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Source: *The Journal of Egyptian Archaeology*, Vol. 8, No. 1/2 (Apr., 1922), pp. 5-12

Published by: [Egypt Exploration Society](#)

Stable URL: <http://www.jstor.org/stable/3853619>

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THE ANTIQUITY OF EGYPTIAN CIVILIZATION

BEING A PLEA FOR SOME ATTEMPT TO FORMULATE THE LAWS WHICH
SHOULD FORM THE BASIS OF ARCHAEOLOGICAL ARGUMENT

BY PROFESSOR T. ERIC PEET, M.A.

ARCHAEOLOGY can in no sense be termed an exact science, that is to say, its conclusions rarely follow with mathematical certainty from its premises, and indeed but too frequently they do not rise above the level of mere nebulous possibilities or probabilities. This state of things is partly to be accounted for by the very nature of its subject matter, but also, in the opinion of the writer, by the fact that archaeologists have hitherto made no attempt to come to any kind of agreement as to the conditions which must be satisfied by a train of archaeological reasoning in order that it may acquire cogency. We are doubtless all to blame in this, and in our defence it can only be urged that the constant accumulation of fresh material has tended to distract our attention from a really critical use of the evidence already available.

It may perhaps be replied that all are aware of this, but that on such questions as, for instance, the single or multiple origin of certain customs and discoveries, agreement is impossible, some minds being so constructed as to postulate single origins, others multiple. This view doubtless contains a measure of truth, but to make further use of the same example, it is undeniable that on the matter of fact in any particular case one of these types of mind is right and the other wrong, and we are not so pessimistic as to the limits of human reasoning powers as to wish to believe that some guiding principles could not be enunciated after collaboration between scholars which would enable a fair measure of certainty, or at least a very high measure of probability, to be arrived at in some instances. Thus it might be fruitful to discuss whether or not a custom which is a natural one and answers to some obvious and definite need in the development of man is more likely to have arisen in several places independently than a custom which seems to answer to no physical or mental need and to be a pure freak. Is it, for instance, not possible that while the use of copper or of picture-writing was discovered independently in more than one place, the practice of making gold *lunulae* of a particular shape and design had a single origin, and that, in consequence of this, when we find copper or picture-writing in use in two places A and B which are far apart we are not justified in assuming any connection of trade or race between them, whereas if we found these same *lunulae* in both we should have a very strong presumption for assuming a connection? Is it not further possible, in some cases at least, that a custom or use might lie between these two extremes, and that while we should not be prepared to say that it could have occurred to but one people and at only one time, yet we find it unlikely that it should have arisen independently in a very large number of localities?

Whether or not the suggestion made in the above paragraphs is in any way practical, and whether any positive results are likely to be arrived at on these lines it would be difficult to say. It is, however, beyond doubt that some negative results, to use a para-

doxical term, would emerge. It would, in other words, be generally agreed that certain types of archaeological argument, so far from arriving at certainties, do not even establish probabilities, and that they should therefore be dismissed as sterile.

We propose to examine in the light of what has been said a particular piece of archaeological reasoning, partly because if it were sound we should be forced completely to revise our ideas concerning the late palaeolithic periods, still more because it is an admirable example of a type of argument with regard to which archaeology will have to make up its mind if it is to advance.

Six years ago Professor Flinders Petrie published two highly interesting articles in *Ancient Egypt*¹, in which he strove to establish a date of roughly 8000 B.C. for the earliest predynastic graves in Egypt. Those who are acquainted with Professor Petrie's work (and who is not?) will hardly need to be told that he has always been an upholder of very high dates for Egyptian civilization, and one is not surprised to find him tacitly assuming a date of 5500 B.C. for the beginning of the First Egyptian Dynasty. It is true that he stands almost alone in this estimate, for most Egyptologists prefer to think of Menes as coming to the throne more than 2000 years later than this, and even Borchardt's ingenious but probably unjustifiable manipulation of the Palermo stone fragments² fails to push the date back much beyond 4200 B.C. For the moment, however, we may waive this point, for Petrie's *terminus a quo* is, as an illustration of the principles under discussion in this article, more important than his *terminus ad quem*.

In a recent volume called *Prehistoric Egypt* Petrie has still further elaborated the conclusions reached in the articles above quoted. His main arguments are four, and we shall deal with them in turn.

The first is as follows. He begins by assuming 5500 B.C. for the close of the predynastic period. He then says "looking at the proportion which the number of graves bears to those of the historic ages, it seems that the rise of that civilization is not likely to have been later than 8000 B.C." Here we have at the outset an excellent instance of a line of inquiry in our opinion absolutely sterile. In the first place it is manifestly almost impossible to make any estimate of the number of graves which existed in Egypt either in the predynastic or the historic period. There are too many unknown factors in the situation, the number of graves of either period destroyed in past ages, the number "excavated" by archaeologists and never recorded, the number still not discovered; all these things would force us to treat any figures given as unworthy of serious consideration. Still worse, even if we could with a considerable degree of probability establish that the number of predynastic graves was equal to that of the graves dug in any period of 2500 years of the historic era, we should have no right to draw the inference that the predynastic period lasted about 2500 years and therefore began about 8000 B.C., for we have no means of deciding to what extent the population of Egypt remained stable over those periods, or whether, supposing it to have varied, the variations chance to cancel each other. There are three times as many graves dug in England in ten years to-day as there were a few centuries ago for the simple reason that the population is three times as great.

Now we find in *Prehistoric Egypt*³ that the figures on which Petrie bases his conclusions are not estimates covering the whole of Egypt, or even a large part of it, but figures taken

¹ 1915, pp. 59-76 and 122-135.

² *Die Annalen und die zeitliche Festlegung des alten Reichs der ägyptischen Geschichte*, Berlin, 1917.

³ p. 4.

merely from "the group of cemeteries extending over about eight miles recorded in *Diospolis*," that is to say from the graves, 2050 in number in all, examined by a single expedition in one particular season. Are we to believe that in about 12,000 years (to take Petrie's own dates) only 2050 people have died in that eight miles of land, in a country where the death rate is high, and, if this is not the case, can we assume that the proportion of historic to prehistoric burials would be the same among the undiscovered or plundered tombs as in those which Petrie has excavated? Obviously not. The new discovery of a historic cemetery of 500 graves would destroy the whole argument. And in any case it would be quite possible to pick out tracts of country where the same type of reasoning would give precisely opposite results.

Petrie's next argument is geological. He points out that geologists have devised a method of determining approximately the age and the rate of formation of rocks by means of their helium and lead constituents¹, and that the average rate of formation is between 100 and 200 feet of thickness of rock per million years. Moreover the average rate of denudation of the earth's surface varies from 700 to 7000 years for a foot. These figures cannot be directly applied to fix the age of the predynastic period in Egypt, for we cannot bring this short period into temporal connection with any of the appreciable geological changes in the Nile valley except the depositing of Nile mud². There is, however, a possible indirect application, for, working on such figures as these, certain geologists have assigned to the *magdalénien* period in Europe dates ranging from 20,000 B.C. down to 10,000 B.C. or even later, and if we could find a temporal equation between predynastic Egypt and the *magdalénien* we could then use the geological figures to date the former. As will be seen below, Petrie believes that such an equation can be established. The validity of his argument from geology is thus dependent on the soundness of the reasoning on which he bases this equation, and may therefore be dismissed for the moment.

His next argument is based on the rate at which the Nile deposits mud in its bed. Assuming that the deposit amounts to 5 inches per century, the deposit must have begun somewhere between 5000 and 13,000 B.C. As "the deposit was probably slight to begin with, it is reasonable to credit an age of 8000 or 10,000 B.C. for the beginning of cultivation and the rise of the prehistoric civilization." These figures, especially the last, certainly invite criticism, but it is hardly worth while to attack them, since the argument makes another assumption which in itself is sufficient to vitiate the conclusion. The assumption lies in the temporal equation concealed in the words "the beginning of cultivation and the rise of the prehistoric civilization." By the "beginning of cultivation" is meant, as may be seen from the sentence quoted above, the time at which the Nile valley first contained sufficient deposit of fertile mud to be cultivable; but this is a very different thing from the moment at which it was first actually cultivated. A land may wait ten thousand years fit for cultivation before a people acquires the knowledge wherewith to cultivate it. The argument is therefore fallacious.

But there is worse to come. Petrie's next argument is as follows: "There are two well-

¹ See for example JOLY, J., *Radioactivity and Geology* (London, 1909) and HOLMES, ARTHUR, *The Age of the Earth* (London, 1913).

² Surely the complicated geological changes (huge alteration in river level, silting up and scouring out of the great valley at Thebes, etc.) which Petrie equates with an average denudation of 4 feet of land surface and compares with the 20,000 years allotted by some to the *magdalénien* period are, on his own showing, all anterior to the predynastic period, and, as such, irrelevant to the argument.

marked periods, or different civilizations, in the prehistoric graves. Now the average life of a civilization in Egypt is 1300 years, and so two cycles would imply a length of 2600 years on an average." This gives a date of more than 8000 B.C. for the beginning of the predynastic period. It is hardly necessary to point out the futility of an argument of this type. Who is to say what constitutes "a civilization"? It is true that we do, for purposes of convenience, divide Egyptian history up into periods to which we give distinguishing names. It is further true that the divisions which we adopt are in some cases fixed by external or internal events of great importance. But even were it a fact that the historical period in Egypt naturally fell into periods of 1300 years in length (and, be it noted, distinguishable from another by such marked characteristics as to constitute separate "civilizations") this would be an interesting coincidence, but would hardly justify us in making the general statement that in Egypt civilization must run in cycles of 1300 years, and concluding that in the predynastic period it must have done the same, so that the length of that period must be two cycles¹.

Such then are Petrie's arguments for the dating of the predynastic period back to at least 8000 B.C. Three of these involve ordinary logical fallacies, but the fourth is of greater interest for it bears directly on the purpose of this article. He has accepted the geological dating of the *magdalénien* period in Europe as somewhere between 20,000 and 10,000 or even a little later, and in order to date the predynastic tombs back to that period he must demonstrate their contemporaneity with the *magdalénien*. This he proceeds to do by attempting to show that the great periods of the European palaeolithic age are represented in the same order in Egypt. With the *chelléen*, *acheuléen* and *moustérien* periods we are not here concerned. Our interest is rather in the later periods, the *solutréen* and the *magdalénien*.

All Egyptian archaeologists are acquainted with the so-called Fayyûm flints, which on the authority of de Morgan² mainly come from near Dimê and Kôm Ashim. The flints have unfortunately mostly been brought in by natives and no accurate record of the finding of any of them exists. They are said to be gathered on the surface and it is not stated that pottery or other objects have ever been found with them, though this is purely negative evidence. These flints Petrie would identify with those of the *solutréen* period in Europe. It is true that he is able to point to a series of parallels between the two groups which, when illustrated on paper, are very striking. But to what exactly does this amount? In both cases we have the products of a very high standard of flint-working. The method of flaking is therefore very similar in the two cases, and the forms evolved are much alike, for the simple reason that they are designed to serve the same purposes. But surely this does not constitute identity, nor even contemporaneity. When we speak of *solutréen* products in France we refer to certain implements of flint, found associated with the bones of certain animals, notably the horse, the reindeer, the mammoth and the cave bear, and frequently in a determinable relation to other deposits, as in the case of Laugerie Haute, where the *solutréen* deposit lies beneath a very definite *magdalénien* stratum.

In the Fayyûm these conditions are not satisfied. All that we have is the flints themselves and the knowledge that they are picked up on the surface of the soil. It would be

¹ In any case it is difficult to find in Chapter XIII of *Prehistoric Egypt* any authority for the division of the predynastic period into two "civilizations."

² *Recherches sur les origines de l'Égypte, L'âge de la pierre et les métaux*, pp. 72-76. Also in his *Ethnographie préhistorique*, p. 28.

easy to show how a judgment by flint forms alone might lead us astray. Thus the leaf-shaped lance-head so typical of the Fayyûm and of Solutré occurs in various forms at Breonio and Rivoli in North Italy, in a stratum which is quite definitely neolithic. In any case, while noting the similarities between the Fayyûm and Solutré, we ought also to notice the differences, and not shut our eyes to the fact that among the commonest flints in the Fayyûm are arrowheads of most varied forms, while at Solutré there is no evidence that the bow was known.

So far then we have nothing to support the temporal equation of the Fayyûm with Solutré except the occurrence in both places of certain types of flint implement.

Now there exists in Egypt certain evidence which bears very closely on this problem and which has nevertheless been curiously neglected. In 1911 the Egypt Exploration Fund excavated at Abydos a prehistoric settlement¹. The remains consisted simply of a thin stratum of dark sand about 30 to 100 cm. below the surface of the desert containing flints, potsherds, bone implements, pieces of bone, beads, etc. It is clear from the pottery that the settlement was still in existence in the later stages of the predynastic period, though it is not certain how far back it may extend. The most striking fact with regard to the objects found here is the extent to which they differ from the objects found in tombs dated by their pottery to the same age. In other words the conclusion forced on us by this and similar discoveries is that the objects found in tombs of predynastic date are not truly typical of those actually in everyday use among the living, the specimens chosen for burial being usually the best obtainable, sometimes even made for the purpose, while the majority of the objects used for rough work by the living were of types rarely, if ever, represented in the tombs.

There is an important corollary to this. When Petrie speaks of the absence of the Fayyûm flint types from the "cemetery age" of predynastic Egypt he is not altogether correct. It is true that they are absent from the cemeteries, but they are not all absent from the settlements of the same date as these cemeteries. This will become apparent to anyone who will compare the flints from Abydos² with those figured by Petrie³. Thus we have from Abydos the flakes worked to a point for boring (Petrie's Type B), the thick flakes bluntly pointed and with a rounded butt (Type E), the prismatic rods worked on all faces (Type G), the small curved knives (Type F, especially Fig. 91), the arrowhead (Fig. 118), and the round scraper⁴ (Figs. 165-168).

Moreover, other predynastic settlements exist besides that of Abydos. One of the most notable is that of Tûkh⁵, a village beside Naqâdah. De Morgan has described the kitchen-middens which exist at this place, and has figured a number of the flints found in them together with bones of animals and pottery "similar to that found in the archaic (*i.e.* predynastic) cemeteries." Many of the flints found in these kitchen-middens are practically indistinguishable from those of the Fayyûm, as an examination of de Morgan's figures will show. It is not improbable that the kitchen-middens of Khaṭṭārah⁶, which produced similar material, are also to be dated to the predynastic cemetery period, though de Morgan does

¹ *Cemeteries of Abydos*, II, pp. 1 ff.

² *Op. cit.*, Pl. III.

³ *Ancient Egypt*, 1915, figs. on pp. 73, 75, 77.

⁴ None of Petrie's examples is actually stated to be from the Fayyûm though the type does occur in other parts of Egypt along with flints of Fayyûm forms.

⁵ DE MORGAN, *Recherches sur les origines de l'Égypte, L'âge de la pierre et les métaux*, p. 87.

⁶ *Op. cit.*, p. 88. *Ethnographie préhistorique*, p. 39.

Journ. of Egypt. Arch. VIII

not give us definite evidence of this. At Naqâdah itself Petrie found a town of the predynastic period¹ in which, together with "pieces of almost every variety of pottery" known from the predynastic cemeteries, were found flints of the type under discussion. Petrie himself emphasizes their difference from the flints found in the tombs.

The lesson to be drawn from this is as follows. The Fayyûm flints are largely of a type which were being made for everyday use by the Egyptians who buried their dead in the well-known predynastic cemeteries of the Nile valley. On the other hand the fine leaf-shaped lance-head and many of the various arrowhead forms seem to be almost peculiar to the Fayyûm. This fact should deter us from any attempt to assign the Fayyûm flints *as a whole* to the period of the predynastic cemeteries. But at the same time the facts which we have put forward above make it very dangerous to assign the Fayyûm flints entirely to a period definitely preceding the predynastic, and to deny them any connection with the predynastic people of the cemeteries. The temporal equation with the *solutrén* in Europe therefore seems to us nothing more than a presumption, which may or may not contain a germ of truth.

Having equated the Fayyûm flint period with the *solutrén* in Europe Petrie proceeds to establish his next equation, that between the predynastic cemetery period and the *magdalénien* in France. And here he confuses the issue in a very curious and disconcerting manner. He says, with regard to the products of the predynastic tombs, "The main point to be observed is the close connection with the Magdalenian cave products, and the finest Danish work, suggesting that we may find some synchronism." Now even Petrie himself, who is interested in lowering the date of the *magdalénien* period does not suggest bringing it down below 6000, a date to which many geologists would demur most strongly, while the "finest Danish work" belongs to the Later Neolithic Period in Scandinavia, which is known to have closed little before 1500 B.C. and which, according to the best authorities, is not likely to have opened much before 3000, if as early, long after Petrie's *magdalénien* period in Egypt had come and gone. Under these circumstances it is a little difficult to see how comparisons with periods so remotely apart are likely to assist us in establishing synchronisms of any kind. Still we must see what the evidence amounts to.

The parallels with the *magdalénien* consist firstly of the similarity of "the coarse flakes which abound in the prehistoric graves to the Magdalenian cave type"; secondly of the remarkable resemblance of the neatly made double-ended scrapers of the First Dynasty to a scraper found in the deposit of the Grotte de l'Église in France, which is, be it observed, not *magdalénien* in date but *solutrén*; and finally of the fact that the early predynastic people of Egypt, like the *magdaléniens* of France, used bone harpoons. Surely this is thin ice. Rough flakes with wavy outline and slight chipping on the ends are to be found in practically every neolithic or late palaeolithic deposit known; the parallel of the scraper tells against Petrie's case, for it merely shows how exact a parallel can occur between two ages which on his own hypothesis cannot possibly be contemporary, namely the First Egyptian Dynasty and the *solutrén*, which preceded the *magdalénien*; and the bone harpoon is in use to-day among many primitive peoples such as the Esquimaux, the Fuegians and certain North American Indian tribes who live on the Pacific coast.

It will thus be seen that the case for a parallel between the period of the predynastic tombs and the *magdalénien* in Europe is not very strong. Petrie's parallel between the flints found in the predynastic tombs and "the finest Danish work" need not detain us long,

¹ *Naqada and Ballas*, p. 50 and Pl. LXXI.

since the hypothesis which alone lends any value to the comparisons, namely that "there would probably be no objection to dating the Danish work to 7000 to 6000 B.C. like the Egyptian," is completely at variance with the evidence of the tombs in which this finest Danish work is found. These tombs date from very late in the Later Danish Neolithic Period, verging on the metal age, which is believed to have begun in Scandinavia as late as 1500 B.C. In the Earlier Neolithic Period, the era of the kitchen-middens, they are not found.

The comparisons in themselves are far from decisive, though in both countries we find delicate ripple flaking side by side with "vague surface flaking or scaling," and though the method by which the Danes produced the criss-cross ridges on the handles of some of their daggers was also known to the Egyptians.

Quite lately Petrie has tried to justify this high dating by coordinating his own results with those obtained by de Morgan at Susa in Persia. In the lowest stratum of the mound at Susa, de Morgan found fine painted pottery associated with flints which Petrie states to be "clearly of the Solutrean types, like the Fayyûm flints of Egypt, especially the lance-headed flints, rather thick, with finely notched edges¹." Here again far reaching conclusions are based on a few parallels in flint-forms, the only one of which is at all striking is the leaf-shaped lance-head². It is true that this fits in with the very remote dating (7000 B.C.) at first suggested by de Morgan for this early stratum at Susa³, and subsequently by Pumpelly for somewhat similar pottery found in Turkestan. But this high date is based on nothing more than the facts that the civilization found in this stratum at Susa appears to be more primitive than anything yet known in Mesopotamia, and that the stratum itself lies at a depth of 25 metres from the surface. The first fact need hardly force us back far beyond 4000 B.C. and the second proves nothing, for every excavator who has any experience of stratified sites is aware that the thickness of strata is but a treacherous guide to the length of time which they represent, so various and complicated are the circumstances which govern the rate of accumulation. For this reason archaeologists as a whole are not prepared to treat as anything more than hypotheses, and rather improbable ones, the immensely high dates given by de Morgan and Pumpelly for the earliest remains at Susa and Anau respectively.

When therefore we find a few similarities in flint forms used as a basis for the categorical statement that "the Elamite civilization developed in the Solutrean Age⁴, a whole cycle before the Egyptian development in the Magdalenian Age⁵," we can only record the pious hope that in the near future we archaeologists may be able to come to some agreement as to the nature of our reasoning, which surely should be governed by precisely the same rules as in any other science, and yet apparently is not.

The fallacy latent in the line of argument above criticized would appear to consist in the assumption that certain types of flint must wherever found belong to a certain age in the world's progress. It is almost unconscious. We label the leaf-shaped lance-head *solutrén*

¹ *Ancient Egypt*, 1917, p. 32.

² *Délégation en Perse*, I, figs. 414-416.

³ PUMPELLY, *Explorations in Turkestan*, Vol. I, pp. 50-57. His date of 8000 B.C. for the beginnings of the Early Culture at Anau is based solely on the rate of accumulation of culture strata. Hubert Schmidt, the archaeologist in charge of the excavations, suggests a date of about 3000 B.C. or less, *op. cit.*, p. 186.

⁴ Petrie appears to have overlooked the fact that the "*solutrén*" lance-head at Susa does not occur in the lower levels of the earliest stratum, but appears along with the arrowhead in the upper levels in a full aeneolithic milieu (DE MORGAN, *Les premières civilisations*, p. 197; PUMPELLY, *op. cit.*, pp. 73-4).

⁵ *Ancient Egypt*, 1917, p. 36.

because admirable examples of it occur on true *solutrén* sites, and we then proceed, whenever and wherever we find this type of weapon, to date it to the *solutrén* period purely because of its shape, without asking ourselves whether such a date can be supported by the circumstances in which it is found, or the fauna and artifacts which accompany it. The more one studies the flint implements of all places and periods the more obvious does it become that they are dangerous things to argue from. Pottery is safer, but even here there are pitfalls, and it would save much wasted time and trouble if archaeologists would avoid describing two wares or types of ornament as identical when they only mean similar, and if they would lay aside the assumption that all white-filled incised wares either belong to the same period or have a single origin. The corrective in this case is undoubtedly more experience. If we all studied, for instance, early American and primitive modern pottery as we ought we should realize far better than we do how many resemblances are fortuitous which we now firmly think due to racial or commercial contact, and we should even perhaps be able to draw up certain general principles which would help us in interpreting our ceramic evidence, if only by teaching us what kind of arguments to avoid as delusive.