REPORT

OF THE

LECTURE

ON THE

FLINT-IMPLEMENT BEARING BEDS

OF

S. ACHEUL,

DELIVERED BEFORE THE ASHMOLEAN SOCIETY,

NOVEMBER 9, 1872,

BY

JAMES PARKER, F.G.S.

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ASHMOLEAN SOCIETY.

A meeting of the members and friends of this Society was held on Monday evening in the University Museum, Oxford, Professor Rolleston (president) in the chair.

E. CHAPMAN, Esq., secretary, read the minutes of the previous meeting, and the list of books presented to the society since that time, after which the names of some gentlemen were submitted to the society for election, and on a ballot being taken they were unanimously elected members.

Professor WESTWOOD said that, leaving the larger and more beautiful specimens, especially of entomological objects, which had come to hand since the last meeting, he should exhibit a few other things he had received. These consisted of collections of drawings of great value, representing the transformations of the lepidopterous insects of India, which he had received from some Indian officers; though rough, they were characteristic, which was wanted more than minuteness of detail. He also referred to a most remarkable specimen of an excessively old genus of lepidopterous insects, which had been found in a fossilised state at Stonesfield; it consisted of only a simple anterior wing, which was, however, in splendid preservation. The curiosity of the thing was that it belonged to a group which was exclusively, at the present day, South American, and which did not even extend to New Holland where some of the most abnormal South American forms of the lepidopteræ had occurred. He next exhibited a collection of gnats, which he considered to be the insects of which they heard every summer as being imported mosquitoes. Regularly from sunset till about ten minutes afterwards he had found them swarming his house, and at that time invariably killed every one he could find, so that he had been comparatively free from their attacks, though he had on his forehead at that moment a mark from a bite, which had been there six weeks. It was remarkable that, no matter how closely he fastened up the rooms, these insects always found their way into them, and also that, out of the thousands he had killed, not one had been a male. It was only the females which sucked, the mouth apparatus and antennæ of the males being so covered with beautiful plumes that it was impossible for them to suck.

In answer to the PRESIDENT,

Professor WESTWOOD said he did not think the Mediterraneanmosquitoehad been imported into England yet.

The PRESIDENT enquired of Professor Phillips whether lepidopterous insects had been found in any lower strata than the Stonesfield slate, because mammals had been found in earlier strata which, it seemed to him, might have fed on the lepidoptera.

Professor PHILLIPS replied that he had never seen

lepidopterous insects in strata lower than that from which the specimen before them was taken, and his opinion was that that was the lowest strata in which they could occur.

The President then called upon Mr. Parker for his lecture.

Mr. JAMES PARKER, after having referred to the many lectures and essays which had been written upon the flint implements found in the gravels of the Valley of the Somme, and especially to those papers which had appeared in the proceedings of the Royal and Geological Societies, and those of the Society of Antiquaries, said what he proposed to do then was to point out some of the links in the argument which he thought had not received the attention due to them in comparison with other details introduced into the chain of reasoning, as to the immense antiquity of the flint implements in question. He could not hope, indeed, he did not propose to attempt to explain the many and varied phenomena presented by the Somme Valley, or to fix the exact age of the beds bearing the flint implements, but he hoped at least to bring forward some considerations which had not been fairly discussed, and which, if founded upon fact, as his observations he trusted would show to be the case, militated considerably against the views which were commonly held, and of which Sir Charles Lyell was the chief exponent. He thought he would best consult the convenience of his audience by giving to them, in Sir Charles Lyell's own words, the chief points in his argument. His work was practically the summing up of what authors, both English and foreign, had then (in 1863) written, together with conclusions derived from his own personal observation. In his book a section of the Valley of the Somme was given, which Mr. Parker exhibited enlarged and coloured. He was sorry to say that as a matter of fact they could place no reliance upon it whatever, as it differed in many respects from the actual circumstances, but it was necessary to reproduce it there in order to illustrate Sir Charles Lyell's theories. Quoting from "The antiquity of man" (London, 1863), p. 106, he read:—

"The chalk hills which bound the valley are almost everywhere between 200 and 300 feet in height. On ascending to that elevation we find ourselves on an extensive table land, in which there are slight elevations and depressions." . . . . . .

At p. 107,—"Here and there are outlying patches of tertiary sand and clay (bed No. 5) with eocene fossils, the remnants of a formation, once more extensive, and which probably once spread in one continuous mass over the chalk, before the present system of valleys had began to be scooped out,— . . . and their denudation has contributed largely to furnish the materials of gravels in which the flint implements and bones of extinct animals are entombed." . . . . . .

At p. 108,—"The bed marked No. 2 indicates the lower level gravels, No. 3, the higher ones, or those rising to elevations of 80 or 100 feet above the level of the river. Newer than these is the peat, No. 1, which is from 10 to 30 feet in thickness, and which is not only of later date than the alluvium Nos. 2 and 3, but is also *posterior to the denudation of those gravels, or to the time when the valley was excavated through them.*"

"Underneath the peat is a bed of gravel from three to fourteen feet thick, which rests on undisturbed chalk. This gravel was probably formed, in part at least, when the valley was scooped out to its present depth, since which time no geological change has taken place except *the growth of the peat*, and certain oscillations in the general level of the country."

These were briefly the materials for the computation. So many years were ascribed to the peat deposit (this would be found to be computed at 33,000); so many in addition for the excavations which had taken place of the valley; and so many for the deposition of the gravels, marked respectively No. 2 and No. 3. Practically these operations could only be summarised as a whole, and it was only by an induction from a passage elsewhere in his book that they found he computed the time for these operations somewhere about 70,000 years. At the base, and intermingled with the lowest deposit, were the implements in question, several specimens of which were exhibited, and which there was but little doubt now remaining owed their definite shape for the most part to the hand of man.

Although not directly part of the subject before the meeting, he thought it well to say a few words about the 33,000 years of the peat, as it was an important item in the total, and it also afforded a typical instance of the *mode* in which arguments were forced into the service of the author.

He read (p. 110),—"The workmen who cut peat or dredge it from the bottom of swamps and ponds, declare that none of the hollows which they have found or caused by extracting peat have ever been refilled, even to a small extent. *They deny therefore that the peat grows.*"

Mr. Parker could only say that on asking a couple of men who were working at M. Tattegrain-Brulé's pit, and who had worked in the peat pit at other times, as to the depth, &c., of the peat, their account distinctly was that it *did* grow. He had not pressed the point at all, the only questions he asked were as to the total depth, and as to what was at the base of the peat. The men agreed that it rested on the chalk and was nowhere more than nine metres thick. M. Tattegrain Brulé corrected them so far as to say he knew of places where it was over thirty feet thick, and what was to the present purpose as regarded Sir Charles Lyell's statement, they said that the peat was still growing or forming, and that about a metre in a century was the rate, according to their idea. His own conclusion in 1861 was that this was possibly an average estimate, because when they were altering the moat surrounding Aberville he observed that there was a deposit of some two or three feet of peat in it, which they were clearing out, and he thought that they would at least have cleared their moat once in a century. This was not far from the Porte Mercadet, a place often referred to in the account of the discoveries.

He might mention the computation which was made for the growth of the peat in Ireland. This was according to Mr. Griffiths *two inches* in depth *in one year*, but this was an excessive gro th, and under peculiarly favourable circumstances. But before taking such data—the workmen's, which would give at a metre one thousand years for the whole 30 feet, and Mr. Griffiths' computation, which would, under favourable circumstances (and

in many places the Somme Valley presents these), leave it possible for the whole thirty feet to have been deposited since the commencement of Queen Anne's reign,—he thought it well to call attention to an important consideration which affected materially any computation derived from peat growth, namely the intermittent character of the growth—its rapid growth at one time, its slow growth at another, and entire stoppage at others. When the peat during growth reached the highest level at which water would stand in any given locality, it naturally ceased to grow. From its character it could not raise itself to any great degree above the element on which it mainly depended for its growth. Of course it might be in the varied incidents of a long valley that the stream for some cause was kept back, but that could not be for long. The weight of the water would eventually break a course through the obstruction, and then the peat formed at the highest level would sink by reason of evaporation and its own weight, and become more consolidated, and form distinct beds of varied densities such as existed in the peat, and which pointed to that intermittant character of growth. Consequently until they knew what periods of rest took place all computation was impossible, as the facts derived from the observation of incidental growth might have such a relation to the whole as to be not worth taking into account.

The lecturer's view then was that only in a very few cases was there any material growth of peat, such as when the water stood sufficiently above its surface as to supply the means of growth; and that then it was very rapid, the conditions being as favourable as those in Ireland, and it followed therefore that as the peat grew higher in the valley—higher that is to say in regard to the sea level—so fewer occasions would there be of the water lying at a sufficiently high level to induce growth, and from this the probabilities were that in the earlier history of the peat, the occasions being more frequent, the beds would increase as a whole far more rapidly than they they did now.

He next turned to Sir Charles Lyell's computation. This writer had selected the argument from M. Boucher de Perthes' evidence, and though he said "we must hesitate before adopting it," he gave it as the only one of any value, and did not intimate the least wherein any fallacy lay. It was given at p. 110.

"In one case, however, M. Boucher de Perthes observed several flat dishes of Roman pottery, lying in a horizontal position in the peat, the shape of which must have prevented them from sinking into or penetrating through the underlying peat. Allowing about fourteen centuries for the growth of the superincumbent vegetable matter, he calculated that the thickness gained in a hundred years would be no more than *three French centimetres*. This rate of increase (Sir Charles Lyell added) would demand *so many tens of thousands of years* for the formation of the entire thickness of thirty feet, that we must hesitate before adopting it as a chronometric scale."

It was obvious that 0·03 metres in a century required upwards of 33 thousand years to give the 10 metres, which in some places existed in the Somme Valley. The point he would lay stress upon was that the hesitation to accept this should not have been made to arise from the result which it gave, but from the fact that the data

were so obviously worthless for forming any calculation at all. The absolute but erroneous assumption that continual formation of peat went on at one uniform rate, was the basis of the whole argument. This pottery was found, so it was stated in M. Boucher de Perthes' book (Antiquité's Celtiques II. p. 135) to be 0·60 metres (nearly two feet) below the surface. This writer argued further that much of the peat being impure, the factor had to be reduced by one fourth, *i.e.*, to 0·45. Now Samian pottery, it was argued, must be 1,400 or 1,500 years old. It was assumed that (a) at that distance of time it was (b) placed gently on the surface of the turbary so as not to sink through, and (c) circumstances were such that it was not buoyed up, and (d) that the Peat from that moment down to 1863, had gradually, and at one uniform rate per annum, grown over it. Any one of the conditions of course being liable seriously to affect the factor, they were supposed to accept all, and thereby obtain a factor to apply generally to the growth of the peat throughout the Somme Valley. If this was not what was meant by Sir Charles Lyell's argument, nothing could be gleaned from it at all. The lecturer then proceeded to consider the next elements for the computation of the time which had elapsed since the deposition of the implement bearing beds. Without quoting new passages, the words already given shewed the line of argument, namely, "that the peat was posterior to the time when the valley was excavated through the gravels."

It was in vain to look for any figures of computation for such excavation, although elsewhere in Sir Charles Lyell's book (p. 322) it was intimated that the upper and earliest of these gravels were the equivalents probably of beds 100,000 years old, no arguments were forthcoming as to the means of computation. Indeed it seemed beyond all calculation. Imagine the rate at which a trickling stream could excavate and grow into a large one, and carry down the material of a valley one hundred and fifteen miles long, and varying from one mile to ten miles broad. Imagine the millions upon millions of tons of chalk and of other material to be scooped out and carried along and deposited in the sea. The time was certainly beyond all calculation, and the 67,000 years, he was sure, would be found by any one who considered the problem carefully, to represent but a mere unit in the time required under the circumstances.

But then the question forced itself on one. "Was the Somme Valley excavated by the Somme River at all?" Not one line would be found in evidence; it was assumed purely and absolutely, and on that assumption alone, was based all the arguments as to time, which were put forward.

In considering the hypothesis of the excavation of a valley of this kind hy means of a river, the first question to be asked was naturally, "Where did the water come from?" Considering the vast surface to be removed, it was necessary to have a supply of water of enormous quantity and of constant flow. And much more than that it was necessary to have an impetus given to that water by a fall or gathering together of streams to give it force sufficient to remove, and propel the loosened material forward in its downward progress to the sea.

Two minor considerations also might be mentioned which in a full investigation of the phenomena should not be overlooked, though the scope of the present argument would not allow of any further remarks upon them. First, a certain amount of slope of the bed of the valley from its highest point to its lowest must be necessary. Below a certain incline, water would not move forward to any extent large masses. Now, the bed of the Somme Valley was singularly level for a wide river, there being a fall of little more than 200 feet from the source of the Somme to the sea, a distance of 115 miles, in fact the fall was hardly above that of the Thames between Oxford and London, and the distance was the same. 2nd, there was the consideration of the difficulty of accounting for the disposal of the materials when they reached the river mouth. He had examined very carefully the district at the mouth of the Somme, and could say that they were not deposited there nor were there any signs of them. Nor yet was any *á priori* ground for arguing that the waves had washed the debris into their depths. The history of the coast was directly opposed to this, as the waves were throwing up sand-dunes, and had been so since the earliest times of which they had any record regarding that coast.

Mr. Parker then referred to a large diagram which he had prepared, and on which he had traced the main line of the Somme, with its several arteries—representing by broad lines of colour the several valleys converging into the main valley. The district represented on the diagram was about one hundred and forty miles from east to west, and about 60 miles from north to south. At the eastern end it would be observed that the Somme was simply a small stream, scarcely to be called a river in a strict sense. Of course it was in a way the river Somme, because they considered the source of a river to be the point of departure of the farthest of the numerous streams which go to make up that river, and in most cases it was little more than water trickling along a ditch from some spring. But the word river in its natural sense means the stream of water after many smaller streams had been combined together, and had contributed each one its quota to form the larger one. The history of nearly all rivers was this, and the Somme was no exception. It depended on the drainage of many sloping valleys converging into the main valley. At the upper part it was a brook, and it did not become a river properly so called till it had received the converging rivulets of many small valleys. Till then it was no river; it had no force whatever. It was necessary for the converging valleys to be there to supply the water; it was necessary for the valley to exist to supply the fall; so that when they were asked to accept that the River Somme made the Valley of the Somme, it seemed to him they were asked to believe that the river made the conditions by which itself was called into existence.

It was unreasonable on the other hand to imagine high hills, pouring forth a stream of water above S. Quentin. They could not have existed without so total a subversion of the levels of the country that there would be no need of calling in the aid of river action to account for valleys twice as great as the Somme Valley. But as a matter of fact geologically, such lofty hills could not have existed without leaving a trace behind them.

Looking at the great system of arteries shown in the diagram, the ground to the south east was *on an average* higher than that to the north west. There were here and there hills of the same height, or almost the same, along the whole line, and they were broken up by innumerable valleys and "combes"; but by taking the average from a considerable number it would be seen that there was a general slope, as regards the higher prominence in a north-westerly direction. The hill rising immediatly above the source of the Somme, five miles N.E. of S. Quentin and at a place called from the circumstance "Fon-somme," only reached 308 feet in height above the level of the sea, and the drainage of this alone supplied the upper tributary. Three miles to the south east was a hill reaching 400 feet, but it appeared to add little, if anything, to the supply. They would have to go several miles to obtain a higher level, and directly they reached it they found that it no longer supplied the Somme, but the Aisne and the Oise, which were tributaries of the Seine, and belonged to a distinct system. If they continued their search for still loftier elevations they would, still proceeding in a south easterly direction, find hills rising to 800 and 900 feet, but they gave off their streams to tributaries of the Meuse, and they would be obliged to follow their waters through Holland before they were discharged into the sea. In a word, the whole system depended upon the water-sheds of the hills rising only to 300 and 400 feet above the level of the sea. The Somme depended mainly for its water upon the combined supplies of its chief tributaries, the Avre, the Noye, and the Celle, but all along its course it was assisted by numerous smaller streams gathering the rain water which fell upon the slopes of the numerous ravines descending into the main valley.

But connected with the Somme system, it was pointed out that there were several parallel rivers following the same course as the Somme, *i.e.*, descending from the south-eastern ridge in a north-westerly direction. To the north the Authie, and to the south the Bresle, the Yeres, the Eaulne, and the Bethune. They depended also upon the same sources of water, and were in every respect similar in their circumstances, and could scarcely have been different in their origin. If it were objected that springs now no longer in existence might have originally supplied a much larger body and a much greater force of water than now, it must be remembered that the district was a chalk district. Each ravine was as a rule dependant upon the rainfall of its own slope. All that could be done, therefore, was to increase the rainfall, and add what perhaps there might be independent reason for adding, heavy snowfalls, and of long duration by which the April suns provided an amount of water, far in excess of what was thrown down the ravine now. And yet that would affect the argument but little, because the sloping ravines converging to the great general valley must have been already there before the excessive rainfall or snowfall could be of any value. The sudden melting of snows on large flat expanses produced no material results; it was the valley, the ravine, and the gully which gave the force to the water, and without them the water but evaporated into the atmosphere or soaked away as best it might.

It was not a part of his task that evening, the Lecturer said, to explain the phenomena of the Somme valley, but with that map before him he felt called on to say a few words as to the operations which he thought it suggested. He might add that the veiw he took was based not only on the data then before them but upon the study of the levels of the Ordnance Survey in a much more minute degree than was represented by the figures on his diagram, and beyond this by many a tramp over the hills in question, some times in geological excursions, more often archæological. The great parallel lines of rivers, the furrows as it were stretching in a direction similar to that of the sloping chalk, suggested that the river valleys belonged to the operations consequent on the upheaval of the great mass of chalk from its ocean bed. He compared the result with what any one might see on any argillaceous shore, where the base was impervious and yet soft. The descending tide left channels and furrows, by which the surface was drained, but afterwards modified in character by evaporation and exposure to atmospheric influence. The great chalk expanse of a hundred miles was enormous in comparison to the few yards of a tidal shore, and so were the valleys of a hundred and two hundred feet depth to the little drifts of two or three inches. But this was not all; If it were argued that the effect was not proportionately sufficient, it might also be reasonably replied that the emergence of this vast chalk bed from the ocean was probably not of that passive character which belonged to a tide receding from the shore, but it might well have been the result of active elevation of the chalk, and such elevation could scarcely have been unaccompanied by fissures and inequalities which, as a rule, would lie as regards their greater intensity in lines at right angles to the main axis of elevation. That was just what those valleys did, and the minor fissures represented by the smaller ravines lay again in a general sense at right angles to them, as might be seen by a glance at the ordnance map before them, on which the valleys were slightly tinted. The general aspect of the Somme valley and its tributary ravines, pointed distinctly to operations connected with the rising from the ocean bed. Whether that took place in tertiary or post-tertiary times, whether once or more than once, were not questions with which he had now to deal. All he would lay stress on w s that those rivers and valleys, and among them the Somme River and Somme Valley did not owe their origin to the slow excavation of River action, and therefore the assumption of that action as a measure of time in connection with phenomena which the valley presented, was an absolute error.

He next passed on to the consideration of the deposition of the gravels. Practically the two arguments were based upon the same premise. The current of the Somme excavated the valley, and in doing so deposited the upper level gravel. It afterwards excavated the upper level gravel and deposited the lower level gravel. It afterwards excavated that gravel and the 33,000 years of the peat formation set in. At least this was what was meant, the lecturer contended, if there was any meaning in Sir Charles Lyell's argument at all. It was difficult to quote one single passage stating this. At p. 125 there was a good deal about beds 1, 2, and 3, but it would be found that the reference was to another set of beds in another

section and in reverse order. And yet the descriptions were intended to be a continuation of the same argument. Again at page 132 in referring to the first section for comparison with the Menchecourt beds; he spoke of No. 2 as the lower level gravel, and No. 3 as higher alluvium, but at p. 121 the low level beds at Menchecourt were spoken of as the older alluvium. He could not but think that if a clearer explanation of the phenomena had been given the fallacies involved in the conclusions would have presented themselves to the mind of the readers if not to that of the compiler of the book.

Before quitting this part of the subject the lecturer referred to the passage at page 141, where it was said there were, "patches of drift at heights intermediate between the higher and lower gravel, and also some deposits shewing that the river once flowed at elevations above as well as below the level of the platform of S. Acheul." He pointed out how practically the line of demarcation between high and low level gravels did not exist in fact, and that the argument therefore in regard to age derived from this difference of level was wholly untenable.

Having treated of the general aspect of the Somme valley as regarded the evidence for the antiquity of the implement bearing beds, he gave an account of the position of the beds in a particular district, namely that of S. Acheul, about $1\frac{1}{2}$ mile east of Amiens, a district which had yielded more of the flint implements in a small space than any other.

The plan exhibited some ten or twelve pits or cuttings in a space of about one mile from east to west, and three-quarters of a mile north to south. The levels of the surface of pits were marked, and a series of coloured sections of the sides of the pits, &c., drawn to scale. From these it appeared that while there was a gentle slope of the surface of the ground towards the south, there was a very rapid descent of the underlying chalk in a particular part, and in this hollow there had been the accumulation which contained the flint implements. The actual section presented a "combe" in the chalk filled up nearly to the level of the sides with gravels and sands, not stratified horizontally, which would have been the case had they been the result of deposit in a wide expanse of river, nor following any line suggested by possible current action.

He pointed out also in several instances that in a general sense the gravels were dependant on the chalk contours, but presented also the kind of inequalities which would arise from subaërial action. The surface materials seemed to have fallen, slipped, or drifted into lower levels, and arranged themselves partly according to their relative gravities, partly as said before, according to the ground on which they fell or over which they passed. And finally the varied action of wind drifting the surface sand and loam, of rain washing and separating lighter materials, and the possibly far more effective action of the melting snows, in loosening, shifting, and undermining the previously formed gravel—all those causes, coupled with the fact that they were no doubt intermittant and acting only at perhaps long and irregular intervals, were necessary to be taken into account in understanding the various phenomena which were seen in the details of the sections. Neither then in the consideration of the general phenomena nor in the minute

details were there any circumstances which suggested river action; on the contrary they militated against it and suggested subaërial action. But this being so, the very basis of Sir Charles Lyell's computation of enormous time was cut away. It was made to depend upon the slow action of the river cutting through an enormous chalk plateau, and carrying down to the sea millions of tons of chalk and other material, and all this before a peat formation commenced, which took 33,000 years. It was not his object to argue how long those beds might have been in formation under subaërial action, or how short a time was sufficient, the many accidents arising from the combination of the varied circumstances already detailed rendered all argument as to measure of time very uncertain, but what his object had been was to show that the computation put forth by Sir Charles Lyell, and followed by so many others, was based upon utterly false premises.

Mr. Parker, before concluding, drew attention to a large collection of flint implements derived from the S. Acheul beds, chiefly from his own cabinet, but supplemented by others, by S. Sharp, Esq., F.G.S. Also implements from other places, and from Bone Caves, Turbaries, British burial mounds, &c., &c., for the sake of comparison.

He pointed out that if rudeness was a criterion of immense antiquity, several of those from the British graves at Brighthampton, near Oxford, found with characteristic British pottery must be put long anterior in date to the S. Acheul implements, which were of a more developed type in fact the very perfection of the S. Acheul implements, while it told on the one hand with overwhelming force in favour of their being the work of man at the same time militated against the enormous antiquity ascribed to them unless we imagined man to have been wholly stationary, if not even retrogressive in the art of fabrication of his necessary implements of domestic and aggressive life.

The PRESIDENT said that, as every part of the world was now shown to have had a flint period, it bore on the interesting anthropological question whether man rose from a savage state, or whether the present savage was a degradation from a higher state.

An interesting discussion then arose as to the age of the flint implements (a large collection of which were shown by Mr. Sharp and Mr. Parker), and the question whether such valleys as that of the Somme were formed by a pressure from beneath, or by the water courses which now ran in them, in which the President, Professor Phillips, Mr. Parker, and Mr. Sharp took part, after which,

The PRESIDENT proposed a vote of thanks to Mr. Parker for his valuable contribution to their information on the interesting subject they had had before them, which was cordially agreed to, and the proceedings terminated.