

Milton Núñez

MY PROBLEM WITH SUSILUOLA

Milton Núñez, Department of Archaeology, P.O., Box 1000, FIN-90014 Oulu University, Finland

This paper consists of two parts. It begins describing my first experiences with the Susiluola material and my reasons for abstaining from criticising its investigations until now. The second part discusses and presents my views on some of the issues in the ongoing Susiluola debate.

BEFORE 2002

I first learnt about the existence of the Susiluola Cave from a newspaper some time in the mid-90s.¹ There was a large reportage on the site with a photo of people excavating the cave and a close-up of something that resembled a chopper. The reporter explained the views of the researchers from the Geological Survey and the National Board of Antiquities about the possible traces of human activity in the cave filling. There was also a brief interview with the late Professor Ari Siiriäinen, who in turn dismissed idea of the lithic material from the cave being anthropogenic.

I was aware of Siiriäinen's knowledge of lithics and knew for a fact that he was not negative to the idea that humans could have reached Finland during previous interstadial/interglacial periods. We had spoken a few times about this at the Department of Palaeontology,² and during the 1970s he had even brought me a thermally fractured stone coated with a dark charcoal-like material that had been found deep below till in northern Finland. To our disappointment, however, acid treatment showed the black substance not to be charcoal.

Thus, based on Siiriäinen's opinion, I too dismissed the artefact nature of Susiluola's finds. I was content to put the matter off of my mind and turn to the more pressing task of creating a full-

fledged Archaeology study program at Oulu. But Susiluola did not go away. Some Oulu students that took part in the excavations kept speaking about it. I was of course interested. After all, I had done my MSc research on the sedimentology of a French Palaeolithic cave (Núñez 1972). This and, much to their surprise, that 20 years earlier I had worked at other French Palaeolithic sites and studied lithics with Professor François Bordes at Bordeaux came out during our conversations. That may have been the very reason why, while visiting the National Board of Antiquities late in 1997, I was approached by Hans-Peter Schulz and asked if I would like to see the Susiluola finds.

I was able to examine the material collected so far. The few dozen small pieces of sand/siltstone were ambiguous, of the kind that could have easily resulted from natural processes. I told Schulz so. I also said that, provided that the material was indeed anthropogenic, the absence of Levallois technique and the presence of 'notches' gave the assemblage a certain Clactonian aspect, which would mean a greater antiquity than his suggestion of 100,000–130,000 years, perhaps twice as much.³ He replied that similar industries dated to around the same period as the Susiluola deposits were known from Germany. I accepted this explanation since I was neither familiar with the German material, nor had I actively worked with the Palaeolithic for nearly 20 years.

Despite the ambiguity of the material, two items caught especially my attention. Both had irregular pseudo retouch features that were obviously fresher and due to secondary chipping by natural processes. I pointed to Schulz one that, though smallish, reminded me of Clactonian-type notches. Unfortunately, this item was not much

help in solving the question of human authorship. Such notches can be produced by impact in cave roof falls, in high-energy environments or through slow pressure in stony sediments – all of which were certainly connected with the history of Susiluola's sediments. It was nevertheless the closest to a recognized 'tool type' in the whole collection.

The second item, also seen as special by Schulz, was somewhat harder to interpret. It was a small elongated siltstone flake 3–4 cm long that, in addition to a small but clear percussion bulb, appeared to have been detached by a high-speed blow. This last feature was more difficult to link to natural processes and placed me in a dilemma: Regard the whole collection as composed by geofacts, as the great majority of the lithic collection suggested? Regard the high-speed scars on a single item as anthropogenic, implying that the rest of them may be as well? In the end, I did neither and opted to wait, hoping that future research would show the way. That was what I told the Scandinavian colleagues that asked me about Susiluola after Schulz's presentation of the site at the 1998 Nordic Archaeological Congress in Umeå. They were interested but cautious. At the Nordic PACT Symposium in Stockholm a few years earlier, Schulz himself had reported the discovery of the remains a Viking period fortification wall at Varikkoniemi, which had later turned out to be a misinterpretation.

Although I hoped to answer the question with new excavated material, I never got to see any. When in 1998, we drove from Oulu to Susiluola to see the site, excavations and new finds, we found that these had been taken to the bank valve on the previous day. Schulz apologized for having forgotten about my visit. However, according to my students working there, finds were usually taken to the valve on Fridays. Since my visit took place on a Thursday, they suspected that Schulz did not want me to see the new finds. I never sought to examine any Susiluola finds afterwards.

2002

For nearly five years the high-speed scars kept haunting me. Although the bulb could be related to an impact from falling debris and/or in high/middle energy shore environments, I had problems in finding a high-speed impact source. The

roof was not high enough for the needed speed on free fall. A roof block of the size required to reach the right momentum was not likely to have restricted its effect to such a minute surface (<1 cm³) and, moreover, it would have probably crushed any stone in its path. It was possible but very unlikely. The beach processes I had observed in the Åland Archipelago, even during storms, did not seem violent enough for the needed high-speed impacts either, and a search among the gravel of rocky beaches failed to reveal any such features. Nevertheless, even if all seemed very hard to explain and unlikely, I still felt I could not entirely rule out natural causes.

Then an explanation came unexpectedly, nearly as a revelation. While snorkelling at a rocky cove in the Mediterranean, I noticed a small opening by the water surface in the limestone cliff. I immediately recognized the formation from childhood experience: the mouth of a narrow passage that usually leads to a larger cavity. Exploration showed an approximately 50 cm wide and 3 m long corridor that opened to a roughly spherical gallery 7–8 m across. As in my childhood, I lied down on the sandy floor, legs in the water and eyes closed, listening to the waves' sounds in the grotto. I dozed off, to be awoken by increasing wave activity. While reflecting that it was time to leave, there was the rush of a particularly large wave followed by a loud bang behind me. I turned to the noise to see a still-moving stone. I just had stumbled into a process that could generate high-speed scars. I should have known, but had completely forgotten. The feeling was cathartic as I swam out of the grotto.

Up until then, whenever asked about Susiluola I had always said: 'Of all the items I examined, there was one that could not quite explain as the result of natural processes.' However, after the grotto incident I began to say that all items I had seen could be explained as the result of natural processes. Nevertheless, I still felt reluctant to completely discard the possibility of pre-Holocene human activity at Susiluola.

Why? It is difficult to find a concrete answer. It is true that I had not seen all the material. But perhaps I was not ready to give up the dream of pre-Holocene human occupation on Finnish soil. One thing seemed clear, Susiluola's antiquity made it an extremely important site in terms of Finland's and Fennoscandia's Quaternary Geol-



Fig. 1. Pavement formation in a rock cavity connected to a seasonal flood channel of the Oulanka river, Kuusamo, Finland.

ogy. The fact that it may hold traces of human activity raised its significance to European, even world level. I wanted the question of human activity to be conclusively solved once and for all. But even if no human traces were found, Susiluola would still provide an important window to Finland's Pleistocene environments. I felt that the site should be properly and thoroughly tested.

AFTER 2002

The long waited report of the investigations at Susiluola finally appeared late in 2002 (Schulz et al. 2002). The article prompted a certain degree of criticism, much of it justified. I must confess a certain disappointment when I read it in February 2003. I had expected more exact information on many central points. The briefness of what was said was frustrating, the manner in which it was said was sometimes irritating. The reader was given data and their interpretation with little or no proper explanation to support it, let alone any mention of alternative explanations. This applies

to such crucial subjects as stratigraphy, dating, alleged artefacts, and indications of fire. It is very likely that at least some of the criticism was provoked by this presentation style.

Yet, despite these obvious flaws, I chose not to publicly criticize the article or its interpretations, fearing that it could hamper the possibility of obtaining funds for further investigations at the site. I felt that once proper research was carried out at Susiluola, the anthropic/nonanthropic nature of the site would become evident. This is also the reason why I supported the applications for funds made for such purposes.

Unfortunately (or luckily) I was abroad on sabbatical when the *Tiedepäivät* discussions took place. It was not until 2007 that I finally got to read the papers concerning the debate raised by this event. However, there was one particular thing not related to the debate that made me change my attitude towards the Susiluola investigations: the newly published children's book *Elämä kivi-kauden Suomessa* (Purhonen & Miettinen 2006). It was undoubtedly a nice long-needed book tell-

ing with a few simple words and fine colour illustrations about Finland's Stone Age. It would have been an excellent and didactic children's book, but the two first pages depicted Neanderthal-like beings at Susiluola. This was too much. Despite the heated debate about the artefact nature of Susiluola finds, the book takes them for granted and goes on to place the makers of the presumptive artefacts at Susiluola in a book meant for children. This was unscientific and irresponsible to say the least. It was like an attempt to create another myth as the Hackman model, which could still be seen in school books in the 1970s (Hackman 1905; cf. Núñez 2002). As a consequence of this, Susiluola ceased to be my problem, though it certainly continues to be a problem in Finnish prehistory.

THE PROBLEM WITH SUSILUOLA

Much has been written for and against the interpretation of Susiluola in the last five years (e.g., Schulz et al. 2002; Purhonen 2004; Rydman 2004; Carpelan 2005; Forström 2005; Kinnunen 2005; Matiskainen 2005; Pellinen 2005; Pettitt & Niskanen 2005; Saarnisto 2005; Schulz 2005a; 2005b; 2005c; Donner 2006). However, with the exception of the article by Pettitt and Niskanen (2005), the debate has been carried out in Finnish and in local journals. Here I will attempt to address and comment some of the issues raised in the debate.

On expertise

I fully agree with Christian Carpelan (2005), there are no Palaeolithic experts in Finland. This is conceded by all involved, but some speak and act as if they indeed were. It is true that a few have studied Palaeolithic collections and even done fieldwork at Palaeolithic sites abroad, which has provided us with a good general knowledge of the subject. But we are not experts simply because we have not enough experience. If Susiluola's deposits contain the human component its investigators claim, then it is an extremely important site that calls for close collaboration with researchers/institutions with the proper experience. It is not enough to invite an expert for a couple days to look at the excavated lithic material at the National Board of Antiquities. There is even the risk that the well-treated guest would feel obliged to refrain

from too hard a critique and politely express ambiguous or not too negative views about what s/he has seen. Foreign experts should have taken active part in the Susiluola fieldwork. The investigations should have been carried out as a joint research project with European universities/institutions that possess the necessary expertise. Whether we like or not, we do need outside help in this matter. It is irresponsible to excavate such an important site as is claimed to be with our limited knowledge about cave stratigraphy, cave excavation techniques and Middle Palaeolithic lithics. That is precisely why we are where we are today.

Stratigraphy

The stratigraphical scheme of Susiluola as presented by Schulz et al. (2002: 13–19) seems somewhat simple for a cave sequence spanning over 100,000 years. Cave sediments tend to experience repeated phases of partial/complete deposition, erosion and redeposition, which may have been particularly complex due to the various glaciation, deglaciation and littoral episodes undergone by the Susiluola Cave. Whichever the reason(s), the vagueness of the descriptions and the continuous failure to provide adequate proof/support for the interpretations, it is very frustrating to the reader. The interpretation of the cave stratigraphy (Schulz 2002: 13–14) may be satisfactory, though it raises various questions about issues not addressed by the writers. The correlation of layer IV with the Karjenkoski paleosol is somewhat tenuous, as already pointed out by others (Matiskainen 2005; Saarnisto 2005; Donner 2006). It is certainly possible, even likely, but is by no means self-evident. More supporting arguments are needed, or at least the mention of alternative explanations and why these are less likely.

A similar problem lies in the statement that clay minerals and Fe-Al extracts indicate that layer IV 1 'was formed under more humid conditions than today' (Schulz et al. 2002: 14; see also Matiskainen 2005; Schulz 2005b). This may well be correct, but nothing more is said to corroborate the assertion, let alone the mention of whether cave microclimate and/or the length of pedological processes may have differentially affected the layer. Cave and open-air sediments are not subjected to the same environmental conditions and



Fig. 2. Naturally shaped 'core' of claystone (ϕ 9 cm) with multiple scars from impacts from falls and stream turbulence that resemble those of human workmanship. Found in a talus that is seasonally affected by arroyo erosion at a steep rocky promontory c. 1000 m a.s.l. in Morella, Spain.

processes. Moreover, there may be major differences even within the same cave. Such deficiencies in interpretation/explanation end up making the reader weary and suspicious.

The stratigraphy of the archaeological units – whatever they may represent – is still more problematic (Schulz et al. 2002: 16–17). It is difficult to see how on the basis of so little material and no structures, the authors could discern 'several occupations'. An even more puzzling statement refers to the so-called pavement: 'The pavement marks an interglacial floor of the cave. It is the only archaeological in situ horizon that contains lithic artefacts' (Schulz et al. 2002: 18). Such pavements are no more than an erosion surface (Fig. 1). The finer material of one of more layers has been removed by erosion, causing the stones in the washed deposits to 'deflate', generating thus the pavement. Consequently, few or none of the pavement stones are likely to be in primary position. Moreover, the pavement implies a hiatus. There need not be any connection between the *soil redeposited* of Layer IV1 (Schulz et al. 2002: 13) and layers IV2 and V beneath it. This conflicts with the authors' statement that Layer IV1 is 'part

of the same paleosol as Layer IV2, but has been redeposited in a probably littoral environment' (Schulz et al. 2002: 18). It makes little sense, and the authors even seem to contradict themselves later in the article (Schulz et al. 2002: 24). Finally, if the authors' reason for linking layers IV1 and IV2 is that both show evidence of pedological alteration, then the deposition of these two layers and the pavement between them must antedate the formation of the paleosol.

One also wonders why there has not been any attempt to see indications of eolization and/or beach processes on the surface texture of fine fractions (cf. Núñez 1972; Núñez & Alhonen 1974) from the different layers, particularly IV and V which are supposedly interglacial. I could go on with this, but the addressed issues suffice to make my point.

Chronology

Due to the mentioned reasons, the reader also reacts to the correlations and chronology advanced by Schulz et al. (2002). Nevertheless, even if one may question whether the sediments are from early, middle or late Eemian or a Weichsel interstadial, the TL and IRSL dates (Schulz et al. 2002: 15) suggest that we may indeed be dealing with rather ancient deposits. Although the pollen results are not very illuminating, they do not contradict the possibility of a pre-Holocene date. Nevertheless, several authors have raised serious questions against the chronological interpretation advanced by Schulz et al. (e.g., Matisckainen 2005; Pettitt & Niskanen 2005; Saarnisto 2005; Donner 2006). Here again more research is needed, regardless of whether there are anthropogenic deposits or not.

Lithics

Much criticism has been directed to the interpretation of certain lithic material regarded as artefacts by Schulz et al. (2002). This again may in part have to do with the vagueness of the presentation of the presumptive artefacts. For example, only 31 of the ca. 900 so-called artefacts are illustrated in the article and there is no mention of which layer or part of the cave these came from. The drawings are of good quality though, based on the lithic material I saw in 1997, there may be

an intentional/unintentional tendency to depict natural chippings as more retouch-like than they really are (cf. Donner 2006).

Of the few dozen presumptive artefacts I examined, none could be unquestionably and exclusively ascribed to human hands. All can be explained as the result of natural processes, even if the possibility of some of them being human-made cannot be discarded. Admittedly, I only observed about 10 % of the Susiluola material, but the items illustrated in the article (Schulz et al. 2002: 42–4), which are probably the most artefact-like, do nothing to change my mind.

It is difficult to see the items illustrated in page 42 as choppers and scrapers, and the same applies to the presumptive cores in page 43 (Schulz et al. 2002). Particularly the scrapers' 'retouch' is erratic and most likely due to natural processes. Moreover, I have observed similarly fractured rhyolite on Åland cobble beaches and claystone in continental arroyos (Fig. 2). They may impressionistically resemble choppers, scrapers and cores in the drawings, but they are not convincing as unquestionably made by human hands. Were it so, one would expect more regularity and uniform wear in the 'retouched' areas.

Perhaps one should take more seriously Kari Kinnunen's (2005) suggestion. In his answer to Kinnunen, Schulz (2005c) argues about the presence of platforms and scars that he interprets as reduction technique. The problem is that the shore processes affecting the Susiluola sediments may give rise to features similar to those generated by human reduction techniques. They may also make platforms, which then facilitate further flaking. The blow directions indicated in the 'cores' illustrated by Schulz et al (2002: 43), are not necessarily wrong, they are just not necessarily human made. After all, wave and ice action perform a reduction process as well. The main difference is that natural reduction processes are random, whereas human reduction techniques are selective.

Based on the little material illustrated, the reduction sequences presented by Schulz et al. (2002: 26, 28) seem to have little base on reality (cf. Matiskaïnen 2005; Pettitt & Niskanen 2005). Nor do the authors make their claims more credible by implying the position of illustrated presumptive artefacts in the reduction sequences. Moreover, despite the insistence of Quina-type

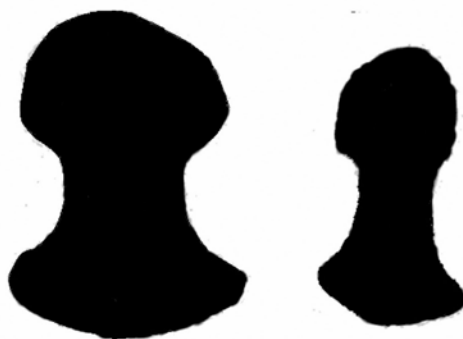


Fig. 3. Silhouettes of the phalanges from the fingertips of a Neanderthal (Krapina) on the left and a modern human on the right (after Putman 1988: 464).

reduction being used at Susiluola (Schulz 2005b), the illustrations of figures 13–14 (Schulz et al. 2002: 26, 28) as well as the illustrated cores (Schulz et al. 2002: 43) seem to lack low angle characteristic of this particular reduction technique. Furthermore, neither the location of Susiluola nor the date assigned by Schulz et al. (2002) fits very well with the chronological and geographical distribution of the Quina-reduction technique, which appears at the end of the Eemian (ca. 80,000 BP) and is, as far as I know, very much restricted to southern France.

Another feature that speaks against the human-made nature of the objects classified as artefacts is their small size. As pointed out by Pettitt & Niskanen (2005), it is difficult to think that such minute objects would have been made and utilized by coarse-fingered Neanderthals (Fig. 3). For example, half of the illustrated 'cores' appear to be less than 3 cm across (Schulz et al. 2002: 43, numbers 1–3, 9–11), which is close the width of a Neanderthal's thumb.

With respect to the question made by many critics of why use siltstone when better materials like quartz and quartzite were readily available? Although I am far from being convinced that the objects were made by humans, it would not be unlikely that accustomed flint workers would have preferred materials with similar fracture pattern. I say this based on my own experience. After having knapped flint for months in France in the early 1970s, it was very exasperating to work with quartz on my return to Finland. Instead

I found Åland rhyolite and siltstone much easier to work with.

Indications of fire

There are several mentions of ‘burnt stones’ in the article and the indications of fire are briefly discussed in half a page (Schulz et al. 2002: 18). Unfortunately, there is no mention of the criteria used to define such burnt stones. I seem to remember seeing a couple of thermally broken stones during my visit to the site, but thermic fracture need not imply exposure to fire. If these stones were indeed ‘burnt’, it would be interesting to know how it was determined that they had had a heating episode. This could be measured fairly easily. Furthermore, as pointed out by Heikki Matiskainen (2005), if they did undergo heating, one wonders why they have not been TL-dated. Even more puzzling is the following statement.

The only traces of fire are scattered burnt stones in Layer IV 2 at the border of the pavement and in Layer V at the back of the cave. These stones were clearly in a secondary position. Magnetic susceptibility measurements [...] showed a number of strong anomalies in the fine sediment near the burnt stones, which indicates campfires during the occupation of Layers IV and V (Schulz et al. 2002: 18).

It is difficult to understand why supposedly burnt stones that were in secondary position would have affected the surrounding soil (cf. Pettitt & Niskanen 2005: 84). Certainly, they had long cooled down before ending up where they were found by the excavators. Another important point to make is that, as I understand it, magnetic susceptibility refers to magnetizable rather than magnetized materials. The anomalous magnetic susceptibility detected in soil near the ‘burnt stones’ may well be related to iron minerals liberated through the weathering of the stones themselves. If the criterion for ‘burnt stone’ was their crumbly state – which need not stem from fire – then this would explain the reported anomalous readings.⁴ In any event, the results do not necessarily imply that such stones underwent heating in a human-made hearth.

One cannot but wonder why there has not been any attempt to extract, analyze and date the charcoal-like particles allegedly fixed to the cave roof.

A pre-Holocene date from these particles would not necessarily imply human presence, since they may have been carried into the cave by water/wind after a forest fire. Nevertheless, such a date would strengthen the controversial interpretation presented by Schulz et al. (2002).

Neanderthals?

Pettitt & Niskanen (2005) have discussed some of the problems related with a Neanderthal presence in Finland. Since I agree with most of their arguments, the only thing that remains to be said on the subject is the following. Had been humans in Finland 100,000–130,000 years ago, they may well have been Neanderthals. On the other hand, anatomically modern humans (AMH) with the same lithic technology as Neanderthals (Mousterian) were in the Near East in Eemian times (e.g., Bar-Yosef et al. 1992; Foley & Mirazón Lahr 1997). It is therefore feasible that some of the Middle Palaeolithic sites of the Russian Plain identified with Neanderthals on the basis of their Mousterian industry (cf. Hoffecker 1987; 1999; Dolukhanov et al. 2002; 2005; Kuzmin & Keates 2004; 2006; Pavlov et al. 2004; Anikovich et al. 2007) might contain in fact evidence of AMH occupation. In other words, we have little knowledge of the kind of hominid(s) that have left their early traces in the Russian Plain; and this would apply to any eventual Eemian anthropogenic in Finland. At any rate, as we have seen, the evidence for human activity in Finland during the last interstadial period is both slim and ambiguous. It is therefore wise to wait for more concrete evidence of pre-Holocene human activity before we dare say anything about Neanderthals being in Finland 100,000–130,000 years ago.

FINAL REMARKS

I have expressed my concerns about the interpretations of the Susiluola Cave by Schulz et al. (2002). It was not easy, mainly because I hated criticizing work that, despite its shortcomings, was carried out with the best intentions and abilities of those involved. Schulz et al. (2002) certainly have done us a favour by divulging their results and interpretations for our scrutiny. It must have taken courage to do so knowing that they most certainly would be criticized. Having said

that, it is clear that there is much left to be desired in the investigation, interpretation and reporting of the Susiluola Cave. Much may be blamed on inexperience of the investigators and their reluctance to collaborate with expertise-possessing institutions.

As a source of consolation and, perhaps, hope I would like to point out to them that archaeological data are always incomplete and archaeologists are bound to be sometimes right and sometimes right in their interpretations. For example, when over 80 years ago Bøe and Nummedal found a series of early sites in northern Norway with what appeared to be artefacts of an unknown industry, a French prehistorian labelled them as eoliths. Norwegian archaeologists continued their research undeterred and the questionable material came later to be recognized as the Komsa culture (Bøe & Nummedal 1936). The opposite happened in Finland when Ville Luho (1956) found a series of quartz sites in the Porvoo river valley over 50 years ago. Based on their position above sea level, he assumed correctly that they were rather old. However, Luho's limited experience in lithics made him see similarities between the Askola quartz material and certain late Palaeolithic flint forms, which led to the creation of the concept of a distinct Askola culture. This idea was later dismantled by Siiriäinen (1974) and we no longer speak of the Askola finds as a distinct culture. I mention these two examples to illustrate how it could go for the Susiluola finds in the future.

Unfortunately, like many of my colleagues, I fail to see today any conclusive evidence of human activity at Susiluola, neither in the alleged artefacts, nor in the supposed fire traces. This does not necessarily mean that there was not human activity, simply that there is no proof for it. But precisely because of this lack of proof, there is absolutely no justification for claiming a human presence in pre-Holocene Finland. To do so on the basis of flimsy evidence is both irresponsible and unscientific.

More data is needed to show that the Susiluola material is anthropogenic. However, like Carpelan (2005) I do not feel it is wise to resume fieldwork at Susiluola in the near future. There is plenty of work to be done in terms of laboratory analyses and dating of the already collected material, which should anyway be done before any more digging takes place. Furthermore, I do not think that the

prevailing polarized atmosphere will allow an impartial examination and judgement of new and old material from Susiluola. Those who have seen evidence of human traces would be looking for ways to prove their point, those that have opposed their interpretations would try to debunk both new and old evidence. When it is impossible to conduct an impartial trial in a given jurisdiction, the trial is generally moved to another one. Obviously, the Susiluola Cave cannot be moved to another country, but we can postpone its investigation – and destruction of its precious sediment archives – to another time, to the following generation of archaeologists. When those now involved in the controversy are no longer holding the reins, new investigations should be carried out by impartial Finnish archaeologists in collaboration with foreign scholars and institutions that possess expertise in the subject. Only then, I believe, could be determined with some degree of certainty whether the Susiluola Cave holds anthropogenic material or not.

NOTES

¹ I believe it was *Helsingin Sanomat* but do not recall the exact date, most probably in 1996.

² In addition to Siiriäinen, Professors Joakim Donner and the late Björn Kurtén were positive to the possibility of pre-Holocene human presence in Finland. It was in fact at Donner's suggestion that in 1973 I tested the possibility the 34,000 year old reindeer antler from Tornio being an artefact. Unfortunately, it was naturally shed antler (Núñez 1991).

³ I placed the date of the Clactonian industries roughly within 200,000–400,000 BP.

⁴ As I understand it, magnetic susceptibility has to do with the ease with which sediments are magnetized when placed into to a strong magnetic field, and it is much related to the presence of magnetizable mineral materials. For this reason, I fail to see how the magnetic susceptibility of sediment particles next to a stone would show that they or the stone were once subjected to high temperatures.