deposit of stalagmite. But names cut into this stalagmite more than two centuries ago are still legible, showing that, in a spot where the drip is now very copious, and where the stalagmite is 12 ft. thick, not more than about one-eighth of an inch, or say one-hundredth of a foot, has been deposited in that length of time (British Association Report, 1869, p. 196). This gives a foot in 20,000 years, or 5 ft. in 100,000 years; and there is no reason whatever to consider this to be too high an estimate to account for the triple change of organic remains, of climate, and of physical geography. But below this again there is another and much older layer of stalagmite, generally broken up and imbedded in the cave earth. This older stalagmite is very thick and is much more crystalline than the upper one, so that it was probably formed at a slower rate. Yet below this again, in a solid breccia, very different from the cave earth, undoubted works of art have been found. A fair estimate will therefore give us, say, 100,000 years for the upper stalagmite, and about 250,000 for the deeper layer of much greater thickness, and of more crystalline texture. But between these we have a deposit of cave-earth which implies a different set of physical conditions and an alteration in the geography of the surrounding country. We have no means of measuring the period during which this continued to be formed, but it was probably very great; and there was certainly some great change in physical conditions during the deposit of the lower stalagmite, because the fauna of the county underwent a striking change in the interval. If we add 150,000 years for this period, we arrive at the sum of half a million as representing the years that have probably elapsed since flints of human workmanship were buried in the lowest deposits of Kent's Cavern. It may be objected that such an estimate is so loose and untrustworthy as to be altogether valueless; but it may be maintained, on the other hand, that such estimates, if sufficiently multiplied, are of great value, since they help us to form a definite idea of what kind of periods we are dealing with, and furnish us with a series of hypotheses to be corrected or supported by further observation, and will at last enable us to arrive at the antiquity of man within certain probable limits of error. Without laying stress on any portion of the above very rude estimate, it may, I think, be averred that it is not palpably too high, but is just as likely to be too low; and this last supposition will be rendered more probable when we consider the vast lapse of time implied by the position of some of the recently discoverd palæolithic weapons.

The flint tools found in the gravel at Bournemouth, in the Isle of Wight, and near Salisbury, at elevations of from 80 to 100 feet above the present valleys, imply, according to the best observers, that the whole series of surrounding river valleys have been excavated since they were deposited, and that the system of drainage and position of the coast-line have been very greatly altered. The hippopotamus of the Gower Caves implies changes equally great, since the peninsula of Gower now contains only small streams, and could not possibly have had a large river without very important changes in its relations to the adjacent country. The position of the flint weapons in the valley of the Somme, at Hoxne in Suffolk, and in many other places, all combine in indicating that very important changes in physical geography have taken place since they were deposited. We can hardly suppose that in all these different localities the changes were abnormally rapid, especially as in no case do records of the historic period indicate that any remnant of the process was then going on; and from what we do know of the rate of such changes, and their intermittent nature, we are entitled to affirm that the most extreme estimates yet made of the antiquity of the men who fashioned and used the palæolithic implements is quite as likely to be under as over the truth.

There is as yet no clear evidence that man lived in Northern Europe before the glacial epoch, and even if he did so the action of the ice sheet would probably have obliterated all records of his existence. Every evolutionist, however, now believes that he must have existed far back in the tertiary period, and that the proof of it will be found, if at all, in some of the warmer regions of the old world. Here is surely a problem of grand and absorbing interest awaiting solution at our hands. Geologists are not usually wanting in energy or enterprise, and they number in their ranks many wealthy men. It is to be hoped that they will soon energetically attack the problem; and no more promising field of research offers itself than the limestone caves of Borneo, which can be explored with perfect safety, and at a moderate expense. We can hardly now expect any great additions to our knowledge respecting the antiquity of man in Northern and Central Europe, and must go to warmer regions if we wish for new discoveries and startling revelations.

A. R. WALLACE

## LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

### Fellowship at Magdalen College

I THINK the notice in NATURE of Sept. 25 respecting the election about to take place to a Natural Science Fellowship at Magdalen College requires some comment. The amount of academic preferment which falls to the share of science in Oxford is so small, that it might reasonably be demanded that what there is should be thrown open to as many candidates as possible. When, therefore, it was announced that the Fellowship would be given for proficiency in Biology, it might have been inferred that the electors had this object in view. Biology is held, elsewhere than in Oxford, to be the science which treats of the laws governing organization and vital activity; in other words, structure and function in *all* forms of life, whether vegetable or animal. It was not, perhaps, an unreasonable inference, therefore, to draw from the terms of the notice, that it was the intention of the College to make Biology in its widest sense the foundation of the examination, and to allow individual candidates to exhibit, in addition, such detailed knowledge as they might possess of Zoology, Botany, or even Palæontology. This would not have attributed to Biology a wider meaning than, for example, Mr. Herbert Spencer or the Science and Art Department attach to it. Thinking it desirable, however, to get some official information upon the subject, I wrote to the President, who, after some delay, replied that, in his opinion, as preference would be given to Biology, it would be useless to offer Botany as a special subject. This is not more reasonable than it would be to say, that because Physics was to be the subject of an examination it would be useless to offer Electricity or Heat as a special subject. But the terms of the President's reply were rather ambiguous, and I therefore made some further inquiries. I learnt, as the result, that the College considered it impossible to compare the merits of a candidate who stood on the Zoological, with one who stood on the Botanical, side of the general subject.

I think myself the difficulty is not one which should have been found insuperable; but, assuming that the College had sufficient grounds for a different opinion, then I think the electors should not have offered their Fellowship for Biology, when what they really had in view appears to be a detailed knowledge of the Zoological preparations in the University Museum.

W. T. THISELTON DYER

### The Sphygmograph

THERE appears in NATURE, vol. viii. p. 330, a notice of a thesis for the M.D. Cantab. on the subject of Bright's disease, in which reference is especially made to some sphygmographic observations therein contained. It is apparently from the pen of Mr. Garrod, who is himself the author of interesting and important researches with the sphygmograph and cardiograph. While agreeing with a part of my explanation of the normal pulse tracing, as regards the points in which it differs from the view commonly received, he takes exception to the account which I have given of the tidal or first secondary wave. It may be well to say in reply a few words upon the point at issue, since the reference to it in the thesis was very brief and incidental, and I should not wish it to be taken as a full account of my views as to the mechanism of the pulse.

The explanation of Mr. Garrod himself is that the tidal wave is an instantaneous wave due to the closure of the aortic valves. This theory was first proposed by M. Marey to account for the tidal wave in many of its forms; but, so far as I know, it has not been adopted by any writer on the subject in England with the exception of Mr. Garrod. There is this difference, however, between them, that while M. Marey holds that the dicrotic wave has nothing to do with the aortic valves, but is a reflection from the periphery, Mr. Garrod considers that it is the wave of expansion from the closure of the aortic valves, which becomes separated from the instantaneous wave as it recedes from the heart. Thus the faculty of originating two waves of different velocity, which by most writers is attributed to the first impulse of the heart, combined with the closure of the mitral valve, is by Mr. Garrod denied to that event, but ascribed to the closure of the aortic valves. Now I believe it to be mechanically impossible for any wave to be propagated with a velocity different from that of the wave of expansion, except the purely vibratory wave of sound, and Mr. Garrod appears himself to hold that a mere vibration produces no elevation in the tracing. The question, however, may easily be determined experimentally. If there appear in the tracing two waves which are travelling with different velocities, their relative position will vary at different distances from the heart. Let, therefore, anyone who wishes to settle the question for himself take tracings of a good many