PRESIDENTIAL ADDRESS TO THE DORCHESTER MEETING OF THE INSTITUTE.[1]

By Lieut.-General PITT-RIVERS, F.R.S., F.S.A.

When Lord Dillon paid me the compliment of asking me to preside on the present occasion I accepted very readily, although I have given up attending Archæological Meetings on account of ill-health; but for some years past I have been engaged in carrying on excavations in this and the neighbouring county of Wilts, and having now completed the much more arduous task of recording the last part of what has been done, in the fourth volume of my quarto work on *Excavations in Cranborne Chase*, I thought it would be a good opportunity of reading before this Society, on the occasion of its visit to Dorchester, a *résumé* of the results of my investigations. I thought at one time of bringing here numerous large diagrams, plans, and sections, to explain the nature of the diggings that I have made, but on second thoughts it appeared to me impossible in a single address to give a detailed account of the work with the necessary precision, and I decided to complete the letterpress and illustrations of my quarto volume, and divide it into parts, fifteen copies of each of which I have placed upon the table or distributed to some of the members, who will thereby be put in possession of the evidence obtained, whilst I confined my address to giving a general outline of the results, which I hope may have some interest, although I am fully sensible that the value of such investigations depends mainly, if not entirely, on the precision with which the evidence is recorded.

Before, however, embarking upon the chief portion of my address, I desire to make one or two brief remarks upon a subject which, at the present moment, occupies a considerable share of the attention of the archæological world, namely, the palæolithic period of Egypt.

Mr. de Morgan, late Director-General of the Antiquities of Egypt, in the preface of his great work on *Egyptian Origins* (1896), makes the complaint that the students of Egyptian history, engrossed by the enormous resources at

[1] Delivered, August 3rd, 1897.

their disposal of materials for the study of the metallic period, and the deep interest and precision of knowledge afforded by the interpretation of the papyri, and influenced also by the facility with which they are transported up and down the Nile in boats, instead of passing along the country landward, have paid less attention to the deposits on the sides of the Nile valley or to the discovery of the sites of palæolithic and neolithic man in that Valley, than has since been done on the continent of Europe. This may have been partly true at the time when Mr. de Morgan's preface was written (1896), but since then the discoveries of Mr. Flinders Petrie and Mr. Seton-Karr have given abundant evidence of the existence of implements of palæolithic type in the valley and the desert bordering it on the east side. Much earlier than this, however, in 1881, I claim to have been the first to discover flint flakes and cores *in situ* in the stratified gravels of the Nile valley at Koorneh on the outskirts of Thebes, gravel in which, after having become indurated through the cementing together of the particles by calcareous infiltration, the Egyptians had cut their flat-topped tombs with square supporting pillars, that have continued perfect in the gravel-rock until the present day, thereby producing evidence of exactly the same character, in so far as sedimentary deposits are concerned, that had satisfied the fathers of prehistoric archæology in the valley of the Somme. Having experienced the drawbacks to careful study alluded to by Mr. de Morgan in travelling up the river as a Cook's tourist, I decided, on arriving at Luxor, to abandon the steamer and remain there whilst the boat went up to the Cataracts and back, thereby giving me a clear fortnight for the deposits of gravel in the valley. I selected Gebel Assart, a plateau in the bottom of the valley to the north-east of Koorneh, consisting of a delta of hardened sand and gravel, which had been washed down by the Babel Molook, in which the Tombs of the Kings are situated, and spread over the valley below, and which after depositing a delta in the valley between the sides of it and the river, had afterwards cut a channel through it by running water. In the sides of this channel or waddi, the Egyptians had cut their tombs, tunnelling under the nearly perpendicular banks in

the gravel, at that time converted into hard rock. After a careful examination of the sides of this waddi, extending over several days, I succeeded in finding unquestionable chert flakes and cores and one rough tool embedded in the matrix, which, of course, must have been deposited long previously to the hardening of the gravel, the erosion of the waddi, and the cutting of the tombs, some of the flakes being actually chiselled out of the sides of the tombs.

This discovery was afterwards referred to at some length in a paper read before the Victoria Institute by Sir J. W. Dawson in 1884. Dr. Dawson, however, made the unaccountable mistake of saying that the flints found by me in this gravel were natural forms, and that the bulbs of percussion on them were caused by the knocking together of the fragments of stone by natural causes, during the process of deposition. But he is mistaken in supposing that bulbs of percussion are formed to any extent in this way, as Professor Rupert Jones, in his remarks upon the paper, explained very forcibly at the time. If a single bulb of percussion could be so formed, which the examination of the constituents of different kinds of gravel shows is very rarely the case, the production of all the recognised characters of a flint flake could not be obtained by this means. The production of a single bulb on the flat side of the flint, two or more facets at the back with the hollows left by the bulbs of flakes previously struck off on them, and the small flat surface at the top, being the residuum of the flat surface of the core on which the blow was given to flake it off, all formed by blows delivered nearly at the same spot and in the same direction, could not possibly be produced otherwise than by the hand of man. This is an axiom so thoroughly established as to be familiar to the merest tyro in prehistoric investigations. I had considerable experience at the time in the fracture of flint, and had been in constant communication, and had worked with Sir John Evans, Canon Greenwell, Sir John Lubbock, and the late Professor Rolleston, and it would have been quite impossible for me to have made a mistake upon the question of the natural or artificial form of a flint flake or core. In fact, I think that it would have been thought ridiculous by any of the well-known archæologists that I have named to suppose that I could have

made such a blunder, in such a rudimentary matter.[1] I had discovered a palæolithic site in the valley of the Thames in 1869, being the first discovery of palæolithic implements in the gravel of the Thames above London, associated with the usual fauna of the period, which was identified by Professor Busk. This was published by me in the *Reports of the British Association* in 1869, and in the *Quarterly Journal of the Geological Society* in 1872, with a contoured plan and numerous sections, and with remarks by Professor Prestwich, who complimented me on the "exactness and completeness of the description," together with remarks by Mr. Godwin Austen, Dr. Evans, Mr. Flower, and Professor Ramsay, the latter of whom spoke expressly of the "undoubtedly artificial character of the implements." The discovery was subsequently recorded by Mr. Whitaker in the *Memoirs of the Geological Survey* for 1889, p. 341, who spoke of it as a "wholesale discovery." If Sir William Dawson had read these reports and descriptions, showing that my then discovery had been thoroughly ventilated and accepted by geologists, I think he would certainly himself have thought it incredible that eight years afterwards I should be found incapable of distinguishing a flint flake from a natural form. Furthermore, the actual discovery of the Egyptian flints now under consideration was very well authenticated and confirmed by others. Knowing that it was of a nature that was likely to be disputed, and impressed with the importance of having a competent witness to the find, I communicated the results of my searches to Mr. Campbell of Islay, who was stopping at Luxor at the time, and who was himself a geologist, and he went with me on a subsequent day to the spot and saw me chisel out a flake from the side of a tomb, he himself chiselling out another close to it. The finding of this particular flint is described by me in great detail, with drawings and sections, in my paper in the *Journal of the Anthropological Institute*, Vol. XI, 1882, p. 390, Mr. Campbell, who was present, authenticating the discovery in his remarks made at the time the paper was read (p. 397). Since then, other geologists have found similar worked flints in the same gravel and at the same spot. Sir J. W. Dawson was, of

[1] *Ancient Stone Implements of Great Britain*, 2nd Edition, 1897, p. 652.

course, a distinguished geologist, but his knowledge of prehistoric archæology was much more limited. There is nothing in his paper to show that he had ever seen the flints of which he spoke, and as they have always been in my possession, I am aware that he could not have done so. Had he seen them I am certain that no one with the most rudimentary knowledge of the fracture of flint could have mistaken them for natural forms.

It is true that my finds in these gravels did not include palæolithic implements of the tongue-shaped and oval types that have since been found in the desert and in the high plateau above the valley, from which deposits, the flakes and cores were I believe originally washed down into the delta below; but as the deposits in the bottom of the valley were as hard as rock, nothing could be seen but the few specimens that were in evidence embedded in the gravel on the sides of the waddi and the sides of the tombs. Quarrying into the matrix itself would have been a very costly and laborious undertaking, and though no doubt the usual more generally recognised palæolithic types would have been brought to light in abundance, the flakes and cores were amply sufficient to prove them to be works of human art. A palæolithic implement of the recognised type affords no better evidence of the presence of man than a flake. The surface of the delta was covered with implements of the usual palæolithic forms and they had frequently been noticed on the surface by others, and after having discovered flakes and cores and one rude implement, in the stratified deposits below, there could be no doubt that the implements themselves would have been found in the same gravel, if the rock had admitted of being dug into. Dr. Dawson himself admitted in his paper that there could be no doubt of the prehistoric period of the gravel; all therefore that was necessary was to show the presence of flints of human handiwork in it, to prove their immense priority to the Egyptian age.

It may be said perhaps that it sounds like ancient history to speak of discoveries made in 1881, when so much has been done since by subsequent explorers; and implements of palæolithic type have been found both on the plateau land immediately above the Nile Valley by

Mr. Flinders Petrie, and in the desert by Mr. Seton-Karr at some distance to the eastward of the Nile.[1] This is quite true, but on the other hand the evidence of age afforded by the position of the flints found by me in the deposits at the bottom of the valley, so far as it goes, is much better. The implements of palæolithic type found by Mr. Petrie and Mr. Karr, as I understand from their descriptions, and their verbal communications to me, were found on the surface only. Flints found on the surface of the soil cannot be legitimately disconnected from flints of the surface period, except by form, and form alone is not conclusive in determining date. The same forms might have been used in different countries at different periods. I have little doubt from their number and the absence, as a rule, of flints of a more advanced type amongst them, that they will eventually turn out to be of the true palæolithic age, but the evidence is as yet insufficient. Amongst the flints obtained by me from Mr. Seton-Karr, kindly selected by him as typical, from the large number found by him in Somaliland, was a leaf-shaped spear-head, $2\frac{5}{8}$-in. in length, $1\frac{5}{8}$-in. in breadth, and $\frac{1}{2}$-in. greatest thickness. This could not have been used in the hand without a haft, and might have been employed at the end of a rod as a spear-head. Another was a small scraper, and several flakes had secondary chipping on them. These correspond more nearly to the neolithic than the palæolithic forms of Europe, or at any rate to the more advanced palæolithic forms of the French caves. Had these been found in sedimentary deposits, it is possible they might have been found separated from the more typical and larger specimens of earlier palæolithic type; as it is, it perhaps leaves open a little doubt as to the exclusively palæolithic character of these flints. And this makes it necessary for me to vindicate the character of my original finds, which has been aspersed by Sir J. W. Dawson. We have there, at any rate, evidence of the use of flint tools by man at a time long antecedent to the cutting of the Egyptian tombs, whereas in estimating

[1] Petrie's *Naqada and Ballas*, 1896, pp. 49 and 50; and *Journal of the Anthropological Institute*, Vol. XXV. 1896, p. 271. Mr. Karr's implements were found in two localities, viz., on the right bank of the Issutugan in Somaliland, 85 miles S.W. of Berbera, and in the Waddi-el-Sheik in the desert some 30 miles east of the Nile.

the value of the surface finds above the valley, and in the desert, we are met with the objection that there was nothing to have prevented a flint worker from walking up the cliff with his flints and depositing them on the surface of the hills, at any period in the history of the valley. We must continue to rely on the *gisement*, as others have done before, and I have little doubt that it will be forthcoming hereafter, although the surface of the desert does not appear to favour the formation of sedimentary deposits. Mr. Seton-Karr informs me that the flints discovered by him were found lying bare on the surface of the top of a hill, from which the overlying earth had been denuded, probably by rain in ancient times. If so, the earth or sand so denuded must have been washed down, together with some of the flints, to a lower level, probably towards the bank of a stream or river which I understand passes through the ground on which the flints are distributed. It is possible that such flints may hereafter be found covered by deposits which had accumulated over them, and even that it may be possible to find with them the bones of extinct animals, which would at once show their place in the history of the district.[1] When we reflect that the first dawn of history in Europe is quite recent in comparison with the earliest dates that can be fixed with certainty in the history of Egypt, the importance of great precision in researches of this kind, in that country, is obvious, and this is no doubt the reason why those who uphold the

[1] Mr. Seton-Karr, in a brief but valuable paper, since published in the *Journal of the Anthropological Institute* (August, 1897), says that the implements of true palæolithic type and large size were found in one spot on the right bank of the Issutugan. The smaller implements of neolithic type were found all over Somaliland and also on this spot, but not in any quantity, and all were surface finds. One implement figured by him from the flint quarries of Egypt is of distinctly neolithic, and rather an advanced neolithic, type; and he says that many of the types of implements from these flint mines are new to science. So that as the evidence stands, it would seem as if a transition might be traced in the same sites from the palæolithic to the neolithic types. This, however, although to be expected in a late palæolithic site, is not characteristic of the implements of the river-drift in Europe, in which the implements are all of early palæolithic type, without any admixture of later forms, so that judged by form alone, if form alone were reliable, the evidence of date is as yet inconclusive. An implement of palæolithic type was found in the ditch of Wor Barrow, in deposits which would lead one to attribute it to the Bronze Age, or it might perhaps be neolithic, showing that the finding of such a form must not necessarily be taken as evidence of a Palæolithic Age. Another hand-tool, with part of the outside coating of the flint evidently left to serve as a handle, was found at the bottom of the ditch of Barrow 29. Both of these are figured in my fourth volume.

so-called Chronology of the Bible, are extremely jealous of any evidence which tends to prolong the time between the Egyptian monuments and the first appearance of man in the Nile Valley. The necessity that every discovery should be dated by its sedimentary deposits and by its fauna is clear, and on the other hand, the greater the time, the greater the probability of finding such differentiation in the fauna of the two periods, as can be relied upon for the determination of the periods. Meanwhile, a model of my finds at Gebel Assart with the tombs and the stratification of the gravel, the original flakes and cores and a rough tool found in the deposits, has been placed in my museum at Farnham, Dorset; so that any visitor who has sufficient knowledge of the subject can estimate for himself the value of this part of the evidence relating to the antiquity of man. Mr. Seton-Karr has examined this model, and I have reason to think that when he returns to Egypt, which he intends to do in a few months, he will endeavour to obtain further evidence of the character that I have indicated, the importance of which he appeared himself to be well aware of. Since writing this I have seen the following remark on this subject by M. Salomon Reinach in *L'Anthropologie*, Tome VIII., No. 3, p. 327:—"Tant qu'un gisement n'est pas caractérisé et daté par la faune, il ne présente, pour la paléontologie humaine, qu'un intérêt très restreint. Il faut attendre que de nouvelles recherches nous aient appris si les outils paléolithiques de l'Égypte sont vraiment comparables à ceux de l'Europe et de l'Amérique."

I have been rather tardy in replying to the remarks of Dr. Dawson, but it was only quite recently that I heard of his paper. I have not been in the habit of reading regularly the *Journal of the Transactions of the Victoria Institute.* I have no doubt that valuable papers have been read before that Institution, but as a rule, I think, it has been generally admitted that some of its publications are not entirely free from theological bias in questions that have a bearing on the antiquity of man.[1]

[1] "A tendency to cut down the lapse of time necessary for all the changes that have taken place in the configuration of the surface of the earth and in the character of its occupants, since the time of the Palæolithic gravels, still survives in the inmost recesses of the hearts of not a few observers."—Presidential Address by Sir John Evans, K.C.B., to the British Association at Toronto, 1897.

Since that time, ill-health has prevented me from going to Egypt, and I have been obliged to confine myself to excavations nearer home, but the matter is in good hands, and I have no doubt will be worked out thoroughly. I shall, therefore, make no further apology for devoting the remainder of my address to excavations in entrenchments in Wilts and Dorset.

The former area of Cranborne Chase affords happy hunting ground for the prehistoric archæologist; for although the whole extent of it did not belong to one owner, agriculture was a good deal impeded by it all over, and many of the antiquities owe their preservation to that cause. This is important, as the determination of the age of prehistoric works of the Stone, Bronze and Iron Ages, depends entirely on the identification of small relics, such as fragments of pottery, household utensils and such like objects; and the larger the number of them found embedded in the earthworks, the greater the facility for observing the transition from one period to another. It is also of great advantage to confine oneself to a limited area, for although Roman pottery and Roman relics were pretty much the same all over England, the earlier British pottery and utensils varied a good deal in different districts, and the forms recognised in one place, do not necessarily suffice to identify those of the same age in a different district. Many of the earthworks were occupied successively by different people, and where this has been the case, the transition of the periods is shown by changes in the forms of the objects discovered; so that a thorough knowledge of each period is necessary to distinguish the different people that lived in the same camp or entrenchment. This, for the reasons here given, can be better done in a small homogeneous area than in a large one. My three previous quarto volumes of excavations in this district were devoted to villages of the Roman Age, and tumuli of the Bronze Age. The fourth volume, now about to be issued, relates chiefly to camps of the Bronze Age and to a single long barrow of the Stone Age.

Whilst others have been occupied with the examination of the towns and military works of the Roman Age; Silchester, by a committee of the Society of Antiquaries,

the Roman Wall of Northumberland by a committee of North Country antiquaries, my attention, with the exception of Bokerly and Wansdyke, has been given chiefly to an agricultural district of the same period and the Bronze Age. Both are of equal interest. From the richer and more populous localities objects of greater intrinsic value and more advanced art might be expected, but from the poorer agricultural regions not less valuable evidence of the social condition of the settled mass of the population of the country may be obtained. Moreover, in such a region, the succession of the periods may be better shown.

Our knowledge of the Bronze Age in this country appears to be more limited than that of the other periods, for although we have an immense amount of information, derived from grave-mounds and relics associated with the dead, very little investigation has been made into the habitations and camps of that age or the Stone Age. Dr. Anderson in his work on *Scotland in Pagan Times,* 1886, speaking of the Bronze Age, says, "not a trace of a dwelling of the Bronze Age has been discovered in Scotland," and again in his preface to the Stone Age he says, "As I have before had occasion to remark of the Age of Bronze, I have now to repeat with respect to the Age of Stone, that there is no vestige of a dwelling or defensive construction, which can be proved by evidence to have been the work of the men of the Stone period." I observe also that Mr. Boyd Dawkins in his opening address to the Antiquarian Section of this Society at Scarborough in 1895, after giving an account of the work of the past year in other branches, says, "Our knowledge of the Bronze Age in Europe has not been greatly enlarged by recent discovery." Again, with respect to square-shaped camps of the Bronze Age, Mr. G. de Mortillet, in giving an account of the rectangular Bronze Age terramares of Italy, in which he divides them into "habitations terrestres" and "habitations lacustres," says of the distribution of such camps in Europe generally: "Les habitations terrestres ont été fort peu remarquées. On n'en a signalé qu'un petit nombre. C'est une lacune à combler. Il y a beaucoup à faire dans cette voie."

These and other remarks of the same purport that I have come across, are enough to show that the oppor-

tunity of examining a series of square-shaped camps, now proved to be of the Bronze Age, was not to be lost sight of, and having several near Rushmore, on my own and my neighbour's property, I decided to excavate them thoroughly and ascertain their age.

The square or rectangular camps here described and detailed in my fourth volume, are four in number. (1) The South Lodge Camp in Rushmore Park; (2) a small square entrenchment on Handley Hill; (3) the Angle Ditch on Handley Down; and (4) the Camp on Martin Down, 2¾ miles to the eastward of Handley Down.

The South Lodge Camp.—This is an entrenchment, about ¾ of an acre in area, which, at the time I first saw it, was of very low relief, the ditch having completely silted up, and the rampart become denuded by age, but the ditch was found on excavating it to be 6·6 feet deep beneath the silting. I dug six sections through the ditch and rampart at first, which, proving insufficient, I dug the whole camp—ditch, rampart, and interior space—completely over. The result of this second digging was very satisfactory. At the bottom of the ditch, in positions which showed that they were deposited when the ditch was open and had been just dug, I found a bronze razor, so-called, a bronze awl, and an urn of the Bronze Age. A little higher up, about 3 feet from the surface, I found another bronze razor, a bronze bracelet, and a bundle of bronze wire, all recognised as being relics of the Bronze Age and figured in my fourth volume. All above that was Roman. The ditch was 6½ feet deep all round, and the lower half of it appears to have filled up before the Romans or Romanised Britons occupied it. A bronze looped spear-head was found quite near the top of the silting, adjoining the Roman deposits. The age of these looped spear-heads is uncertain, but there is no doubt that they are of late Bronze Age, which accounts for this specimen being found so high up. Nothing of iron was found below the Roman deposits. In the body of the rampart, nothing but Bronze Age relics and pottery were found. The rampart having, of course, been thrown up at the time of the first construction of the camp, nothing was able to penetrate into it subsequently. This and

other excavations in this district show the importance of digging the whole of a camp over down to the undisturbed soil. The most erroneous conclusions may be come to by confining the explorations to sections only, and many of the most important objects may be missed. The description of the excavation of this camp was first published in the *Wiltshire Archæological Magazine*, Vol. XXVII., p. 206. A detailed account of it, with contoured plan and sections and drawings of the objects found, is given in my fourth volume of *Excavations in Cranborne Chase.*

Handley Hill Entrenchment.—The square entrenchment on Handley Hill included only $\frac{1}{4}$ of an acre of land. It was more regular in form than the South Lodge Camp, and in lower relief, the crest of the rampart being only about 0·8 foot above the level of the ground, and the ditch shallow in proportion. The contents of the entrenchment, after digging it all over, were found to consist of Bronze Age pottery only, with a little of Roman Age at the top, a well-formed flint celt in the centre of the interior, and a leaf-shaped arrow-head close to the outside. A silver denarius of Trajan was found on the old surface line beneath the rampart. This, if the rampart had been higher, would have conclusively determined it to be of the Roman Age, but the very slight depth of the soil above the coin, made it doubtful whether it might not have worked down from the top at a period later than its construction. I consider this camp, therefore, to be Bronze Age or Early Roman. The sides and angles were more regular than in the other camps, and I should not be surprised if this were found hereafter to be the chief distinction between the square-shaped Roman and the square-shaped Bronze Age camps.

Angle Ditch, Handley Down.—The ditch of the "Angle Ditch" was of nearly the same dimensions as that of the South Lodge Camp, but there was no rampart. It was on the grass-covered down, and was only discovered by hammering with the flat side of the pick on the grass. The sound of a blow on a grass-grown surface is hollow over an excavated and filled-up excavation, and this sometimes affords the only means of tracing such works on the

downs. The ditch made a right-angle turn near the centre of its length, and appears probably to have guarded one corner of a rectangular enclosure, the position of which could be seen on the ground. It was filled like the other ditches with chalk rubble near the bottom, mixed silting half-way down, and mould at the top. At the bottom, in the chalk rubble, where they must have dropped in when the ditch was open, were found, portion of a bronze palstave, part of a bronze razor of the form common in these parts, a British urn, and a sandstone rubber. Higher up, at 1·5 feet from the surface, portion of a very well polished stone hammer, with a cleanly-bored hole in it; all recognised relics of the Bronze Age. The pottery of the Bronze Age was all at the bottom of the ditch, and the Roman and Romano-British pottery in the surface mould only, extending to 1·2 feet from the surface. A great part of the oblong area, of which this ditch appears to have marked one corner, was strewed with pottery of both the Romano-British and the Bronze Ages.

Martin Down Camp.—This was a rectangular enclosure, containing about 2 acres. It was situated in a slight depression commanded within bow-shot by the ground outside. The ditch and rampart were of the same form as in two of the other camps before-mentioned, the ditch being 12 feet wide at the top and 9·8 feet greatest depth. The rampart on the inside was of very slight elevation; there was a gap on the north-west side 172 feet in length, with neither ditch or rampart, which, together with the large size of two other openings on the east and south sides, makes me think it was chiefly used for pounding cattle, though no doubt intended also for defence. The silting of the ditch was the same as in the other camps, consisting of pure chalk rubble, 5 feet, at the bottom, above which was mixed silting 3·3 feet, and mould for 1·5 feet at the top. Two bronze implements were found, both in the chalk rubble at the bottom of the ditch, viz., a bronze awl and the stem of a bronze razor. The pottery consisted of Bronze Age fragments at the bottom and Romano-British pottery only in the mould at the top. The mould, in this case, had begun to collect over the silting previously to the Roman Age. It appears probable that a spring in the shallow bottom of the Combe

formerly existed close to the camp, which supplied the inhabitants and perhaps their cattle with water. Romano-British and Bronze Age pottery was found in the interior, the whole of which, with the ditch and rampart, was trenched over like those of the other camps. British pottery only was found in the rampart, except a few fragments of Romano-British quite at the top. The small number of Bronze Age relics found in this camp gives force to the suggestion that it may have been a cattle station, and not thickly occupied by men, although the pottery proves that it was inhabited. The small number of implements may also have arisen from the necessity of saving every fragment of bronze in the shape of a broken or damaged tool for re-casting, evidence of which necessity is shown by a large hoard of damaged bronze implements found near Donhead, Wilts. Some of these are but very slightly injured, which shows that they did not delay melting them down, as soon as they became the least unserviceable. These were mostly large celts, containing a good deal of metal, whereas it may be noticed that the implements found in the ditches of the camps consisted chiefly of small tools, such as awls, bronze wire and razors, the blades of which were very thin, and which would make only a very slight contribution to the melting pot.

The people of the Bronze Age boiled their food with red-hot flints, like those of the Roman Age, the number of burnt flints being about equal to those of the Roman stations, and in the ditches of the camps, the Bronze Age deposits produced an equal number of these flints to those of the Roman deposits above them.

Wherever burnt flints are found in abundance on the surface, they nearly always indicate the presence of pits or habitations beneath the soil. One point, however, deserves attention. Sir John Evans, in his latest edition of the *Stone Age*, makes the remark that flint implements are not as a rule found to any extent amongst the relics of the Bronze Age. It is quite possible that in the tumuli of this district, I may have included some as Bronze Age, that were in reality of the Stone Age. All the tumuli of the Bronze Age do not contain bronze implements, and it is sometimes difficult to decide to what age a round barrow may belong; but I think there

can be little doubt that flint implements were used to a considerable extent by bronze-using people. One observation has, however, been made in these diggings, which appears to corroborate, to some extent, Sir John Evan's statement, viz., that in the Roman deposits in the ditches of the camps, that is, in the surface mould, the number of flint flakes greatly exceeded those found in the Bronze Age deposits beneath, although the number of small fragments of pottery enormously predominated in the Roman deposits.

In the South Lodge Camp, being the first Bronze Age camp excavated, the flakes in the several deposits were not counted, but this was done in the two camps dealt with afterwards. It is difficult to account for. We know that the Romans used flint flakes in the *tribulum* for threshing out corn, and they may perhaps have used them for some other agricultural purposes. The circumstance has been noticed by others as well as myself, and appears worthy of being recorded.

The flakes, though all having bulbs of percussion and facets, were generally of a rough kind, not such as would have been struck off for knives and implements by the skilful flint workers of the Stone Age. Had they been wasters, the *debris* of a flint workshop, the cores would have been found with them, but these, and flint implements were absent in the Roman strata. The flakes must have been imported as such, for a specific purpose. The same inference must be drawn from my finding 445 flint flakes together in a very restricted area on the old surface line beneath the rampart of Winkelbury Camp (*Excavations*, Vol. II., p. 239). I picked out a number of flint flakes together from the exterior slope of the main rampart of Maiden Castle near the old surface line. There is no reason to suppose that any part of Maiden Castle is of the Stone Age. It has occurred to me from the finding of so many flint flakes in the ditches of these entrenchments, whether they might have been used for defence, as we use the broken fragments of glass bottles. It is a mere suggestion. The discovery shows that rude flint flakes, when found in the fields on the surface, must not be regarded as certain evidence of the Stone Age. Nothing but long experience and careful observation will suffice to throw light on this point, in the future.

I have found very little trace of an Iron Age in this district. I excavated some time ago the camp at Mount Caburn, near Lewes,[1] which produced relics of the late Celtic period, similar to those since found by Mr. Bulleid at Glastonbury, but little of that period has been found here. It must have existed at the time of the first construction of the Romano-British villages recorded in the first three volumes of my work. The iron door-keys found in all these villages and figured in those volumes, belong to that period. I have found no trace anywhere of pottery with late Celtic ornamentation on it. The ribbed pottery discovered by Mr. Arthur Evans at Aylesford, is found in the Romano-British villages, but rarely. A late Celtic fibula was discovered in the Romano-British village of Rotherley, *Excavations*, Vol. II., Plate XCVII., Fig. 5, but all the other fibulæ were of Roman form. Enamelled brooches were found in Woodcuts and Rotherley, but none in the Bronze Age entrenchments. The custom of depositing their refuse in circular pits, 4 to 10 feet deep, was common in the Romano-British villages, and I found them also in the late Celtic camp of Mount Caburn, but not a single pit was found in the Bronze Age entrenchments. The practice of burying the dead in these filled-up refuse pits and ditches in Roman times has brought to light a large number of skeletons of Romanized-Britons in Woodcuts and Rotherley, which shows that they were undoubtedly a long-headed race and very short, ranging from 4 feet 9 inches to 5 feet 7½ inches in height for the males. The skeletons of the Bronze Age were found only in graves and were round-headed and taller, but only three were found that could be measured. This is not enough to determine by itself the stature of the Bronze Age people of this district, but as far as it goes, it confirms the researches of Dr. Thurnam in the neighbouring district of Salisbury. Sir Richard Hoare, who excavated such a number of tumuli in this district, unfortunately took no notice of human skeletons, by which omission not only was the important evidence of race afforded by them lost, but it was destroyed for ever. This shows how careful we should be to record

[1] *Archæologia*, Vol. XLVI. p. 423.

everything. I have twice been offered by neighbours permission to dig upon their property, on condition that I would not disturb the human bones or rebury them immediately. Of course I refused to avail myself of permission so hampered with unscientific conditions. This excessive reverence for bones of hoary and unknown antiquity is a great hindrance to anthropological science. The interesting questions of race can only be studied by careful measurements of the bones and skulls, and the preservation of them, if possible, in museums for future reference.

Every entire animal bone found in all the excavations has been measured and the depth recorded, with the result of showing that the small ox, about the size of our Kerry cow, standing 3 feet 5 inches at the shoulder, largely prevailed in both ages. Out of the enormous number of bones discovered, only two were found in the ditch of Wor Barrow, giving a size equal to our Pembroke bull, 4 feet 10 inches at the shoulder, and one in Martin Down Camp estimated at 4 feet. This shows that a large ox of some kind did exist in the Bronze Age, but was not common. It is seldom that, in excavations, horn-cores or other bones showing the peculiarities of the breed are discovered. But the size can always be ascertained with accuracy, and this I have done by means of test animals of modern breeds killed for the purpose of comparison, the measurement of the bones of which are given in my first and second volumes. The sheep in both ages was a small slender animal equal to a St. Kilda ewe, standing 1 foot 8 inches to 2 feet 1 inch at the shoulder. The horse in both ages was a small animal of the size of our New Forest pony, standing 12 hands 3 inches at the shoulder. From the number of bones, it appears to have been eaten in Roman times; but judging by the small number of the bones found, it was not eaten in the Bronze Age. The pig was a small slender animal in the Bronze Age, which surprises me, as I have always supposed that the early pig was a long-legged animal. The pig in the Romano-British villages of Woodcuts, Rotherley and Woodyates, was about the size of our modern pig, viz., from 2 feet to 2 feet 4 inches at the shoulder. The dog in Roman times varied from the size of a retriever to that

of a small terrier, but there is a difficulty in distinguishing dog from fox in some cases.

This part of my excavations in the entrenchments of the district, proves that the people of the Bronze Age certainly did live as they did in Italy, in enclosures of squarish shape and slight relief. They were probably strengthened by stockades on the banks, without which they could hardly have served for defence, but probably their chief use was to keep off wolves and wild animals. They must have been a pastoral people having flocks and herds, and there are not wanting indications that they may also have cultivated the soil in fields from the prevalence of lines of terrace near them, but on this subject I had rather withhold my judgment, as no sound argument can be based on proximity in this matter. The water supply in these camps appears to me to be defective, but there is clear evidence that in a chalk district the water formerly ran out much higher in the combes than now, and spots now remote from water may have had springs close to them. I have proved this in one instance by clearing out a Roman well 188 feet deep at the Romano-British village of Woodcuts, and finding the ironwork of a bucket at the bottom, but no water; and the water in modern wells on the same hill is lower. The position of the Bronze Age camp on Martin Down, in a slight hollow, shows that it must have been selected chiefly for shelter and the vicinity probably of water at that time, and not exclusively for defence. The positions of the South Lodge Camp and Handley Hill Entrenchment, close to the summits of hills, but on one side of them, were probably chosen for the same reason. Had defence been their only object, a command of view on all sides would have been very important. The villages of the Romanized-Britons in this district, though surrounded by banks and ditches of nearly the same size as those of the Bronze Age (not quite so deep), were of curved outline, forming irregular rounded enclosures. We must not assume that because the Bronze Age camps here described were of small size and squarish form, all Bronze Age camps were the same. These may have been chiefly for agricultural purposes, whilst the defensive camps and fortresses were different. The exploration of a large camp, when it comes to be

done, will be a work of great time and expense, if it is done thoroughly, and if it is not done thoroughly, it is better not attempted.[1]

The next division of the subject in the papers upon the table (exhibited at the meeting of the Institute) relates to the excavation of a single long barrow, called Wor Barrow, on Handley Down. It was the largest barrow in this part of the country, and was surrounded by traces of a broad ditch. I removed the whole body of the barrow, and on the old surface line beneath it, was found a trench cut in the solid chalk, enclosing an oblong area 93 feet in length by 34 feet in width. The trench was filled with large flints, and photographs were taken, of the soil above, showing marks of stakes sticking into the trench. The flints were for wedging down the stakes. I think that this was a wooden version of the long chambers of stone found in barrows of the same kind in places where stone was more plentiful. In the primary interments within this enclosure on the old surface line and to the south of the centre, six skeletons were found huddled together beneath a small mound of brown mould, three in sequence, crouched on the right side, and three put in with them as bones, with the long-bones laid out by the side of the skulls. I have never found any clear trace of cannibalism in this district, but the custom of burying human skulls and bones with the dead, and bones buried separately but not in sequence, appears to have been frequent. It is not easy to account for this. A recent discovery made by Mr. Petrie in Egypt, and figured in the *Illustrated London News* of the 17th July, 1897, showed that a skeleton of the 5th Dynasty, about 3500 B.C., was found, the bones of which had been cut up and put in a box, with an effigy of the deceased by the side of it. Something of this sort may have occurred here. No relics were found with the primary interments except a small piece of rude pottery accidentally put in. The absence of relics is frequently the case in long barrows; so that the whole of the evidence of this being a barrow of the Stone Age rests upon the form of the skulls and the measurement of the bones. The skulls show that they were a very long-headed race.

[1] The word "camp" is used in this paper, in its accepted archæological sense, as an enclosed entrenchment; it is not strictly correct.

The skulls are all photographed, but photographs cannot be relied upon for measurement. I have contrived a craniometer, which is exhibited at the meeting, which shows the profile of the skulls with perfect accuracy, the lateral measurements being given on the side elevation at the spots where they were taken; the measurements of the profiles are from the *meatus auditorius*. Four of the primary interments here were people of very small stature, but one was a tall man. The estimation of stature of all the skeletons is taken according to Dr. Rollet's method. Dr. Garson, our best English authority on this subject, has checked, in fact he has recalculated, all my measurements and found them correct. It is impossible to over-estimate the importance of anthropometry in all these enquiries. The ditch of this large barrow, which was 13 feet deep in some places, was thoroughly excavated and the silting examined. The body of this, or any other barrow, gives only relics of the age of the barrow: the ditch gives a record of all subsequent ages, during which it was gradually silted up. It will be seen by the average sections exhibited, that the Roman Age was confined to a thin film of Roman pottery and relics close to the top in surface mould; beneath these were relics of the Bronze Age and neolithic period. The secondary interments in this barrow were probably of the Roman Age, and were found extended near the top of the barrow and in the ditch. A Roman coin of Constantine II. was found near the forehead of one of them. Of these secondary interments, ten had been decapitated, two of which had the heads touching the fingers of the left hand, as if laid down after decapitation by the sides of the bodies. I think the top of the barrow may have been used as a place of execution in Roman times.

Close to this long barrow were two round ones, both of which are mentioned as having been opened by Sir Richard Hoare, but I found relics in both of them. The ditches of the round barrows on this Down were of two kinds. In one, the ditch had been irregularly dug, merely to obtain earth to form the barrow, without regard to its shape. In the rest, the ditch had been symmetrically formed in a true circle. I have no means of knowing

whether these two kinds mark different periods in the history of barrow making. It is worth noticing that no round barrow has been found in my district without a distinct ditch round it, except one in Susan Gibbs' Walk, Rushmore Park, which had the smallest mound found by me in this district. In other parts of England it has been supposed that this was not the case, and that the earth was brought from a distance. To what extent the ditches may have been overlooked in cultivated soil I am unable to say, but I have no doubt that in some soils this may very likely have been the case.

It is noteworthy in connection with this subject that the only four graves found by me with the so-called drinking vessels in them, had no ditches or mounds, except the one above mentioned, which had a very slight mound. The first was that found in the centre of the Romano-British village of Rotherley, described in *Excavations*, Vol. II., p. 5. It was only discovered whilst trenching the surface of the interior of the village. It had no mound over it, or ditch round it: the drinking vessel was at the knees. I believed it to be a Bronze Age interment of previous date to the construction of the village. The second was that in Susan Gibbs' Walk, above mentioned. It was in a clean cut grave, 3 feet deep. The skeleton was crouched, and the drinking vessel at the feet. It had no ditch, and the mound is described by me as being so slight, that it was hardly noticed on the surface, *Excavations*, Vol. II., p. 22. The third was the one on Handley Down, between the Angle Ditch and Wor Barrow. It was in a cleanly cut grave, 3 feet deep, with no ditch or mound, the skeleton crouched, with the drinking vessel at the feet; it was only found accidentally in trenching the surface. The fourth was that described in *Excavations*, Vol. III., p. 240; the drinking vessel, by itself, was found in a cleanly cut grave, 3 feet deep, with no mound or ditch, and no bones, and was only found accidentally whilst digging a hole for sand. Of the three skeletons found in these four graves with drinking vessels, two had very round heads and are typical Bronze Age skulls. The head of the third was longer.

There is no knowing how many of these graves

without mounds or ditches may exist in the soil; as they show no mark on the surface, they can only be found accidentally. They are consequently a class of interment of which we have very little knowledge and I am not aware of their having been found persistently in any other district. The drinking vessels are of the same form, quality and character of ornamentation as those found by Canon Greenwell in the Yorkshire Wolds; he says they are usually found with skeletons and not burnt bodies.

It is noticed that in my district small fragments of pottery of the drinking vessel type and quality, are found with other Bronze Age pottery in the ditches both of the camps and the tumuli. Fragments of pottery with chevron patterns on them are also found in the ditches of both camps and tumuli, which affords evidence of their being probably of the same period. As the camps are proved to be of the Bronze Age, those tumuli which have no bronze implements in them, but which contain chevron patterns on their pottery, are shown by this to be of the same period. The distribution of chevron patterns all over the world is traced in considerable detail in my fourth volume.

One round barrow on Handley Hill consisted of a small mound with a distinct ditch round it and a causeway of undisturbed chalk across it; the graves in the barrow appear to have been rifled and the contents taken out; but fifty-two secondary interments were found buried in urns or marked by fragments of British pottery just beneath the soil on the level ground outside the ditch on the west side. These were probably graves of the family or descendants buried near the grave of their chief. Had it not been for the method I have adopted of contouring the ground and trenching the whole of the area included in my plan, these secondary interments must inevitably have been overlooked.

The use of carefully made models of excavations are of the utmost importance in museums. There are as many as 118 models of different kinds in my museum at Farnham. I have found that by means of them, I can explain the nature of the evidence that is relied upon, in a few minutes, whereas it would take visitors a long time to acquire the same information from written descriptions

with plans and sections, if it could be done at all. Rows of relics arranged in cases, without regard to their *gisement*, are of no use for educational purposes, and I can hardly doubt that models will be largely used in future, both in general and local museums, more especially in those localities in which the excavations have been made. My models are of well-seasoned mahogany and are carved from the contoured plans. Carpenters should be trained to the work; my estate carpenters are so used to it, that I have only to put a contoured plan before them on a proper scale, and they will cut it out with the utmost precision; but of course I supervise the construction of the models very closely.

One of the most instructive parts of these excavations, and one which might eventually enable some idea to be formed of the length of time that has elapsed since the construction of these works, is the study of the silting of the ditches. I exhibit three sections of the ditch of Wor Barrow, taken on different sides, showing the denudation and silting that has taken place since the ditches were cleared out and my excavations abandoned in 1893. Since then the ditches have been left untouched and exposed to the atmosphere. In the centre of the ditch the chalk rubble has risen 2½ feet, consisting entirely of pieces of pure chalk, similar to what was found in that place at the time I re-excavated it.[1] Seams of mould are seen in this rubble formed by turf mould fallen from the edge on the top of the ditch, similar to those which, in the old silting, had lost colour and been compressed by moisture and the superincumbent weight of the silting above them. The sides of the chalk rubble had covered the undisturbed chalk sides for a space of 8 feet from the bottom, leaving 6 feet or more still exposed to the action of rain and frost, which in time will further disintegrate it and add to the size of the talus, and then the whole of the sides being sheltered completely from those influences will cease to fall down, and the chalk rubble in the ditch will become stationary at the same height that I had found it; and the mixed silting, consisting of finer chalk mixed with mould and produced by other causes, will supervene; finally the mould will be added to it at

[1] This was explained by sections in the illustrated copy of my address.

the top as before. The top of the ditch meanwhile had enlarged about a foot and a half in width on each side during the four years that it had been exposed to the air. The new talus was produced by precisely the same causes as the old one.

This investigation shows that all the fragments of pottery that had been found by me in the old chalk rubble were of the period of the first construction of the ditch, or within a year or two after it, and it accounts for so little having been found in this part of the silting, which had accumulated very rapidly. The hard sides of the ditch had assumed a convex form owing to the lower parts of them having been covered up and sheltered from the frost and rain much sooner than the upper parts. This convexity had already begun to increase as shown in my new section, and will continue to do so until the whole of the sides are covered up by the new talus, possibly a period of some five or six years, an infinitely short time compared with the whole time necessary to complete the entire filling of the ditch. The fine mixed silting above the chalk rubble was not formed by the disintegration of the sides, but by the accumulation of fine sediment washed down from the barrow or blown in from the surrounding country. Lastly, when the grass had begun to grow upon the barrow, the ditch and the surface of the downs around the barrow, all silting would cease and the mould would begin to grow by the decay of vegetable matter, and this, judging by the evidence of most of the ditches, could not have taken place until a comparatively short time before the Roman occupation. Finally, the pure surface mould, without stones or any other substances, must have grown some six inches in the centre of the ditches after the people of the Roman Age had ceased to deposit their relics and pottery.

By the same reasoning, we may assume that the smaller Bronze Age ditches of the South Lodge Camp, the Angle Ditch and Martin Down Camp, must originally have been a foot or two narrower, and the convexity of their sides must have been due to the same causes. The ditches in the case of these camps may have been kept open, and even their depth increased for defensive purposes. It is also possible that the convex sides may to some extent

have been adopted intentionally to increase the width at the top and the depth at the bottom; but still, as the deposits show that they silted up gradually and were not filled up, the pure chalk rubble found in the bottom of all of them must have been due to the disintegration of the sides, and the objects found in this rubble must all have been deposited soon after their first construction, or after they had been left to the erosive force of the atmosphere. Whether any estimate can ever be formed of the time necessary for these deposits to accumulate appears possible, but doubtful. That the ditches must have silted up in a constantly and greatly diminishing ratio appears certain; and the surface mould, owing to the greater luxuriance of the grass caused by the greater depth of the mould and greater moisture in the centres of the ditches, must be constantly increasing at a greater rate than on the surface of the country generally.

In the examination of the ditches of camps and barrows, attention should be given to the method of conducting the excavation. The most natural way, the easiest, and the one generally adopted is to dig down to the bottom in one spot, and then work out the ditch horizontally all along. This frequently leads to error in assigning the fragments of pottery and relics to their proper *gisement.* Fragments from the top fall down and are picked up on the bottom, to which part they are often erroneously attributed. The proper way is first to take off the turf over the whole area that it is intended to excavate, and then work down from the top in a succession of spits; in this way the pottery and relics from the upper spits are removed and recorded before the lower spits are dug into, and no mistake as to the depth of the objects can possibly occur.

The practice of contouring the ground before excavation is of great importance, both in barrow and camp digging. In a properly conducted excavation the whole of the surface soils and deposits have to be disturbed, and contouring is the only means by which the original shape of the work can be recorded. I think that no ancient earthwork should be excavated without preserving a record of its original shape. Contours are necessary to record the direction of the drainage of a village or camp,

a matter of no slight importance in those pluvial ages, and one entirely affecting the formation of the sedimentary deposits, on which the evidence of age so greatly depends. Contouring is indispensable in the case of camps and other defensive works, to show the command of ground, and whether the work is or is not a defensive one, as in the case of the Martin Down Camp, in which it was shown by contours that it was slightly commanded on all sides from the outside. By means of contours, a section can afterwards be drawn of any part of a camp, and in any direction. It is also necessary in some cases to show the likelihood of a spring having existed within, or close to, an earthwork, which is also exemplified by the Martin Down Camp. Contoured plans are indispensable for the construction of models. The contours were very carefully surveyed with a spirit level on the ground. It is neither a very difficult nor laborious process. I have restored all my camps on the ground to their original forms before leaving them; and my usual copper medalet, shown in my first volume, p. xx, has been put into all my diggings.

I regret that it has been impossible to distribute the detailed account of all these diggings to more than a few of the members.[1] If I had done so, there would not have been time to read them. These portions of my future fourth volume are intended to verify the statements made in my address, and will be of use chiefly to those who are actually engaged in archæological excavations. They are not intended for casual readers. The record of an excavation takes about five times as long as the actual digging. The relic tables given in my fourth volume, and in the previous ones, are very useful as recapitulations of the things discovered, without trespassing upon the text. Being in a tabular form, they enable the enquirer to ascertain in a few moments every object that has been found in all the different parts and deposits. Everything has been recorded in this way, however small and however common. In the same manner, there has been no selection in my fourth volume of objects for illustration; everything has been drawn, down to the most minute fragment of pottery that had a pattern on it. Common things are of

[1] Fifteen copies with elaborate illustrations were distributed at the meeting; all are included in the fourth volume of *Excavations in Cranborne Chase.*

more importance than particular things, because they are more prevalent. I have always remembered a remark of Professor Huxley's in one of his addresses. "The word 'importance,'" he said, "ought to be struck out of scientific dictionaries; that which is important, is that which is persistent." Common things vary in form, as the idea of them passes from place to place, and the date of them and of the places in which they are found, may sometimes be determined by gradual variations of form. There is no knowing what may hereafter be found to be most interesting. Things apt to be overlooked, may afterwards turn out to be of the greatest value in tracing the distribution of forms. This will be admitted when it is recognised that distribution is a necessary prelude to generalisation. I regret to find in endeavouring to trace the distribution of patterns, that archæological societies illustrate fewer things than formerly. It is thought, perhaps, that when a form has become common, there is no use repeating or even recording it. This is a great mistake in my opinion, and there is no excuse for it, now that illustration has become so much more economical and so effectual. The illustrations need not be elaborate, but sufficient to trace the transition of the forms. If ever a time should come when our illustrated newspapers take to recording interesting and sensible things, a new era will have arrived in the usefulness of these journals. The supply, of course, must equal the demand, but the demand shows what intensely stupid people we are. People bowing to one another appears to form the staple of these productions, as if it were not bad enough for those who are compelled actually to take part in such functions. Field sports are no doubt things to be encouraged, but can it be necessary to have a picture of a man running after a ball upon every page of every illustrated journal in this country? Let us hope for evolution in this as in all other things.

The compilation of a work of so much detail necessitates the employment of clerks. I make it a rule that nothing in the letterpress should be issued that is not in my own writing, and of course I am responsible for the whole. But the calculation of the numerous and tedious indices, the compilation of the relic tables, the photo-

graphs, the identification, measurement and restoration of the skulls, bones and pottery, the surveys, the contouring, careful labelling and correction of proofs, printing of the tickets, the collection, arrangement and description of the indispensable museum, the drawing of the plates and the close supervision of the workmen on the ground, requires the assistance of at least three men of different qualifications. Living in my house they must necessarily be men of good character as well as energy. Those who have left me have generally obtained more lucrative employments. This, of course, entails loss of time, and the training of the fresh ones from the very beginning. This part of the subject is important for those who may contemplate a similar method of conducting excavations. I shall allude to it in my fourth volume, as I have done in my previous volumes. As a rule I have been well served by my clerks.

There are many subjects connected with these excavations that I have been unable even to touch upon in this address. The amount of work still before the future of anthropologists is enormous, when the camps and habitations of prehistoric men come to be gone into. So far from barrow digging and camp excavation having been worked out, as I understand some persons have asserted, it has hardly yet commenced upon a thorough system. But when we consider the rapidity with which ancient earthworks are being destroyed, the utmost care is necessary, not only in preserving, but in examining them. If I were asked to give a title to this paper, it would be "A plea for greater precision and detail in excavations." It has lately been said in one of our leading journals that my methods are too detailed for public funds. I certainly have never had the slightest wish for the assistance of public funds; quite the contrary, I should find them an impediment. But if this is true, which I do not think it is, it only shows, in my opinion, that public funds ought not to be used for the purpose, but that the work should be left to be done by private individuals like Canon Greenwell, whose pupil I originally was, as a digger, who are devoted to these most interesting investigations, and who have the leisure, the knowledge, and the means to do the work thoroughly. I am informed

that there are people who think they are doing good by digging and grubbing out antiquities, without making any record at all of their investigations. I think that a landowner, if he is not sufficiently interested in these matters to work himself, could do no better service to archæology, than by prohibiting the investigations of any one, without obtaining some security that they will be well recorded. A discovery dates only from the time of the record of it, and not from the time of its being found in the soil. In conclusion, I have only to acknowledge my short-comings. Notwithstanding the care that I have taken to omit nothing, I am aware that my investigations fall short of what they ought to be, and probably will be, in the future. But I hope that in so far as regards my own and my neighbour's lands, to whom I am indebted for permission to dig, and in all that concerns the Bronze Age and the square-shaped camps of that age, it will be admitted that Mr. de Mortillet's "lacune" has been fairly well filled up in the particular class of antiquities to which this paper relates.