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DEBATING SUSILUOLA – SOME COMMENTARY

Contributions to the critical discussion about the Susiluola site in Western Finland have been published in two issues of *Fennoscandia archaeologica*; in 2005 Pettitt and Niskanen (FA 22) and in 2007 Donner, Kinnunen, Schulz, Manninen, Núñez and Junno (FA 24).

This article focuses on the critical comments published in *Fennoscandia archaeologica*, additionally it contains also several references to the former debate in the journal *Tieteessä tapahtuu* (2004–2007). The replies to critical comments deal with the significant arguments of each article; subjects referred to several times, such as the stratigraphy of the cave, the dating of the sediments, and the palaeogeographical environment are handled as a block in the end of the article.

One problem of this debate is that the critical comments are based on the preliminary publication of the results of the 1997–2000 excavations (Schulz et al. 2003), the answers also include data from the 2003–2006 excavations (Schulz et al. in press), which had not yet been published in 2007. Thus the argumentation is on an inadequate level. Hopefully the debate will continue after the new data are presented.

P. Pettitt and M. Niskanen: 'Neanderthals in Susiluola cave, Finland, during the last interglacial period'

The authors argue that the lithic 'artefacts' from layer IV:2 are natural inclusions, none of them is unquestionably man made, and that the excavations have not provided convincing evidence of Neanderthal inhabitation during the last Interglacial period (Pettitt & Niskanen 2005: 86).

The analysis of the Susiluola publication from 2003 presented therein is exhaustive, and considering that none of the authors got acquainted with the site or the find material, excellent and as antithesis in Hegel's sense welcome; but it also bears problematic features. The analysis showed insufficiencies in the presentations of the stratigraphy, dating, petrographic analysis, and the lithic material (e.g., lacking comparison to naturally cracked material) and was thus helpful; similar comments were also made by other colleagues. Insufficient, however, was basing arguments on references to articles in Finnish non-archaeological forums, without checking their scientific nature.

The analysis of lithic material based only on drawings without having seen the original pieces is problematic. A serious interpretation of non-flint lithics should be based on the study of the original material. Without knowledge of the fracturing quality, as well as experimental striking and retouching of these raw materials, any interpretation remains speculative. The authors propose that the fracture surfaces of the lithics presented are natural without any reference to the physical structure of the materials. In some cases, just short comments like 'These pieces are clearly natural' (Pettitt & Niskanen 2005: 85) are presented, as if it was obvious that no arguments are needed. However, any conclusion should base on arguments. Interpretations without having seen the piece can also be misleading: 'I.1:2 ...As the fracture mechanics of this material is hardly known, we suggest, that several of these [negatives] could have been removed in one kinetic event, as often happens on brittle coarse grained materials [...] and given the size of the piece (7 cm in maximum dimension) we question its efficacy as tool…' (Pettitt & Niskanen 2005: 85). The piece in question (KM 30301:1) is actually 10.9 cm in maximum dimension and its weight is 613 g. Precambrian sandstones found in the area are very tough, comparable in toughness to quartzite; this was testified by numerous striking experiments. Striking experiments have also been carried out later, and the results show that fine grained sand stone can be used as a raw material for tools; and, in fact, it has been in use on Neolithic Stone Age dwelling sites in northern Ostrobothnia (Hertell & Manninen 2005: Tabell 1; Hertell 2006: 79–80).

J. Donner: 'On the lack of evidence of artifacts in the Susiluola cave in Finland'

In his article, Donner (2007) focuses only on one lithic (KM 30301:10), which he claims to be a natural pebble. To underline his conclusions, he refers to Kinnunen (2005) and Pettitt & Niskanen (2005). The piece in question is shown as a photograph in Figure 1, the interpretation by Donner and by the authors in Figure 2. It seems obvious that geologists pay attention to different features than archaeologists. Additionally, the interpretation of the piece is complicated by the fact that its edges are rounded, which is a common feature in the Susiluola assemblage. It has to be considered that fracturing by a kinetic impact – natural or artificial – always produces sharp or splintered edges; the rounding of the lithics is caused by secondary processes. Donner understands the piece as a rounded pebble and states that 'no clear striking platform could be identified, nor any ripples at the surface.' To his opinion, all sides have similar surfaces with a chipped scar on one side (Donner 2007: 55–6).

However, according to striking experiments carried out with these raw materials, the piece shows clear striking marks on its ventral side. The scars on the slightly round edge start from a point located inside the edge, which indicates chipping by pressuring with a pointed tool. It is difficult to understand, how initial points of fracturing lying

Fig. 1. High-resolution photo of KM 30301:10.

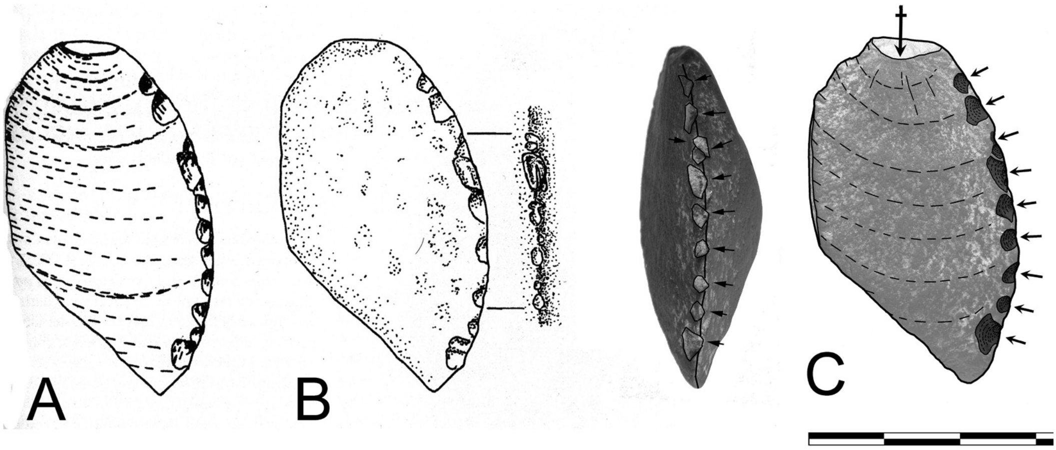

Fig 2. KM: 30301:10, a) drawing (Schulz et al. 2003) presented by Donner (2007); b) drawing by Donner (2007); c) interpretation by the authors (drawing on photo).

inside the edge could be produced by natural forces, for example wave action during a littoral phase, as Donner (2007: 54) claims. He also pays attention to the drawing of the piece presented in 2003 (Schulz et al. 2003), which shows more scars than in his own drawing. The drawing of the piece has indeed been corrected in 2005 after getting the possibility of high resolution scans, which allowed a better interpretation of the surface textures (Schulz in press; Schulz in prep; Schulz et al. in press). The result is, indeed, quite similar to the interpretation of the scars made by Donner, however, the retouching of the edge is very regular. There is only one clear scar on the dorsal side, and all ventral scars are, on the basis of surface textures, of the same age. The piece shows clear striking marks; it is a flake, not a pebble (Figs. 1, 2C). To our opinion, the arguments presented by Donner are not convincing. The striking marks, as well as the shape and the regularity of the edge, indicate human impact rather than a naturally cracked stone.

K.A. Kinnunen: 'Fractured siltstones in Susivuori esker close to Susiluola cave, Karijoki Finland'

Kinnunen (2007) presents a comprehensive study of silt stone occurrence in Susivuori esker near Susiluola cave, its petrography, morphology and cutting quality. In his critical view, he focuses on an argument presented by Schulz and others (2003) that siltstone is not recorded from outside the cave and therefore it must have been brought in to the cave by human action. He shows in his study that siltstone, also in fractured mode, appears in the esker nearby. As archaeologists, we have no competence to evaluate his study in geological or morphological terms.

One problem might be that the definition of rock species varies, depending on whether it was determined by an archaeologist or geologist. Kinnunen as a geologist is without a doubt using correct terms. Siltstone, as a quite new raw material for archaeology, was defined by the Susiluola team on the basis of special surface textures: somewhat shiny/fatty fracture surface that often showed ripples starting from the point of impact. It contains quartz grains <50 µm, hematite-pigments, and cryptocrystalline material (SEM/EDS-analysis by the Geological Survey of Finland). According to the analysis the term siltstone for this raw material is obviously wrong, but it was kept on because it had already been published under this name. The material defined as very fine-grained sandstone does not show clear ripples on fracture surfaces. A conical fracture with occasional flat bulbs and usually a small crushed area around the point of impact is a usual feature for this stone. Small mineral grains are visible under microscope.

The pieces presented by Kinnunen (2007: 59–63) resemble – judging from the photos – raw material that was defined by the Susiluola team as very fine-grained sandstone. To get forward in this subject, it would be good to check the raw materials together with Mr. Kinnunen.

But even in the case that siltstone – according to our definition – occured outside Susiluola cave, this would not question the artefact character of the finds inside the cave (see Schulz 2007). It should also be considered that Susiluola cave functioned as a 'sediment trap', and as such preserved preglacial sediments and lithic material. Then, the cave however would not have been the major place of human activities, but we could expect that the main activity took place on the terrace in front of it. Artefacts pertaining to this area were probably mostly destroyed by glacial forces – or transported to its surroundings.

In the paragraph on cutting quality, Kinnunen claims that siltstone is too soft for working on wood. Actually, special tools for woodworking appear first in late Palaeolithic/early Mesolithic context. With the exception of notched pieces, used for example for shaping spear shafts, we would not expect that Middle Palaeolithic tools were suitable for working on wood. However, for example fine-grained sandstone can be used quite well for cutting meat (cf. Hertell 2006: 78). One should also bear in mind that rock type should never be used as the only criterion when discussing about flaked artefacts and naturally fractured stones (e.g., Manninen 2007: 77 with references; Núñez 2007: 89–90).

M. Núñez: 'My problem with Susiluola'

Núñez's article (2007) reflects his own experiences with the Susiluola project divided into three time horizons: before 2002, 2002, and after 2002. Especially his passages describing discussions or situations are somewhat problematic – their original content cannot be verified any more. And how trustful are they really? At least two claims can be rejected on the basis of archived reports: the visit of Núñez at the cave site did not take place in 1998, and in the course of a field season,the finds were never taken to a bank valve.[1] The material of the earlier excavations that was stored in the bank was presented to Núñez in the National Board of Antiquities. Thus, we reject his idea that we did not want to show all the finds to him.

As the second subject of the article 'My problem with Susiluola', a critical view of the 2003 publication (Schulz et al. 2003) is presented, along with a kind of a summary of the debate about Susiluola.

In the paragraph 'On expertise', Núñez reproaches the missing experience of the excavation team and the missing cooperation with Palaeolithic experts and institutions. First, the excavation leader studied 1979–85 in Tübingen (Germany), participating from the very beginning in cave-excavations and working all these years with MP/UP lithic material. The crew of the Geological Survey of Finland that participated in the project has wide experience in the research of glacial sediments in Finland (cf. Hirvas & Nenonen 1987). Additionally, through the work lasting eight field seasons the excavation team has gained quite a lot of experience. It should also be remembered that Susiluola is up to now a unique case: a cave in Precambrian bedrock filled with glacial sediments. To our knowledge there has been no research in analogous sites, and for this reason there is no one who could have helped the team with this case of cave stratigraphy.

An international research project would, of course, have been the best solution, but even for the Finnish circumstances small research budget did not enable this. However, from the beginning the project has been in contact with experts and discussed issues concerning, for example, the lithic material, interpretations, excavation- and analytical methods. Intensive analytical conversations took place with representatives of the Universities of Köln (Germany), Liège (Belgium), Marseille (France) and recently Szczecin (Poland).

The international co-operation is passed over by Núñez with the following comment: 'It is not enough to invite an expert for a couple of days to look at the excavated lithic material at the NBA. There is even the risk that the well-treated guest would feel obliged to refrain from to hard a critique and politely express ambiguous or not too negative views about what he has seen'(Núñez 2007: 87). Here Núñez apparently points to Prof. Alban Defleur (Univ. of Marseille), who was invited to the Tiedepäivät (Days of Science) discussions in 2005. Defleur, however, was interested in the material already in 1998 and commented it critically (via Eirik Granqvist); in 2003 he came to Finland and studied the lithic assemblage of Susiluola cave.

In addition, Núñez ignores the fact that several Middle Palaeolithic experts have studied the material and commented on it, which has been referred to in the critical discussion (cf. Purhonen 2004; Matiskainen 2005).

For these reasons we reject Núñez's statement that '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' (Núñez 2007: 87).

There have been several problems with the preliminary interpretation of the cave stratigraphy presented in 2003, to which Núñez also pays attention. The research in 2003–2006 provided important new data about the stratigraphy and chronology. While these results will be published in other papers, a summary of the results is also presented in the 2006 excavation report in the archive of the National Board of Antiquities.

Núñez's critical review about lithics is a summary of the earlier discussion on the topic. An answer to this has already been presented in the previous issue of FA (Schulz 2007). Núñez pays special attention to partly small sized lithic material and presents an illustration of the silhouettes of the phalanges of a Neanderthal and a modern human. With this, he aims to demonstrate that Neanderthals could not have handled such small-sized lithics. However, numerous Lower and Middle Paleolithic assemblages include small tools, for example the Middle Paleolithic 'Taubachien' in Eastern Central Europe (e.g., Valoch 1984; 1988; Moncel & Neruda 2000); the average size of the tools is less than 3 cm. Who then was the author of those assemblages, if not the Neanderthals?

M.A. Manninen: 'Non-flint pseudo-lithics: some considerations'

Manninen (2007) quite correctly points out the fact that archaeological research in Finland has been almost devoid of artefact–geofact discussion. He presents basic data about fracture mechanics and pseudo-artefacts with good illustrations and comments the debate about the Susiluola lithics. The geofact–artefact analysis presented in the same issue (Schulz 2007) should enlighten this subject. His conclusion that evidence of the origin of the Susiluola lithics probably will never be reached is, however, to our opinion, quite pessimistic. We are convinced that the tools nowadays used for distinguishing between artefacts and geofacts (cf. Baales et al. 2000; Schmude 2004; Schulz 2007), also suit for the raw material of the Susiluola assemblage.

Commenting the questions of Pettitt and Niskanen presented in 2005 (FA 24)

1. Do the deposits of Layer IV 2 convincingly belong to OIS 5?

One problem of the stratigraphy of the cave presented in 2003 (Schulz et al. 2003) was the correlation of different layers from areas that were not any more connected due to emptying of the cave's central part in 1996. That problem could be solved with data of the excavations in 2003–2006. According to the recent results, layers IV 2 and VI are of the same age and genesis. The formation in question is a granulate pebble gravel that remained in the cave after the Eemian littoral stage. In the front part of the cave, the gravel was impregnated by a well-developed palaeosol. The layer contains marine diatom species which are recorded from the Eemian phase of the Baltic basin, but from no later period of the Baltic Sea. This is a strong reason to place the genesis of these layers into OIS 5. Layer V, which covers this sediment unit, shows clear signs of deposition by current and also contains the same diatom species as the layer below. This sediment block is covered by a glacial boulder belt and topped by littoral sediments from the Ancylus phase. Because no littoral deposition could have taken place between the Eemian and the Ancylus phases as the mouth of the cave is located at the altitude of 116 m a.s.l., the genesis of layer V has to be connected to the proglacial environment of the middle Weichselian glaciation. Thus, we have to expect, that the layers IV 2 and VI formed the floor of the cave for nearly 50,000 years – from the late temperate stage of the last interglacial, when the cave was uplifted over the Eemian sea level, to the first major glaciation of the middle Weichselian. For this reason, an accurate dating of the material buried in this sediment is impossible, at least at the moment.

TL- and OSL-dates of the sediments produced quite different ages. Several factors complicated the dating of the sediments. One such factor was the sedimentation process itself. For example, layer V contains materials of different origins and ages, while layers VI and IV 2 formed the floor of the cave for a long time and, thus, possibly contain fine-grained sands of different ages. The differences between TL- and OSL-results in some samples show that a part of the material was not bleached completely, therefore giving ages that are 'too old'. The main problem, however, is the coarse-grained sediment matrix with a grain size median between 0.6 and 2 mm. Fine-grained material with the size of ca. 200–300 µm, which was used for luminescence dating, occurs with a frequency of 0–4 %. Due to the coarse matrix, the likelihood of contamination with fine sand possibly even of recent origin is high. This was already noted in the pollen and clay mineral record; meteoric water and water dropping from the roof of the cave transported fine material from the top into the lower layers. Three dates obtained from the samples taken in 1997 were published in 2003; they range between 36–128 ka (IRSL) and 102–148 ka (TL) for the layers II, V and IV:2. Final results from the samples collected in 2004–2006 are not yet available. According to preliminary results, the 2004 series produced ages between 55–40 ka (layer VII ca. 40 ka, layer VI 45–40 ka, layer IV:2 49 ka and layer V 44–55 ka). The ages do not correspond to the stratigraphic sequence. This is even more conspicuous with the samples from 2005, measured on quartz grains: the layers VI 2 and VI produced ages between 16–21 ka and upper layer II an age of 48 ka. The final interpretation of these dates must wait until the report has been received. Another series of samples taken in 2006 and analysed in Riso, Denmark, gave results between 36–14 ka (Auri et al. 2008). With these results, we are facing two alternatives: 1. The results show the ages of the Susiluola cave sediments. In this case we should put into question the whole Weichselian main glaciation, because the majority of the dates fall into the LGM-phase; 2. The results do not show the ages of sedimentation; then, evidently, luminescence dating is not very suitable for cave sediments consisting of coarse material.

Contamination with recent fine-grained material is obvious, as is also the presence of only partly bleached material – a clear example is the date range of 148–14 ka from layer IV:2. The only clue concerning the age of the layers IV:2 and VI, and the age of roughly contemporaneous human activity, is that the radiocarbon dates for the ash lens in layer IV:L that covers layer IV:2 in the western part of the cave are older as the determination range of the ^{14}C-method (Hela-1087 > 40 ka, Hela-1105 > 40 ka), which gives a *terminus ante quem* (reference of all: Schulz et al. in press).

2. Was this presumed interglacial site, which may even have been located on a small Eemian Sea Island capable of supporting wildlife, let alone Neanderthal predators?

3. How did Neanderthals colonise this region?

After the melting of the Saalian glacier, Susivuori hill was for a short time an island in the Eemian sea, and due to the land uplift, it was probably a few thousand years later connected to the mainland. We have no exact data regarding the land uplift during Eemian interglacial, but comprehensive data about the land uplift during the Holocene; and we can assume that in both events the speed of land uplift was quite similar. After the melting of the Weichselian glacier, the area of southern Ostrobothnia was under water and Susivuori Hill itself was nearly 100 meters below sea level. About 8700 years ago[2] the hill formed a small island in the Baltic Basin. As the land uplift continued, this island became larger and was finally joined to the mainland about 6000–5500 years ago.

Fennoscandia was a large island at the beginning of the Eemian interglacial. During the late temperate stage, it became connected to the northwest Russian mainland as a result of land uplift (Miettinen et al. 2005). The beginning of the early Weichselian was marked by a glaciation of western and northernmost Fennoscandia around 115–110 ka (Mangerud 2004). Its extent in northern Finland is not known. During the following interstadial land uplift ceased approximately 20,000 years after the melting of the continental glacier (Fig. 3B) (e.g., Breilin et al. 2005). Three lakes formed in the basin of the Baltic Sea. In the area of the Gulf of Bothnia, the water level was 80 m lower than at present (Tulkki 1977). The land connection to northern central Europe was formed probably after the first stadial of the Early Weichselian at the latest.

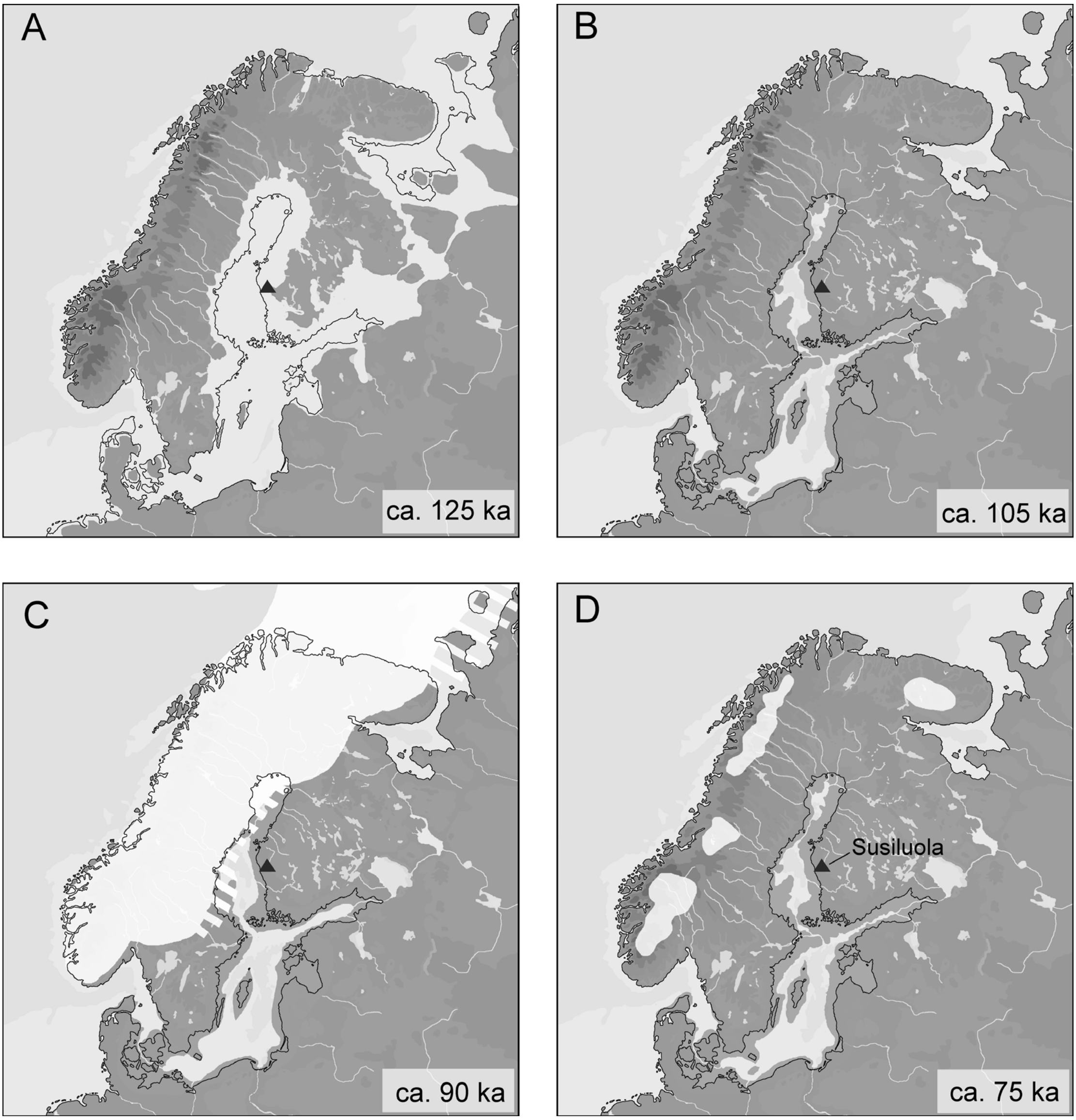

Fig. 3. Model of the palaeogeographical development (glaciations and shore line displacement) of Fennoscandia during the last interglacial and the Early Weichselian c. 125–75 ka. The location of Susiluola Cave is marked with a triangle. The model of the Eemian shoreline during the late temperate stage of the interglacial (level at Susivuori Hill = 116 m a.s.l.) was calculated by placing the hypothetical centre of the uplift area in Southern Finnish Lapland. The Holocene Ancylus gradient around 8.5 ka was used for the shoreline gradient. The Weichselian end moraines were "removed". The dimensions of the glaciers are based on Mangerud (2004), Svendsen and others (2004), Hirvas and Nenonen (1987). The shorelines of the isolated Baltic basin are based on Tulkki (1977), Nenonen (1995), Mangerud (2004) and PANGAEA data (Baltic Research Institute Travemünde).

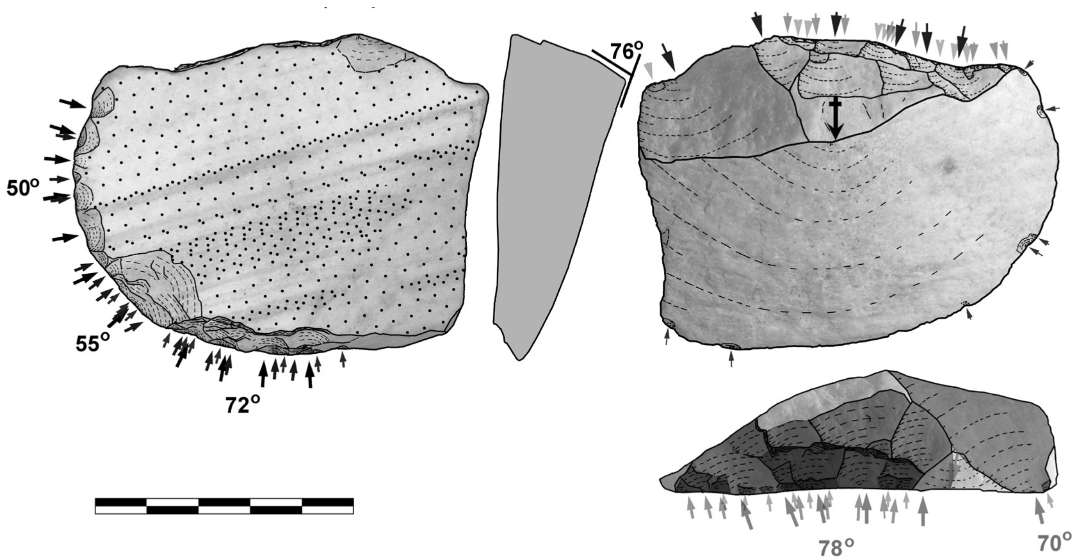

Fig 4. Scraper KM 33810:16, drawing on photo.

After an interruption caused by the second Early Weichselian stadial, the situation was re-established and it lasted to the beginning of the Middle Weichselian glaciation. According to the presented model, the colonisation of Finland was possible during the late temperate stage of the Eemian from the southeast via the isthmus of Karelia. During the warmer periods of the Early Weichselian, no geographical barrier would have hindered the spread of mammals and humans from Northern Central Europe to Fennoscandia (Fig. 3 with references).

The colonisation of the area via land bridges was thus possible from the late temperate stage of the interglacial until – with some possible interruptions –the beginning of the middle Weichselian glaciation (ca. 75 ka). During the whole period in question, Susivuori hill was connected to the mainland.

4. Why is there a lack of contemporary Neanderthal settlement in neighbouring regions or even anywhere north of 58°N?

The northern border of the distribution of European Middle Palaeolithic sites is quite sharply drawn by the ice-border of the LGM. The glaciation extensively destroyed the older surface deposits including possible remains of human occupation. Southern Ostrobothnia is one of the few areas inside the LGM-border, where terrestrial interglacial sediments have been preserved and Susiluola cave is the only sediment trap known in this region. Several stray finds from Denmark have been regarded as Lower or Middle Palaeolithic artefacts that indicate older settlement in Southern Scandinavia (Holm & Larsson 1995).

A possible occupation of these areas before the major Middle Weichselian glaciation has also been proposed in studies concerning the late Middle Palaeolithic settlement of Central Europe (Jöris 2004). A few Russian sites lie farther north, outside the LGM-border on the western side of the Ural Mountains. At Elniki II site, a small number of artefacts were found in a loess sequence below the last interglacial soil, which indicates human activity before the last interglacial at the latitude of 58° N. In the Garchi I site (59,2° N), a find horizon was covered by loess dated to > 60 ka (Pavlov et al. 2004).

5. Can one eliminate entirely natural causes for any of the artefacts from the cave?

The question was discussed in the last issue of FA (Schulz 2007). The sediments of Susiluola contain undoubtedly stones cracked by natural forces. The analysis of the different processes that caused natural fracturing in Susiluola and the surface textures caused by these processes as well as flaking experiments produced clear results. Additionally, the whole fractured material – not only the pieces classified as artefacts – was used for geofact-artefact analysis. The analysis showed a number of pieces with 'striking marks', which had to be classified as geofacts. Several pieces like a coarse tool (Schulz 2007: Fig. 3) and many of the notched pieces belong into a 'grey zone', where a decision whether they are artefacts or geofacts was not possible. Also – in the strict sense – all flakes without series of dorsal negatives should be considered possible geofacts. However, littoral processes that produce flaked pieces also destroy their products by abrasion; therefore a typical geofact assemblage contains a majority of cores. The core–flake index of the rock type assemblages and the higher proportion of smaller flakes point to artificial reduction. At last, the find material contains numerous modified tools, which show clear archaeological reduction strategies and, on the other hand, no plausible explanations for a genesis by natural processes can be offered.

6. Can one show one artefact of unambiguously human manufacture?

Numerous lithics were presented in 2007 (Schulz 2007: Figs 1–2, 4–6, 7–10). The example in Figure 4 is a piece where not only natural processes can be rejected (a regular convex edge with 11 primary negatives and 22 secondary negatives, and a transversal retouched edge with 6 primary negatives and 16 secondary negatives, and partly stepped retouch) but it also shows all features of a typical Middle Palaeolithich scraper with typical marks of resharpening (cf. Dibble 1995).

We hope that our answers are satisfying and will provide a good base for further discussions.

NOTES

[1] Excavation report Susiluola 1999, topographic archive, National Board of Antiquities, Helsinki.

[2] Holocene ages are given as calibrated ^{14}C-ages: Atmospheric data from Reimer et al. 2004.

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