The natural fracture of flint and its bearing upon rudimentary flint implements / by J. Reid Moir. The occurence of a human skeleton in a glacial deposit at Ipswich / by J. Reid Moir. Description of the Ipswich skeleton / by Arthur Keith.

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13.

THE OCCURRENCE OF A HUMAN SKELETON IN A GLACIAL DEPOSIT AT IPSWICH.

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DESCRIPTION OF THE IPSWICH SKELETON.

By Arthur Keith, M.D., LL.D.

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THE NATURAL FRACTURE OF FLINT AND ITS BEARING UPON RUDIMENTARY FLINT IMPLEMENTS.

By J. Reid Moir.

Read at Norwich, December 4th, 1911.

The determination of human flaking as against Nature's work upon flints has always been a vexed question with archæologists. It appeared apparently in the first place with the discovery of the Neolithic arrow-heads, as in some old books we find these relics described as "elf-darts," which clearly proves that a supernatural origin was accorded to them. Though unfortunately we have no records of the discussions held over these first discovered Neoliths, we may I think assume that in those days the archæological camp was split into two sharply defined sections, one declaring that the arrow-heads were the outcome of supernatural agencies, the other that some ancient race of men had fashioned them. Be that as it may, the fact remains that no such discussions have for many long years raged round these particular flaked flints, and they are now accepted as being as much evidences of man's work as is a dining-room table or a fireside arm-chair.

But the state of quiescence which had for such a long time settled over pre-history was rudely and completely disturbed by the assertion of that famous Frenchman, Boucher de Perthes, that he had found definite flint implements in the undisturbed gravels of the River Somme associated with the bones of extinct animals, and that it was abundantly evident that some representative of man was living in the remote period when these animals existed. Immediately this statement was made known to the world the archæologists were once more plunged into a most bitter controversy regarding the flaked flints discovered, and chaos reigned supreme. The majority of authorities made haste to condemn Boucher de Perthes' discovery as being of no value, as in the first place the flakings on the flints had almost certainly been produced by natural causes, and, if not, then the workmen at the pits had made them themselves, and surreptitiously inserted them in the gravel, where they were dug out by the unsuspecting but misled archæologist. Lastly the great French biologist, Cuvier, stated that he knew of no evidence which proved that any human remains were contemporaneous with the Pleistocene mammalia.

There were, however, a few men who having seen some of Boucher de Perthes' implements, gave it as their opinion that these stones had undoubtedly been fashioned by man, and that they were of the same age as the mammalian bones found in the gravel with them. But these opinions made no headway, and for a long period, I believe twenty-five years, the others held the field, the only advance noticeable from the earlier dispute about the Neoliths being that the supernatural agency idea had disappeared. Gradually however—very gradually you will admit

—opinions began to change; investigations in river gravels in other countries were prosecuted, the actual bones of the men who made the implements were discovered, and at last, after much searching of heart on the part of the archæological world, Palæolithic man came into his own.

With the general acceptance of the Palæoliths, comparative peace once more settled over the question of Early Man, and though there were some who held that a well-flaked Palæolithic implement could not very well be man's first effort in flint chipping, and that consequently his earlier efforts ought to be found in deposits older than the Palæolithic gravels, yet these were but "voices crying in the wilderness," and little heed was paid to them. The first mutterings of the coming storm, which was to sweep away the limitations placed round the antiquity of man, were heard from Ightham in Kent, where Mr. Benjamin Harrison, having found Palæolithic implements in the highest river-gravels of his district, began to search those higher and older deposits which are situated up on the plateau of Kent. It is useless for me to describe here the tremendous and prolonged battle which raged over these Kentian Eoliths as they were called, or the admirable manner in which Mr. Harrison took all the criticism with which he was assailed. It is sufficient to say that for the third time the archæological world was greviously disturbed and the usual division of forces took place.

Those who supported Mr. Harrison asserted that his flaked specimens were undoubtedly man's work and exactly the type of implements which would be expected to be met with in such an ancient deposit; while the others contended that they saw nothing approaching man's work about the stones, and that the flaking was undoubtedly due to natural forces, such as earth-creep, pressure, natural percussion, the foundering of drifts and so on. But as time went on and discoveries of exactly similar flaked stones were made in contemporaneous deposits on the Continent and elsewhere, the consensus of opinion gradually swung round in favor of the "humanity" of the Kentian Eoliths, till to-day it is only a few who hold the opposite view. These Eolithic implements are certainly very rough and primitive in appearance, and as, till their discovery, the somewhat elaborately flaked palæoliths were considered to be the oldest relics of man's work, it is not to be wondered at that the immense gap which obviously separated the two cultures, was the cause of their non-acceptance by many archæologists. What was needed was an intermediate form which would bridge over the gap between the earliest and the later cultures, and when, in 1909, I found human implements below the Red Crag I saw on examination that I had discovered the type of implement for which we had been looking.

I would like it to be understood that in this paper I am not attempting a defence of these pre-crag pieces, as their form and flaking is so obviously "human," but I am anxious to put forward my views regarding the human origin of those rudimentary edge-

chipped stones which are looked upon as man's first efforts in flint chipping. We may, I think, take it, that the opponents of these particular flaked stones base their case on the effects of natural percussion and pressure, and I propose therefore to discuss these two effects in detail.

Before doing so, however, it will be interesting and instructive to review the evolution of the mental processes of the opponents of Early Man since the discovery so many years ago of the Neolithic arrow-heads. These you will remember were put down to supernatural agencies and described as "elf-darts"; then with the discovery of the palæoliths an advance is observable as the supernatural position is abandoned and deception and vague natural forces take its place. With the finding of the Harrisonian Eoliths, these natural forces are for the first time enumerated, and lastly we have the supernatural agencies, deception and vague supercession of forces, and two specific forces, pressure and percussion remaining.

The ground having been thus cleared we are able at last to test by experiment whether by fortuitous blows or pressure we can produce flaking on a flint which is indistinguishable from man's work.

I would point out that the conclusions put forward in this paper are the result of *practical experiments*. Theoretical papers dealing with the manner in which flints *might*, under abnormal circumstances, get flaked by natural means, appear to me to be a complete waste of time to write and an annoyance to read.

Some six months ago when turning this matter over in my mind, it occurred to me that those of us who are continually handling implements, recognise something about them which tells us they have been flaked by man, but that if we were asked to give our reasons for this belief we should have difficulty in doing so with sufficient force to convince a sceptic. I therefore concluded that it was largely a matter of instinct or subconscious recognition, and that my task was to discover what exactly it was about these flaked flints which differentiated them to us from others which we would not accept as "human." My next move was to find out some means by which I could fortuitously flake flints by percussion, and by comparing them with the specimens we looked upon as man's work, try to discover where the difference lay between the two.

I decided that if I got a sack and put in it some seven or eight flint nodules I could, by violently shaking the sack about, cause the stones to strike one another fortuitously and thus produce flaking on some parts of their surfaces. This idea was soon put into practice, and after shaking my sack and its contents about for some little time, I emptied the flints out, and was very delighted to see that one of them showed signs of distinct flaking on one edge. This stone was at once very carefully examined and compared with others in my collection, which I concluded had been humanly flaked, and also with one which I had flaked

myself. This examination and comparison soon showed me that there *was* a different appearance between the stone from the sack and the others, and that primarily this difference was the angles at which the various flakes had been removed.

This difference in the direction of the flakes removed, had, I have heard, been discovered some time before by Mr. W. J. Lewis Abbott, of Hastings. I, however, had heard nothing of his discovery when doing my experiments, and it is very interesting that we should have both arrived at the same conclusion independently of each other.

The sack specimen had obviously had its flakes removed at varying angles, as in Plate XXII., Figure 1, the arrows indicating the direction of the blows. This is exactly what one would expect from a series of *fortuitous* blows, which would naturally impinge at all angles.

The other specimens had obviously had their flakes removed along the edges at a *constant angle*, which is exactly what one would expect from a series of intentional blows, and which is inevitably the case when putting edge-flaking on a flint oneself. To detach flakes at varying angles would be a most troublesome and useless procedure.

Having once found a profitable line of study like this, I quickly followed it up and made a series of outlines of various flints from my collection, which in every case showed that the blows on their edges had been delivered at a constant angle—as in Plate XXII., Figure 2.

I also continued my sack experiment, and through the kindness of Miss Nina Layard, was enabled to examine a number of edge-flaked flints from Lowestoft beach, where Nature had been at work, *and in every single instance I found that the flakes on these stones had been removed at varying angles, as shewn in Figure 1.*

Now the argument of the "Natural Forces" men has been that a stone in its progress down the course of a swift-running stream would be struck by other stones, and edge flaking result, which would be indistinguishable from man's work. Well, even supposing we admit that a stone may get edge-flaked by stream action, I do not think that the most enthusiastic backer of Nature would or could believe that she would remove all the flakes at a *constant angle* along the edge. By various experiments I have proved that the great majority of fortuitous blows impinge directly on the edge itself, and in addition to shattering it, remove short flakes quite different from those removed by human blows, which are delivered on *the side* of the edge. I have been able to exactly imitate the Lowestoft beach specimens by selecting a suitable stone and striking it directly on the edge with another. I, therefore, have confidence in stating that we have here one criterion for differentiating Man's work and Nature's.

The method I adopt to ascertain the direction at which any given flake has been removed is to examine it carefully with a high-power lens and look for two things, viz., ripple-marks and those small fissures in the flint, both of which result from a blow. Having located the former, I get a pencil and draw a line down the centre of the flake, which is at right angles to the ripple marks, and then I draw two other lines, one on each side of the central one, and these also are at right angles to the ripple marks as they curve up on each side, as in Plate XXII., Figure 3. Having drawn these lines, I continue them out till they meet, which point is where the blow was struck which detached the flake. The small fissures in the flint are even more certain a guide, as a long experience of flaking flints has shewn me that these always point to the place where the blow impinges, as in Plate XXII., Figure 4.

These little fissures are very interesting, as while they are nearly always prominently visible on a surface which has been flaked by a blow, they are hardly ever present on one produced by pressure, and if they are visible, are badly formed and incomplete, like a pressure bulb.

It has often been stated that percussion and pressure remove exactly similar flakes from flints. With this I entirely disagree and can prove that it *is not* so. Of course when the whole of the bulbar cavity is visible on a flake, then these methods are not needed, but if the flake has been truncated in the making of the edge, then they become of great importance and tell us as certainly as if we had seen the bulb itself in what direction the blow was delivered.

The next thing I noticed on these edge-flaked stones from my sack was that they all showed *where shattering blows had impinged on the edge itself*, and this again is what must happen with fortuitous flaking. These shattering blows appear, and no doubt are, different from the abrasion of the edge of an implement by human use.

On the other hand, however, one cannot conceive of any prehistoric man being so regardless of his own interests as deliberately to strike and consequently blunt the edge which he was trying to make sharp, and so here again I assert we have another criterion of man's work upon a flint—as those specimens which I flake myself show no signs of blows which have impinged on the edge itself. But one of the most interesting specimens which I got by my sack experiments was one which showed a most definite sinuous edge for a distance of 4½ ins. along one side, and I would draw most careful attention to this specimen and the conclusions to be drawn from it, as they appear to me to be of very great importance. Now all of you are no doubt familiar with this sinuous edge on implements (produced by alternately detaching a flake from the two sides) and of the great stress laid upon it as being conclusive evidence of man's work. Well, you can understand how intensely surprised I was to find such an

PLATE XXII.

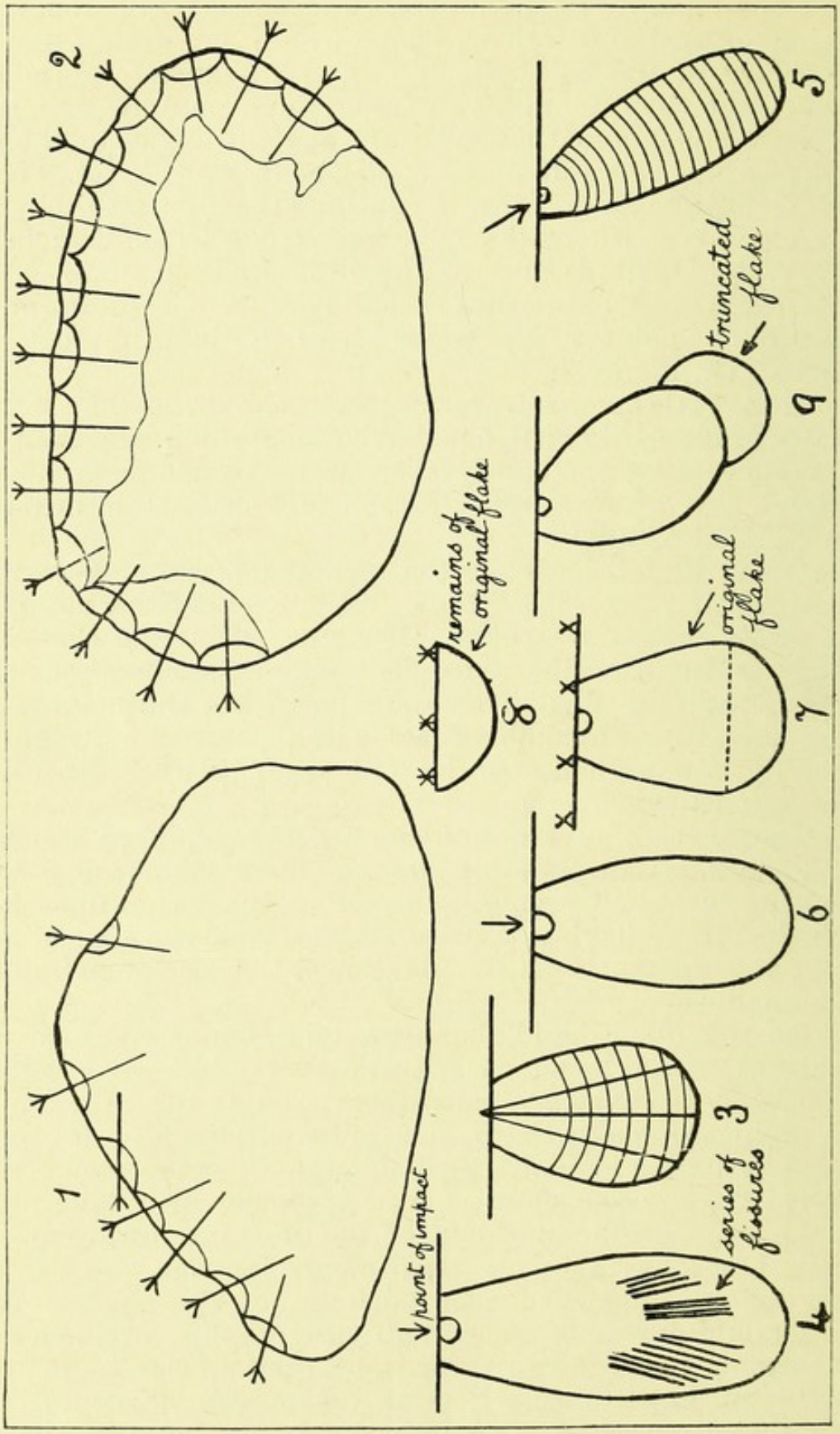
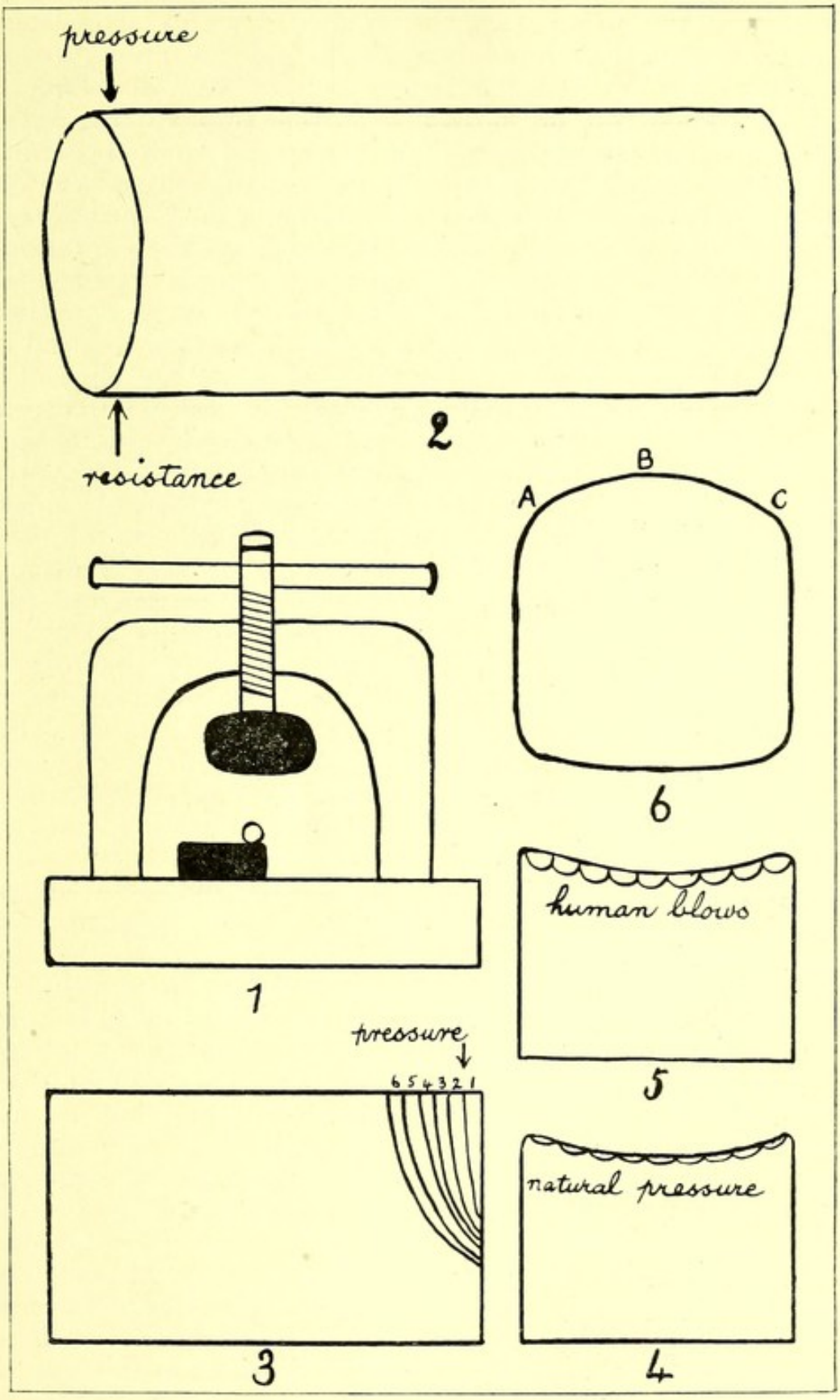

1
2
point of impact
series of fissures
3
4
remains of original flake
8
original flake
7
truncated flake
9
5
6

PLATE XXIII.


pressure
resistance
2
A
B
C
6
1
human blows
5
pressure
6 5 4 3 2 1
3
natural pressure
4

edge produced by fortuitous blows, and how at first I was inclined to believe that nature had in this instance exactly simulated human flaking. On examining this specimen however I first noticed that the flakes had been removed as usual at varying angles, and also that some blows had impinged upon the edge itself, but I also noticed that nearly every flake which had been detached showed on its surface *prominent* ripple marks. These puzzled me a great deal, and I set to work to produce a sinuous edge myself which would correspond to that upon the sack specimen, but found I was incapable of doing it. My next effort was to detach a single flake which would show these prominent ripple marks, and at last after many attempts I was successful. I will not recount in detail the process of reasoning by which I arrived at this result, but it is sufficient to state that I found that to detach such a flake it was necessary to strike the edge of the flint an *oblique* blow, as in Plate XXII., Fig. 5. I also found that it was extremely difficult to detach such a flake, as sometimes I had to deliver as many as seven or eight blows before the flake came away. Further experiments showed me that as the obliquity of the blows increased or decreased, so the visibility of the ripple marks increased and decreased. After this discovery I began to examine implements of various ages in my collection, and found that it was the rarest thing for any of them to show flakes with prominent ripple marks, and that the majority of them did not show them at all, *i.e.*, to the naked eye. I also examined very carefully some palæoliths in my collection with a most definite sinuous edge, and found that the flakes had first of all been removed at a constant angle, that there were no signs of blows having impinged actually on their edges, and lastly, that the flakes did not show prominent ripple marks.

I then set to work to imitate the flaking on these accepted specimens, and found that this was easily done by *vertical* blows. (Plate XXII., Fig. 6.) Now, having had practical experience of the difficulty of detaching oblique flakes and the ease with which vertical ones were detached, I reasoned that it would indeed be a strange prehistoric man who would make an edge with oblique, when he could with infinitely less trouble produce a better with vertical blows (and I have no hesitation in stating that no man, past or present, could produce such an edge as is shown in my sack specimen if he tried), and I accordingly decided that here again was another criterion for man's work on any given stone. This opinion was confirmed by the examination of other stones from my sack, and of some which I had flaked by throwing down on to some others. *These in every case had their flakes removed by oblique blows*, and I began to wonder why it was that nature should always flake in this manner. The answer to this question was completely and finally given me by a friend who pointed out that there were 180 angles at which a flake could be removed from a flint, but that only a few of these are the angles that would remove a true vertical flake. *This of course means that Nature has*

many more chances of removing an oblique flake than a vertical one, and needless to say she so removes them. I may say that I have corroborated this statement by flaking flints of *all sorts*, and have always got a uniform result. This does away with any argument that the ripple marks are due to the structure of particular flints or to their lines of colour.

I am unable to say exactly why an oblique blow produces a flake with prominent ripple marks, and must leave that problem to the mathematicians, but I assert it always does so. It must be remembered, however, that to get this result the blow must be delivered on a straight edge as in Fig 5. Now this stone with the sinuous edge, which however has been shown to have no real similarity to man's work, was taken out of my sack at the psychological moment so to speak, that is when the fortuitous blows had, up to the moment of taking it out, produced such an edge. But if I had not taken it out, and had continued the process long enough, the stone would have finally been reduced to a shapeless lump, having about it no form or symmetry. Also I may say that the stone itself, before any flaking began, lent itself, by its peculiar shape, to flaking along one edge, and that unless I put a certain shape of stone into my sack no flaking results. After having noticed the varying angles at which the flakes on this particular specimen had been removed, the number of blows which had actually impinged on the edge itself, finally the most important fact that all the flakes had been detached by oblique blows, I discovered that this edge showed no less than 17 *truncated* flakes. These truncated flakes are of great interest and importance, and I will explain what they are. When one starts to make an implement from a nodule of flint some big flakes are first removed which, in the process of fabrication, become truncated or cut off by the later flaking. In Plate XXII., Fig. 7, the original flake is shown with the edge from which it was struck indicated by crosses. The dotted line across the lower half of the flake shows where the edge has been flaked down to in the process of making the implement. This edge is indicated in Figure 8 by asterisks, and the remains of the original flake are also shown—this original flake has obviously then been reduced in size and is called a truncated flake. These truncated flakes are often looked upon by the uninitiated as being of thermal origin, as sometimes they are so much truncated as to show very little of their original surface, and consequently it requires a most careful examination to recognise that they are conchoidal. (Plate XXII., Fig. 9.)

I have often had a flake pointed out to me which, by its extraordinary shape, has been looked upon as non-human, but in nearly every case its humanity can be proved. Now, my sack specimen with the sinuous edge, which is only $4\frac{1}{2}$ inches in length, showed no less than 17 of these truncated flakes, which is what one would expect when it is remembered that in my experiment the rain of blows is practically continuous, and

consequently the edge is continually being reflaked and truncated flakes formed. On the other hand, with Neolithic and Palæolithic implements with sinuous edges, I find that the average number of truncated flakes on an edge 7 inches long is six. This again is what one would expect from *intentional* blows, but it is quite inconceivable that any prehistoric worker of flint would be so inept at flaking as to be compelled to truncate 17 flakes in the making of a 4½ ins. edge. I therefore look upon this as another criterion of Man's work upon any given stone, and before passing on to describe my experiments with flints under pressure, will recapitulate the various conclusions arrived at by my sack experiment.

In the first place we saw that fortuitous edge flaking on a flint can be distinguished from Man's work by the fact, that with the former the flakes have been removed at varying angles, while with the latter they have always been removed at a constant angle. Secondly with fortuitous flaking, shattering blows have impinged upon the edge itself and consequently blunted it. This particular condition is never found in association with human implements, as no man would intentionally blunt an edge which he was endeavouring to make sharp. Thirdly, that Nature in flaking a stone does so with oblique blows because she has many more chances of so doing, but that as it is extremely difficult to detach an oblique flake with an ordinary human blow it is inconceivable that any man, pre-historic or otherwise, would ever attempt to make an edge with such blows, when he could do so with infinitely more ease and a much better result, by vertical blows. Lastly, that fortuitous flaking is always accompanied by a large proportion of truncated flakes, and that it is impossible to believe that any ancient flint worker would ever be so unskilful as to truncate many flakes in putting an edge on a stone.

Having thus, I hope, put some interesting facts forward regarding the fracture of flint by natural percussion, I will now proceed to describe some of my experiments with natural pressure. This question was brought prominently before my notice by the important article written by the distinguished French prehistorian, M. L'Abbé Breuil, in "L'Anthropologie" last Autumn, in which he states that he found many flaked flints exactly similar to Man's work in the Eocene strata at Belle Assise, which had one and all been produced by natural pressure in the beds in which they are now found. Accompanying his paper is a drawing of the section at Belle Assise, and it is at once apparent that these flaked flints lie under a total depth of 33 ft. of superincumbent strata and that the 15 ft. which directly overlies the flint bed, is composed of fine stoneless Thanet sand. This paper, which attracted much attention amongst archæologists, gave a great impetus to those who are opposed to the belief that certain edge-chipped flints are of human origin, and as I had not till then experimented with flints under pressure, I thought it would be

interesting and instructive to do so. I accordingly purchased an old letter-press, and, having had it adapted to my purpose, I began my experiments.

My first effort was to ascertain whether pressure was distributed *vertically* through sand, because I had heard of certain experiments which apparently proved that it was not so distributed. To put this idea to the test I bought a cast-iron dish which was large enough to allow of the ram of the press to go into it, and put in it first of all one or two thin flakes of flint, so arranged that if even a moderate pressure came on them they would be immediately fractured. I then covered the flints with 2½ ins. of fine dry sand and brought steady and increasing pressure to bear upon them. *When I took the flints out I found that none of them was fractured in the slightest*, but it became evident that the pressure was not being distributed vertically through the 2½ ins. of sand. I continued the experiment many times with the flints arranged differently, *but never succeeded in breaking them.* I did succeed, however, in bursting the cast-iron dish, which conclusively proved that a great deal of the pressure was being distributed *horizontally* and not vertically. I also found that when using a differential screw press, with which we got a very great pressure, that fine flakes of flint protected by only 3 ins. of sand were unaffected by the pressure. The flints were subjected to pressure under dry and wet sand, but always with the same result. *These experiments prove beyond any doubt that pressure is not distributed vertically through sand, but is distributed largely horizontally by such a medium.* This experiment was supplemented by another in which we placed a number of thin and easily fractured flakes of flint on the floor of the press. We did not use the dish in this instance, but simply spread out about ¼ of an inch of fine sand on which the flakes rested and covered them with about ½ an inch of sand. As the pressure was applied, the sand, having nothing to hold it in, spread out on each side till there was not more than ¼ of an inch protecting the flints. This was sufficient however to prevent them being broken by the pressure. I performed this experiment because some of my friends had told me that the conditions in my dish do not occur in Nature, and that if the sand was allowed to flow under the pressure the flints would be broken. Such however was not the case. My next experiment was to put a number of flints in the dish without putting any sand above them and subject them to pressure. We found that a medium pressure shattered them a little, while greater pressure broke them up completely, *but in no single instance did we get anything which for a moment would ever be looked upon as being analogous to man's work.* I also imitated gravel conditions by placing a number of flints in the dish mixed with sand, but was equally unsuccessful. Having proved that 2½ ins. of sand are sufficient to protect flints from breaking when subjected to a great pressure, I proceeded to try to detach flakes from flints by pressure in a different manner.

I selected a suitable stone for flaking, and having put it on the floor of my press, placed another smaller stone on the top of it near the edge, so that when the pressure was applied a flake would be detached from the underlying stone. I arranged the stones as shown in Plate XXIII., Fig. 1.

By this method I found I could detach flakes easily, and was surprised and somewhat perplexed to find that they exhibited excellent bulbs and éraillures. A careful examination of these flakes, however, showed that they exhibited *two bulbs*, one at each end of the flake, which was very interesting, and immediately differentiated them from flakes produced by blows, which could not possibly show a bulb at each end. After a little study, I discovered the reason of these flakes showing two bulbs, the reason being that when the pressure was brought to bear on the stones it was equalled by the resistance offered by the floor of the press as shown in Plate XXIII., Fig. 2. As the pressure increased so the resistance increased, till the flint could stand the strain no longer, and a flake was detached which naturally showed a bulb at both points where the pressure and resistance acted. These two bulbs are generally unequal in size, and the side of the stone *which rested on the floor of the press* always showed the more prominent bulb, though why this should be so I cannot explain. Here again I assert we have a useful criterion for determining whether a flake has been detached by percussion or pressure, as on the former only one bulb is visible, while on the other, two are to be seen. Of course it is possible to detach flakes by pressure which do not show two bulbs, but when this is so the bulb is always quite different from a normal percussion bulb and can easily be distinguished from it, as they are only what can be described as pseudo-bulbs and cannot be imitated by a blow.

Another very interesting fact I discovered with my pressure work was that the flakes removed are often very thin and occur in a series, as in Plate XXIII., Fig. 3.

I have sometimes taken a flint from my press which had as many as 10 of these thin flakes separated, but not actually detached, from the parent block; they can, however, be detached with a pen-knife. I account for them by supposing that as the pressure is applied to the edge of the flint it takes the line of least resistance, and when one thin flake is removed, then the pressure removes another equally thin, and so on. The same effect can be obtained by placing a stone with a sharp edge upon another stone of a suitable shape and turning on them with one's heel. Here again a number of flakes are removed which sometimes give the sharp-edged flint the appearance of a hollow scraper, but a close examination immediately shows that the flakes detached have been exceedingly thin. When I got these effects I tried to imitate them with blows but failed because I was unable to strike the flint near enough to its edge to detach such thin pieces, and I have no hesitation in saying that no one in the past or present could do so either, and I therefore assert that we have here another means of distinguishing Man's work from Nature's.

If one produces a hollow scraper by pressure and then another by ordinary blows, it is at once obvious that the flaking on the two flints has been produced at two different angles, because in the case of pressure very thin flakes are detached, while with blows, deeper flakes are taken off—as in Plate XXIII., Figs. 4 and 5. Of course it must be understood that I am making these comparisons in reference to rudimentary flint implements, and not to some of the later neoliths which were undoubtedly flaked by pressure applied by man.

Another experiment I conducted to simulate the effect upon a flint if it were fixed vertically in an ice-sheet and dragged along over a hard surface—a condition of affairs which I consider would be most rare in nature, but which some of our friends have mentioned as being a highly probable thing to happen—was to get a sharp-edged flint of a semi-circular shape, and securely holding it with a pair of tongs drag it forcibly over a cement floor.

I first of all applied the pressure at A (Plate XXIII., Fig. 6), then at B, and finally at C, and consequently got small flakes removed all round the edge. The resulting edge was however naturally blunted by the pressure, and made useless for any cutting or scraping purposes. Also the angles at which the flakes had been removed varied at points A, B, C. Here again, too, the thinness of the flakes detached showed that they had been removed by natural pressure.

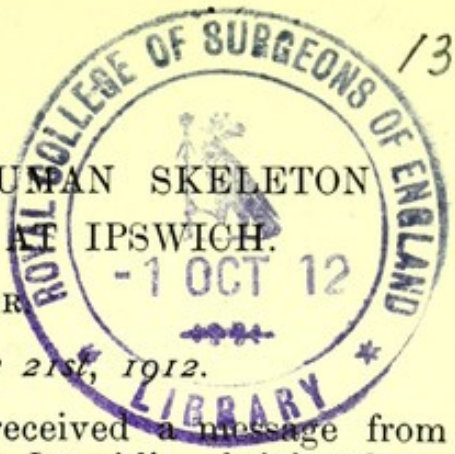
Now these experiments have clearly shewn that pressure does not act vertically through sand—at least *I* cannot get it to do so—and therefore I am justified in disagreeing with those who assert that certain flints have been flaked by this means, under fine sand. Also that flakes removed by pressure very often shew two bulbs, which no human being could possibly imitate. Also that pressure flakes removed which do not shew this characteristic have bulbs which are best described as only partly developed, and could under no circumstances be imitated by a human blow. Also that many flakes removed by pressure are so thin that they could not be simulated by blows, because it would be impossible to strike a flint sufficiently near the edge to detach such thin flakes. Also that it is well to remember that all my experiments were *carefully arranged* so as to get, if possible, good results, aud that Nature does not make any such careful arrangements in gravels, etc.

These conclusions and those arising out of my experiments with natural percussion, seem to me to be sufficient to make us more confident in deciding whether a flint has been humanly flaked or not. They will also, I think, have the effect of showing that these natural forces so frequently quoted by a certain school of geologists and archæologists are incapable of simulating man's work upon a flint. Long before I commenced any of the experiments I have described in this paper, I felt that the men who asserted that natural pressure and percussion could make implements were wrong, and now having brought these two forces into

play, I am more convinced than ever that my belief was right. I should think that in the whole realm of science there have never been wilder statements made than in that of pre-historic archæology, and I have yet to see equalled the audacity of some men who, when they pick up a stone on a sea beach or in a gravel pit, glibly say, "This has been flaked by Nature." When such an assertion is made, as it often is without the slightest evidential proof, I consider it is time for those of us who are keenly interested in these rudimentary flaked stones, to make an effort to place this vexed question of flint flaking on a sounder basis than that of mere ipse dixit.

In conclusion I would like to say that I do not consider my experiments to be the final word in this matter by any means, but I hope that others will follow my example and try to find out by experiment what natural forces can do in flaking flints. May I also urge all members of our society to search carefully all deposits which, by reason of their antiquity, have been banned by some archæologists as being non-implementiferous. These strata have been banned because of pre-conceived ideas about the antiquity of man based on biological grounds. These pre-conceived ideas are worthless. The biologists have no evidence as to when Man first appeared on this earth, and in my opinion are quite absurd in drawing conclusions regarding him from the other mammalia. No, the field is clear, and no one knows how old Man is; so search the deposits of chalky Boulder Clay, the middle Glacial gravels, the contorted drift, the stone bed below the Crags and even the Eocene flint stratum below the London clay. Only by this means will our knowledge of our early ancestors increase, and the true antiquity of Man be discovered.

13



THE OCCURRENCE OF A HUMAN SKELETON IN A GLACIAL DEPOSIT AT IPSWICH.

By J. Reid Moir.

Read at Ipswich, February 21st, 1912.

On Friday, October 6th, 1911, I received a message from Mr. Laughlin, of Messrs. Bolton & Laughlin, brickmakers, Ipswich, to the effect that one of his workmen, while removing some surface clay to get at the underlying glacial gravel, had turned out some human bones. I at once went down to the pit where the discovery had been made and found that a portion of a human skull attached to an almost complete cranial cast had been recovered. The workmen pointed out that two bones were protruding from the spot whence the skull had been derived, and so it was arranged for the remainder of the skeleton to be dug up on the following day. Messrs. Woolnough, Canton, and Snell accompanied me, and we afterwards drew up a short report of our observations. This report is as follows :—

"We, the undersigned, were present at, and superintended, the digging out of the human remains found at Messrs. Bolton & Laughlin's pit, Ipswich, on Saturday, October 7th, 1911. We all most carefully examined the section of decalcified Boulder Clay, under which the bones lay, before any digging commenced, and were absolutely convinced that no grave had ever been dug on the spot before. This opinion was confirmed (1) By the extreme hardness of the Boulder Clay, which necessitated the continued use of picks in getting it up. (2) There was not the slightest sign of any mixing of the soils (such as would occur in an old grave), the Boulder Clay resting normally on the underlying glacial sand as it does in all sections known to us where the succession of the beds is the same. (3) That in the event of a grave having been dug in the past on this spot, it is hardly conceivable that we should have dug down in exactly the same place as the original diggers, and that therefore one side at least of their digging would be visible in the remaining section of Boulder Clay. (We invite anyone interested to visit and examine this section, when they will, we think, be convinced that no early digging has ever taken place). (4) The extraordinary cast of decalcified Boulder Clay which completely filled the inside of the skull, we consider points to the conclusion that the clay was in a semi-fluid state at or since the time when the remains were embedded in it. We think it most unlikely that the clay in its present hard condition (a condition which has apparently been present since the last great extension of the glaciers) could work its way into any skull buried in it. (5) The bones were lying partly embedded in glacial sand, and partly in decalcified Boulder Clay—this sand showed most

plainly the lines of stratification, and was quite conformable with that underlying it.

(Signed),

"Reid Moir.

"Frank Woolnough, F.R. Met. S.

"*Curator of Ipswich Museum,*

"Frederick Canton, M.R.C.S., L.R.C.P., L.S.P., L.D.S.

"Norris Snell, L.D.S."

It was found that when the bones were reached they were in such a friable condition as to necessitate the digging up of the strata in which they lay in big lumps. This was accordingly done, and the same evening the remains, together with the skull fragment and cranial cast, were carefully packed in a suitable box and despatched to Professor Arthur Keith, M.D., of the Royal College of Surgeons, London, for examination. As the skeleton was found lying just at the junction of the Chalky Boulder Clay with the glacial sand—which are both well shown in this pit—it was thought advisable to get the opinion of some expert geologists as to whether the clay on each side of where the skeleton was found was *in situ* and not re-deposited. Accordingly three well-known gentlemen were communicated with—Mr. W. Whitaker, F.R.S., Dr. J. E. Marr, F.R.S., and Mr. George Slater, and they all very kindly visited and examined the section, and their various opinions are here given.

Mr. Whitaker's Report.

The occurrence of the human skeleton at the sand-pit near Prospect Cottages (southward of Dale Hall), Ipswich, is difficult to explain. The pit is worked for sand and gravel, belonging to the Glacial Drift, and is just where Boulder Clay is marked on the Geological Survey Map (48 N.W.) as coming on above the sand, etc. Along the top of the pit, indeed, the sand is in great part capped by a brown loam or clayey sand (sometimes practically a clay), which is clearly the result of the weathering and decalcification of the Boulder Clay. At the northern part of the pit there are also thin masses of Boulder Clay which have not been altered by weathering, and often in the loam there are lumps of Boulder Clay left, surrounded by the loam. The pit reaches up to the top of the hill, and is not dominated by higher ground from which slips can have come. *There is no doubt in my mind that the pit gives a junction-section of the Boulder Clay with the underlying sand and gravel.* The skeleton was found at the eastern side of the pit at this junction. The top earth was only some three feet thick, and the process of weathering has gone to such an extent that perhaps no one seeing merely that one particular spot would say that Boulder

Clay occurred, but there is unweathered Boulder Clay close by in the same sandy or loamy earth, and I can see no reason to differentiate one particular yard or so of the section from its immediate surroundings. I could see no signs of artificial disturbance of the soil, and was told that none had been seen during the excavation. Slipping seems out of the question. There is a remarkable thing as to the condition of the skull, which I saw at the College of Surgeons. The bony cavity is filled with earth of the same kind as that beneath which the skeleton was found, a brown loam, and the filling is so thorough (as far as can be seen) that a cast of the cavity has been made. Now this could hardly have been done by the introduction of dry earth; the infilling material must have got into the skull in a somewhat liquid state. I fail, however, to understand how man could have lived at the time of the commencement of the Boulder Clay, and I am in hopes that further excavation may throw more light on this strange occurrence. As yet we have the skeleton and nothing else. In conclusion, I wish to say that it is well that this find fell to the hands of a man like Mr. Moir, who at once took measures to secure the remains in such a way as to leave them as much undisturbed as possible, and ready for examination by skilled observers.

W. WHITAKER,
2nd November, 1911.

DR. MARR'S REPORT.

I visited the site of the discovery of the skeleton below clay at Ipswich after the skeleton was removed, and I leave to others the proof of there having been no interment there. My object was to discover the origin of the clay. It resembles decalcified boulder clay, and had patches of unaltered clay here and there. It was very thin, and I should be sorry to pronounce any definite opinion about it, as my knowledge of the glacial deposits of the south-east of England is not so extensive as that which I possess of those of northern England. I do not know how one would distinguish between boulder clay *in situ* and clay which had been derived from boulder clay at a somewhat higher level which had "flowed" as the result of being waterlogged. Such flows are often seen on beaches below boulder clay cliffs, and they strongly resemble true boulder clay. I do not wish to suggest the clay above the skeleton had this origin, but merely that I personally am unable to distinguish a thin mass of such a clay from true boulder clay.

JOHN E. MARR,
15th November, 1911.

In another letter to me, Dr. Marr says: "The material above the sand in the pit is lithologically boulder clay,

which has been decalcified. . . . All I can say is that I see no difference between the decalcified boulder clay in the higher pit, above the normal undecalcified boulder clay, and the decalcified clay above the skeleton."

Mr. Slater's Report.

The pit from which the "bones" were obtained is situated a quarter of a mile West of the Henley Road and a little to the East of the large brickyard of Messrs. Bolton and Laughlin, both pits being situated about a mile North-West of Christchurch Mansion, and bounded by the railway line on the North and the Henley and Norwich Roads on the East and West. The sand pit is marked as such on the one-inch geological map, which also shows the junction of the Middle Glacial Sands and the Boulder Clay, as occurring North of the sand pit at the time when the district was originally surveyed and the map published, in 1882. During recent years the sand pit has been worked considerably, chiefly towards the North, and is now cut back from the roadway in the form of a wide semicircle. The floor of the pit consists of fine cross-bedded middle glacial sands, upwards of 10 feet in thickness, containing intercalations of lenticular bands of clay, above which, and forming the greater portion of the section, is an irregular mass of sand and gravel, containing derived fossils occasionally, especially those of a durable character, such as *gryphæa*. The gravel varies very much both in arrangement and thickness, the maximum depth being about 20 feet. Resting upon this sand and gravel is a band of boulder clay not more than three feet in thickness, for the most part weathered brown but showing unmistakable patches of unweathered boulder clay, the whole thinning off on the Western side of the pit. In places, infiltration has caused the junction between the boulder clay and underlying sand to be irregular, but generally speaking the sand immediately below the boulder clay has been protected and is highly calcareous, a small, well-marked "pan" of calcareous material being well marked in places a short distance below the junction between the boulder clay and sand. Recently the workmen have commenced to extend the pit eastwards, in the direction of the adjoining cottages, and it was whilst so engaged that the bones were discovered. The site where the bones were found was shown to me on October 15th, and was situated at the extreme East of the pit, about three feet below the surface, *i.e.*, from the top of the pit, immediately below the boulder clay and embedded in highly calcareous sands, a small band of the calcareous material being well marked and continuous. As the bones had been removed and a "notch" cut down from the top of the pit to a depth of about four feet, a clear section was shown, but of course there was no means of ascertaining the exact condition of the material removed. *Judging from the*

section now exposed, this portion of the pit varies in no way from other parts of the section, and shows a clear and undisturbed section of weathered boulder clay over the calcareous sands in which the remains were found. There is no reason to doubt that the sands and gravels are derived from glacial material, containing as they do derived Jurassic material, and the boulder clay is part of the large sheet exposed so well in the neighbouring pit, further to the East of Henley Road.

GEORGE SLATER,
October 21st, 1911.

Professor Keith also having examined the bones sent on a most able and detailed report of their characteristics.

It will, I think, be seen from these carefully-compiled reports that in all its aspects this matter has been gone into in as thorough and scientific a manner as possible.

Now it appears that the main points of this discovery for consideration are:—(1) Was a grave ever dug through the boulder clay? (2) Is the boulder clay *in situ* or re-deposited?

Regarding the first, the following facts have been accumulated:—A most careful examination of the section before the disinterment took place showed clearly that no signs of any previous digging were visible, the clay above the skeleton appeared to be in every way the same as that which extended for some distance on each side of it. There was not the slightest mixing of the soils apparent, such as is now to be seen, where the hole dug has been filled in. No grave furniture was found with the body, such as usually occurs in burials of many subsequent periods. Immediately underneath the bones a pronounced calcareous band was present. This deposit, which is often found in sand underlying boulder clay, and which gives a marked effervescence with hydrochloric acid, is much in evidence in this pit, and extends more or less continuously on either side of the spot where the remains were found. If a grave had been dug through the boulder clay, the rain-water, percolating through the loose material filling the grave, would have dissolved away this calcareous deposit.

The skeleton was lying partly embedded in glacial sand and partly in boulder clay. This sand showed clearly lines of stratification, and was conformable with that underlying it. This could not be the case if a grave had been dug down through the clay and into the sand.

The extraordinary cranial cast, formed of the sandy loam in which the bones rested, and which was so perfect that the part of the frontal lobe of the brain connected with speech is clearly marked on it, appears to point to the fact that the material composing it must have been introduced while in a semi-fluid state, and it is difficult to believe that the clay and sand have been in such a condition since the time when a grave would have been dug.

On February 7th, 1912, being anxious to get a good photograph of the hole we had filled in after removing the skeleton, I went down to the pit and cleared away the material under and on each side of our digging. When this was done the striking difference between the material filling the hole and that surrounding it was very manifest, as the former was an indiscriminate mixture of surface humus, boulder clay, and glacial sand, while the other showed hard undisturbed boulder clay underlain by clean, stratified glacial sand. Now I have no hesitation in saying that if ever a grave had been dug on this spot before, the inevitable mixing of the soils would still be visible. Therefore, I think we can quite put aside any idea of a late interment.

The occurrence of such easily-destroyed things as human bones under a stratum which was deposited by an ice-sheet, is at first sight rather remarkable, but my opinion is that the body of this man was covered by a considerable depth of sand before the boulder clay was deposited. Even if the sand was only a few feet in thickness, it would be sufficient completely to protect the bones from any pressure which might be brought to bear upon them. It must be remembered, too, that the erosive power of ice is very variable, as sometimes it passes over a surface of loose sand without disturbing it at all.

Let us suppose that this man, whose remains we have found, was wandering over this sandy land surface, and was overcome with the cold. If this were the case he would lie down, curl himself up for warmth, and eventually get covered by the sand as it was blown by the wind. This supposition is borne out by the fact of the contracted position in which the body was found, and also as the climate was fast degenerating into glacial conditions, it is certain very low temperatures were present.

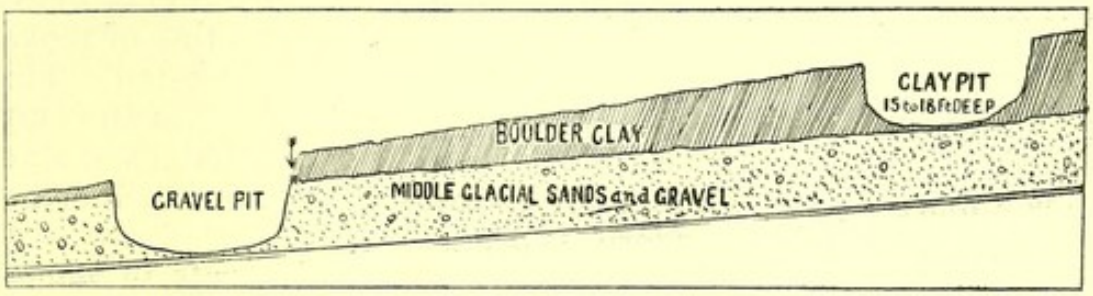
There also seems no doubt that when the boulder clay was first deposited there was a very much greater thickness of it than is seen now. The melting of the ice-sheet which laid it down would cause a lot of denudation, and during the ages which have passed since the ice finally disappeared the same process has been continually going on. It may be asked whether we have found other bones on the same horizon as the skeleton, and we must confess that up to the present we have not done so. But at Leiston, in a deposit which underlies chalky boulder clay and which I look upon as Middle Glacial, a large tusk of an elephant and other bones have been discovered.

Now if this man was lying on this glacial sand, and was covered by the boulder clay, we can be sure that as the clay became decalcified the human bones would also disappear by the same process. This is exactly what has happened. The skeleton was found lying partly embedded in glacial sand and partly in boulder clay. The portions in the sand have been fairly well preserved while those in the clay have almost entirely disappeared. The remaining grains of chalk in the boulder clay are stained to exactly the same depth as the human bones, and it is quite

evident that both have been subjected to the same conditions. This seems to me to be conclusive evidence that this man was lying on the sand before the clay was deposited, and that the processes which since then have affected the clay, have also, in exactly the same way, affected the bones.

The discovery of a human skeleton below the chalky boulder clay did not come as a great surprise to me, because for some long time past I have been finding well-worked flint implements in the clay and also at the junction of the clay-sand where the skeleton was found. These implements are generally unpatinated, and in many cases as sharp as the day they were made. This fact shows, I think, that the ice-sheet which deposited the boulder clay was moving over a land surface on which these implements were lying, and they consequently got incorporated into its bottom moraine. Thus having found the implements we have by great good fortune found one of the men who made them. The worked flints discovered are of pre-palæolithic forms, and as no true palæoliths have ever been found in the boulder clay or the gravels underlying it, we may conclude that these deposits pre-date the palæolithic gravels in this part of the country.

PLATE XXXVIII.



Diagrammatic section ; the pits are half-a-mile apart, with fall of 26 feet between them. The spot where skeleton was found is indicated by an arrow.

We can now proceed to discuss the second question, " Is the boulder clay *in situ* or re-deposited ? " The evidence in favour of its being *in situ* is as follows :—Small holes lately dug by myself have proved that the boulder clay in the pit under discussion, where it is not more than four feet thick, extends *continuously* eastward to another pit where it develops into a section 18 to 20 feet in thickness. The clay above and on each side of the bones is mostly decalcified, though unchanged patches occur here and there. These patches, however, merge imperceptibly into the surrounding decalcified portions, and they must necessarily be one and the same formation. This decalcification has been no doubt caused by the proximity of the clay to the surface, where it has been exposed to all the actions which lead to decalcification. In the deeper section mentioned the same condition is apparent, that is, the uppermost portion of the clay is partly decalcified. The lower portions which are further away

from surface conditions have remained unchanged. The glacial sand underlying the clay in both sections is highly calcareous. This condition could not possibly be present if at any time the clay had been denuded and re-deposited, because the water which would accompany any such phenomena would dissolve out the chalk from the under-lying sand. There can, I think, be no doubt that the material under which the bones were lying is the undisturbed, though eroded and partly decalcified, base of the chalky boulder clay formation.

The evidence against the clay being *in situ* takes the form of the opinion expressed by Dr. Marr that it may have got water-logged and consequently caused to "flow." It is, however, pointed out that the spot where the bones were found is on the plateau at a level of 130 ft. O.D., and with a slope of 26 ft. in half-a-mile to the highest point of that plateau, from which a "flow" could have come. Dr. Marr's opinion, however, is that the slopes *are* sufficient to allow of such a "flow," and also higher ground may have at one time existed which has since been removed by denudation. He also suggests that such a liquid condition of the boulder clay as must necessarily have been present, if it ever "flowed," would account for the complete in-filling of the skull of the skeleton discovered. We have, however, up to the present no actual evidence that such a "flow," and the conditions giving rise to it, ever existed, and it is also presumed that the stones in the clay above the bones would lie with their longer axes along the line of movement if the clay had ever moved in the manner suggested. This is certainly not the case, as they occur in all positions in the clay.

It has no doubt come as a surprise to many that this man, who is apparently so much more ancient than the Neanderthal men, should be of a modern type, while the latter are very primitive. These Neanderthal men made implements of *middle* palæolithic age which are not nearly so finely made as those found in the river terrace gravels, which are, nevertheless, more ancient. Now if we are to judge the type of man from his implements, then a skull of the same age as the early river terrace implements would be of a more advanced type than Neanderthal. That this is true has been proved by the discovery of the famous Galley Hill skull which, though contemporaneous with the 100 ft. terrace of the Thames, and much older than the Moustier deposits in which the Neanderthal men are found, is of a distinctly modern type. This Galley Hill man has opened the eyes of anthropologists to the fact that modern man is much more ancient than we supposed, and this discovery at Ipswich proves that during one of the warm interludes of the Great Ice Period he was already evolved. We cannot say in terms of years how old the 100 ft. terrace of the Thames, with its human remains, is, but that it is very ancient is admitted by everyone.

In a letter written to me, Mr. Whitaker says that at Upminster in Essex this 100 ft. terrace *rests upon*, and is therefore

less ancient than, the boulder clay, and as this man we have found lived before the boulder clay was deposited, some idea can perhaps be formed of his antiquity.

Since the reading of the above paper a discovery has been made at Charsfield, in Suffolk, which affords corroborative evidence in favour of the antiquity claimed for the human skeleton found at Ipswich, and an answer to those critics who complained that no other mammalian bones had been discovered at the same horizon at which it occurred. Charsfield lies to the North of Ipswich, and is about 11 miles distant from the spot where the human bones were found.

A small pit in a shallow, dry valley, is there being worked for stone, and shows an 11-ft. section composed of 1 foot humus and 3 feet blackish gravel, resting upon a weathered surface of chalky boulder clay. This deposit is about 4 feet thick, and is underlain by 3 feet of loamy gravel. Under this deposit and at the bottom of the section the fine, stoneless middle glacial sand is exposed.

Lying on the surface of this sand, and partly imbedded in it and the different overlying material, a large tusk, associated with numerous other pieces of elephant bone, has been discovered. The accompanying photograph (Plate XXXIX.), taken by Mr. Woolnough with the camera suspended over the pit, shows the tusk *in situ*. It was afterwards carefully removed and taken to the Ipswich Museum.

It will be noticed that these remains occurred at exactly the same horizon as the human skeleton found at Ipswich, and examination has shown that the mineral condition and the amount of iron-staining of the bones are also the same. No teeth having been found it is somewhat difficult to say with certainty what species of elephant has been discovered, but the tusk and bones have been sent away for identification to South Kensington, and the report of the experts there will be published at a later date. This interesting discovery was first brought to my notice by Mr. W. H. Youngman, of Charsfield, and Mr. E. P. Ridley, F.G.S., Mr. Frank Woolnough, Curator of Ipswich Museum, and Mr. Fredk. Canton, accompanied me when I went over to have the tusk photographed and removed.

It appears to me that this find affords strong support to my contention that the top of the middle glacial sand was a land surface before the deposition of the chalky boulder clay—this elephant was apparently a contemporary of the Ipswich Man on that land surface. The occurrence of human and elephant remains at the same horizon, and within 11 miles of each other, suggests that we have here a deposit rich in remains of great interest, and now that the owners of pits in East Anglia are on the look-out for and realise the importance of these things, it is highly probable that other relics will be brought to light, which, till the discovery of the Ipswich Man, would have been thrown away as being of no value.

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DESCRIPTION OF THE IPSWICH SKELETON.

By Arthur Keith, M.D., LL.D.,
Conservator of the Museum, and Hunterian Professor, Royal College of Surgeons of England.

Read at Ipswich, February 21st, 1912.

Mr. Moir has described the circumstances under which the skeleton was found and has given the evidence on which these bones have been assigned to the period preceding the formation of the Boulder Clay. It remains for me to give a description of the skeleton and to draw certain inferences as to the kind of man to which it belonged. Before doing so, however, I should like to mention a matter which may be of service to future investigators. So fragile were the remains, such was the degree of disintegration, that Mr. Moir found it impossible to extract the bones from the matrix in which they were embedded. He therefore, most wisely, cut out the stratum in which the bones lay as solid blocks, which were forwarded to me in the condition in which they were excavated. On reaching the Museum we impregnated each block with a solution of gelatine and then proceeded to expose the bones by removing from them the overlying boulder clay piecemeal and leaving them still *in situ* on the underlying glacial sands. The removal of the boulder clay required extreme care and great patience, but the advantage of leaving the bones attached to the underlying parts of the blocks became very apparent when we came to reconstruct the position of parts of the skeleton. There is the further advantage that anyone can now examine the exact relationship of the parts to the strata in which they lie.

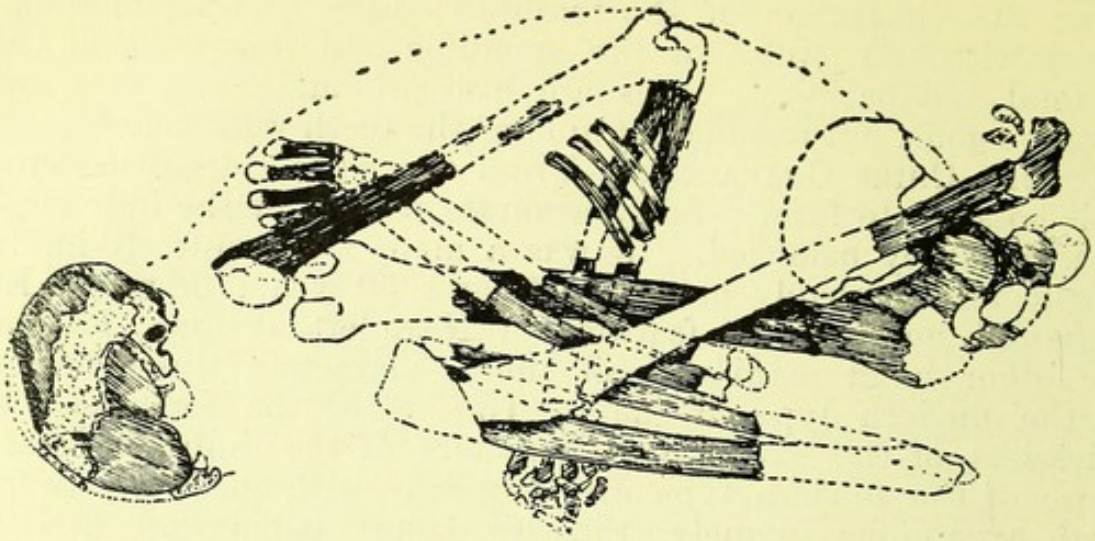
The Parts Preserved.—There can be no doubt the whole skeleton is represented and that its various parts are in their proper position to each other. The right side of the skeleton, which lay in contact with the glacial sands, is much better preserved than the left, which was uppermost and embedded in the boulder clay and thus most subjected to the destructive effects of the roots of plants and the eroding action of the clay. The roots reach deeply in the glacial sands and their effects are especially manifest on the skull and pelvis. The boulder clay (sandy, chalky loam) has also eaten into the bones; all the soft spongy bone, such as occurs in the feet, the ends of long bones and in the spine, was represented by dense clay in which minute fragments of the original bone could be detected. Thus there was not a single complete bone recovered with the exception of the small bones of the right hand. By great fortune the cavity of the skull must have been completely filled at an early stage with the surrounding matrix, and thus a complete cast of its interior has been preserved. A fragment of the frontal bone, sufficient to

show the characters of the forehead; parts of both temporal bones with the joints for the mandible and fragments of the parietal and occipital bones are also present. The face and jaws are gone, but fortunately nine of the teeth were found.

THE CHIEF CHARACTERS.—From the parts thus discovered it is possible to form a fairly accurate picture of the individual to which they belonged. He was a man, about 5-ft. 10-in. in height and probably between 40 and 50 years of age. He possessed none of the features of Neanderthal man, but on the other hand manifests all those characters which belong to the modern type of man. The teeth, the forehead, the thickness of the skull, the characters of the limb bones are those of the modern type of European—with one (or perhaps two) exceptions—namely, the leg bones (tibia and fibula), and perhaps the arm bone or humerus. Only the upper part of the left humerus is preserved, but so eroded is its condition that its original form cannot be quite judged. But enough remains to show that it differed in details of shape and character from the corresponding bone in modern men. I am disposed to lay great weight on the characters of the tibia and fibula. These are exactly opposite in type to the same bones in Neolithic man. The outstanding feature of the tibia is the absence of a sharp crest or shin; this is also the case in Neanderthal man, but in shape and size the tibiæ of the Ipswich and Neanderthal men belong to different types altogether. Assuming that Mr. Moir and I are right in regarding this skeleton as representative of pre-boulder clay man, then it is a most interesting fact to find that the modern type of man—with the exception of certain features of the leg bones—was evolved at this early date, for we are dealing with a period which antedates by a long interval the Mousterian period when Neanderthal man flourished in France and Belgium. I estimate from the cranial cast that the brain capacity of the Ipswich cranium was 1430 cubic centimetres. An average modern man has a cranial capacity of about 1480; a tall man, such as this, should have a certain percentage more—a little over 1500 cubic centimetres. The cranial capacity, however, varies so from individual to individual that too much stress must not be laid on the small size of the brain of the Ipswich man.

THE POSTURE OF THE SKELETON.—We come now to discuss an important matter—the posture of the skeleton. The position of the body at death is easier of realisation than of description. If one squats so that the buttocks rest on the heels and so that the knees are pressed against the front of the body, the main posture of this skeleton will be represented. Only instead of being in the squatting posture the skeleton rests on its right side with the head bent forward so as almost to reach the knees. Then the right arm is flexed and lies beneath the body so that the right hand rests under

PLATE XLI.



The Ipswich Skeleton—Position of body.

(From a drawing by Professor Keith, photographed by Mr. F. Woolnough.)

the right leg. The left arm is more acutely flexed than the right, its elbow being gripped between the knees while the left hand is turned against the left shoulder. It will be seen that the position of the skeleton somewhat resembles the contracted postures found in neolithic burials, and the question arises—Was there a burial here, or did the man sink down, die, and afterwards become overwhelmed beneath the stratum of boulder clay? I do not remember any contracted burial in which the thighs were so tightly flexed on the body as was the case in the Ipswich skeleton, nor one in which there was quite the same disposition of the limbs. A Peruvian mummy in the Museum of the College of Surgeons does show the same degree of contracture of the limbs and almost the same position of the limbs. It occupies a squatting position. From the appearance of the mummy I suspect the body was forced into its present position after death—but there is no certainty of this having been the case. My first impression was that the position of parts seen in the Ipswich skeleton could not be assumed by the body at death, and that therefore force must have been employed to place the limbs in the position which they occupied when the skeleton was discovered. Were I, however, placed in the witness-box and asked if it is impossible for a dying man to assume this posture, and for this posture to be preserved after death, I should have to agree with Dr. Sturge, and admit that such a posture does seem possible. Yet I have never seen a dead body in this posture, but then I have to admit that I have only seen the bodies of those dying under the conditions of modern civilization. I have seen people cuddled up in a drunken sleep, who if they had died in that condition would have retained the ultra-contracted posture.

I therefore abandon the point that the contracted posture necessarily means burial. If there had been a burial we ought to be able to detect in the glacial sands just under the body a distinct junctional line indicating the floor of the grave. No distinct line is visible; such indication as there is—a loss in the distinctness of the stratification to the depth of an inch or more below the skeleton—seems to me to have been caused by the action of the roots which have reached the body and passed some distance beneath it. Mention must also be made of various pebbles and splinters of stone which are found immediately beneath certain parts of the skeleton—as if they might have rolled into an open grave. It must be remembered, however, that in the deepest stratum of the boulde clay such stones are extremely abundant. The question of burial must be left open: it can neither be absolutely proved nor completely excluded, although the balance of evidence is perhaps against burial. In any case, whether burial or accidental inhumation, one must presume that there must have been a land surface at this horizon. That surface may have been obliterated during the deposition of the boulder clay, yet I have great difficulty in picturing a land surface at this place under any of the current theories relating to the manner in which the boulder clay was deposited.

Signs of Antiquity in the Bones.—The bones are not mineralised; on the contrary, they are extremely light. When a fragment is broken off, it is seen that the bone matter has exactly the appearance of chalk—it is white and homogeneous. The surfaces of the bones are stained a light reddish brown, probably an iron stain, but it is seen to affect only the surface layer; the stain never penetrates to a greater depth than a millimetre, often much less. The fragments of chalk in the boulder clay are also white, merely their surfaces bear the red stain. I placed a fragment of the tibia and of the skull in 5% of hydrochloric acid; as controls I took fragments of fossil bones from under brick earth, from beneath the red crag, from a Neolithic skeleton found in chalk, and from an ancient skull from the Thames bed. In the sub-crag and brick earth fossils the bones crumbled to a sediment; in the Ipswich skeleton the bones also crumbled but formed a flaky ragged sediment; the fragments of the other bones retained their shape owing to the cartilaginous and gelatinous tissue preserved in them. So far as this rough test goes, it is in favour of the antiquity assumed for the Ipswich bones. Too much weight must not be attached to the condition of the bones; a mandible from the coprolite pit at Foxhall, near Ipswich, which we must presume is older than the skeleton here described, was rejected because it contained a high percentage of gelatine and did not show a greater degree of mineralisation than the teeth and bones of the Ipswich skeleton. For a similar reason the human mandible removed

by Boucher de Perthes from a bed which Prestwich described as the oldest in the Somme Valley was rejected. The Engis skull, found with bones of the mammoth and rhinoceros is also being rejected because it is not mineralised. The human and reindeer bones found in a cave at Langwith-Bassett, Derbyshire, by the Rev. E. H. Mullins, were remarkably fresh in appearance. I therefore conclude that there is nothing in the condition of the Ipswich bones which prevents us from accepting them as of pre-boulder clay date.

Another condition requires explanation. How did the skull become filled? The matrix is exactly the same as the stratum in which the skull lay—a sandy loam with a certain percentage of chalk. One cannot easily conceive a skull becoming filled if it were now buried in this stratum—a stratum which requires a pick to pierce and dislodge it. The matrix could be carried in by three agencies,* (1) water, (2) roots, (3) worms. I tkink the two last may be excluded, for they are selective in their action. I should not expect them to carry within the skull an accurate sample of the material which surrounded the skull. We must therefore revert to water. Could the rain permeate the boulder clay to this depth? Mr. Moir is quite definite on this point: he denies the possibility. Yet one must remember the roots are now reaching and penetrating the skull, and we may be quite certain that moisture penetrates as far as roots. Yet I do not think moisture could penetrate in such quantities as would fill the skull. We are therefore driven back to suppose that when the skull came to rest here there existed a more moist or fluid condition of the stratum. It is not only the skull which is filled; all the medullary cavities of the long bones are also packed with the surrounding matrix. Even the pulp cavities of the teeth and the finer closed spaces of the bones are stained and contain fine grains of sand.

Characters of the Head.—In dimensions, in shape, and in its essential characters, the head, so far as we may judge from the cranial cast and fragments of the skull, does not differ in any essential feature from that of modern Europeans. Its maximum length, 192 mm. is a common skull length among modern Englisnmen; the maximum width, 144 mm., is also a common measurement; the proportion of breadth to the length, 75 %, also abounds. There is one peculiar feature in the maximum width of the skull—it is situated nearer the posterior or occipital end of the skull than is usually the case amongst modern races, and in this feature the Ipswich man recalls the Neanderthal race. Another primitive feature of the skull is its flattened form; it is

* I have seen lately several skulls from graves dug in chalky loamy soil, which were perfectly filled wlth a matrix exactly similar to the surrounding soil.

compressed from above downwards so that the vault rises only 110 mm.—15 or 20 mm. less than is usual in modern European races. The forehead is sloping; the supra-orbital ridges are not strongly marked; there is absolutely no trace of the massive supra-orbital bar which characterises the Neanderthal race. Indeed, the bones of the skull are remarkably thin—varying from 4 to 5 mm.; at the root of the nose the frontal bone measures 18 mm. in thickness—an amount which occurs in modern skulls. The frontal air sinuses at the root of the nose are small. The joint for receiving the articulation of the lower jaw is of the form which occurs to-day. The muscles of mastication were not large.

Characters of the Teeth.—Nine teeth were discovered, and it may be at once said that in size and shape they present no feature which distinguishes them from the teeth in late prehistoric times. The crowns of the teeth are worn down so that the dentine is exposed on the crowns. An upper wisdom tooth is peg-shaped and remarkably small. I admit freely that we did not expect to find the teeth and the masticatory apparatus in the form just described at so early a date, but what we expect is one thing—what we actually find is another. The teeth showed no marks of disease; only signs of severe wear. They are not deeply stained or mineralised; they are remarkably light, and when broken across the pulp cavities are seen to be stained a brownish red, and so is the layer of dentine surrounding the pulp cavity. The rest of the dentine is chalky in appearance.

Characters of the Limb Bones.—I do not propose to describe fully all the characters of the limb bones at present. It must be remembered that all of them are fragmentary, and my estimation of their original lengths has been arrived at by comparing them with other bones from men of various statures. Without doubt the man was tall; I estimate the length of the femur at about 490 mm. and the tibia at 380 mm., both really long bones. Neither show traces of a great muscular development—rather the reverse. The femur shows no outstanding feature. The flattening of the upper extremity, which is almost invariably present in Neolithic femora, is here absent. As already said, the leg bones—the tibiæ and fibulæ—show the most remarkable features. In Neolithic tibiæ there is a very marked flattening of the shaft from side to side. In the tibia of this skeleton there is no flattening; rather the opposite. In a section at the junction of the lower and middle thirds the shape is that of the letter "D." There is no crest or shin in front of the shaft. The significance of this feature is not easy of explanation, because we do not know the functional meaning of the pronounced shin of the tibiæ seen in all races of modern man. We know, however, the sharp shin is absent in the leg bones of anthropoids, and is but ill-

marked in the tibia of the Neanderthal race. It is, therefore, a significant feature, and one which I expect will characterise the race to which this isolated skeleton belongs. The fibula is also peculiar in form—it is oval in section and does not show the peculiar flutings found in modern and ancient races. No trace of the foot bones was found—which is unfortunate, for from them we might have obtained an explanation of the shape of the tibia.

Mention has already been made of peculiar features of the humerus, but the bone is in too imperfect a condition to allow stress to be laid on them. The forearm bones are slender; the bones of the hand small for a man of 5 ft. 10 in. The individual bones are exactly similar in shape and arrangement to the same bones in modern man.

Summary.—If Mr. Moir and I are right in assigning the remains here described to a man who lived in Suffolk before the formation of the boulder clay, then there can be no doubt we are dealing with one of the earliest representations of man yet discovered. The only other remains which are certainly older are the Heidelberg jaw and the fossil man of Java—*Pithecanthropus erectus*. The Ipswich man differed profoundly from both of these, and in every point in which he differs from them he approaches modern man. In features—excepting the leg bones and some aspects of the head—he is modern man. That the human body should have attained its present form at the period to which the Ipswich man is referred does or should not come to either the zoologist or anthropologist as a surprise. The majority of the mammalia which accompany him had also arrived by then at their present place in the Animal Kingdom. It would certainly be a matter of surprise if the modern type of man was not then evolved. This discovery raises so many issues that it will take some time fully to realise its bearing on many problems concerning the evolution of man.