3.3-Million-Year-Old Stone Tools and Butchery Traces? More Evidence Needed

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ABSTRACT

In the past five years, two exceptional discoveries have been made—the Dikika modified bones and the Lomekwi stone tools. If genuine, they would change current views on human evolution by showing that Pliocene hominins were involved in stone tool use and meat-eating more than 3 million years ago. These paradigm-changing discoveries require solid, unambiguous evidence. Here, we adopt a hypothesis-testing scientific approach in which we show that neither of these discoveries, as reported, has provided compelling evidence of these behaviors in the Pliocene. We do not reject the hypothesis of stone tool use and butchery in the Pliocene. We do however stress that the evidence presented in both cases remains disputable and the inferences drawn from it are not secure. We argue that the hypotheses of non-hominin agency for the Dikika bones and the *ex situ* nature of the Lomekwi assemblage (probably involving a palimpsest of accumulations) have not been discarded.

INTRODUCTION

The 2.6 million-year-old (Ma) stone tool assemblages from Gona (Ethiopia) show a degree of technological sophistication that differs significantly from that documented among chimpanzees (Semaw et al. 2003; Toth and Schick 2009). This technological gap, further stressed by a silent archaeological record between 6 Ma and 2.6 Ma, suggests that the almost four-million-year period of bipedal hominid evolution prior to Gona must have witnessed an increase in the manipulation and tool use skills of our ancestors when compared to chimpanzees. For this reason, we believe there must be an older and technologically distinct stage of stone tool use prior to the Gona early Oldowan; the same as we believe that there must be an older stage of meat-eating from that documented after Gona (Domínguez-Rodrigo and Pickering, submitted). The recent discovery of the LOM3 site (Lomekwi, West Turkana, Kenya) could possibly represent this earlier stage of stone tool making, technologically more similar to the battering tool kit used by chimpanzees (Harmand et al. 2015). However, we believe that the recent report by Harmand et al. fails to convincingly show that the LOM3 materials were recovered *in situ*. Harmand et al. provided a general stratigraphy of the type section and nearby localities, but did not show a detailed microstratigraphy of the site that could be clearly linked to the distribution map of the materials. We believe this is of utmost importance to properly evaluate the context and provenience of the purported Pliocene tools. All the geological information provided by Harmand et al. is macrostratigraphic, mostly in the form of regional contextualization of the sequence that contains the Pliocene sediments with purported tools. The geological descriptions also refer generically to some of the layers in connection with the "artifacts." Either way, no clear stratigraphic relationship of the vertical and horizontal contexts of the artifacts is provided and readers are left with uncertainty regarding the archaeo-stratigraphy of the site. We believe the report published by Harmand et al. (2015) fails to properly document important contextual elements for attributing the purported tools to a Pliocene context.

The discovery of LOM3 has sparked renewed interest in the purported butchery marks on two fossil bones from Dikika (McPherron et al. 2010). The interpretation of these marks was contested through an analysis showing that each of them was a trampling/abrasion mark (Domínguez-Rodrigo et al. 2010, 2012). A recent study of modifications on bones from the Dikika landscape spanning a wide diversity of depositional environments and a diachronic sequence of thousands of years provide further *ex situ* landscape information (Thompson et al. 2015), which could

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be potentially useful for interpreting the original Dikika modified bones (McPherron et al. 2010). Here we critically address the usefulness of such a landscape approach for interpreting agency in the modification of the bones originally reported by McPherron et al. (2010). In combining the evidence reported for Lomekwi and Dikika, our aim is not to argue that butchery with stone tools was not carried out during the Pliocene, but rather to stress that crucial further information is required before we can scientifically accept both discoveries as genuine. Given that these would be paradigm-changing discoveries, we emphasize that the evidence supporting both of them must be unambiguous.

THE CONTEXT OF THE LOM3 LITHIC ASSEMBLAGE

In situ refers to known original stratigraphic context. Secure stratigraphic attribution of an object can only be made when there is clear evidence that it was deposited on a specific stratum or geological layer. A prerequisite for this type of association is a clear separation between the stratigraphic location of the item and the surface deposits. It must be possible to distinguish with a level of certainty between deposits that are the result of surficial modern processes and sediments that have not been substantially reworked since their initial deposition. Most importantly, it must also be possible to distinguish elements in the non-surficial deposits that have not vertically migrated or ended up embedded into underlying sediments when attributing these elements to each type of deposit. This is lacking at LOM3. The LOM3 artifacts are reported as occurring on Pliocene sediments and covered by surface sediments. Harmand et al. show that the site is located on a mid-slope, the uppermost sediments of which form a "plaque of slope deposit" capped with thousands of *ex situ* blocks and rocks (see Harmand et al.´s Figure 2a, upper right). Although their Figure 2a shows that these *ex situ* rocks cover most of the slope, in the stratigraphic profile (their Figure 2b), rocks are represented only on top of and just below the excavation level of the sequence, with the area covered by the excavated profile—and the area above it—free of rocks or recent deposit sediments. This profile drawing contradicts the photograph in their Figure 2a.

The slope deposit lies unconformably on a sandy, silty, and "granulated" stratum; the one purportedly containing the tools (see our Figure 1). Although it is not clear in their publication if the darkened unconformable layer that constitutes the top stratum could alternatively be the upper part of the "Pliocene" deposit, modified in color as a consequence of fine cracks and pedogenic activity, this layer is also clearly resting unconformably upon the massive Pliocene silt. Its age is therefore uncertain, especially given that it is overlain only by colluvium clasts and rocks, and no clear separation is shown between both sediment types. In their Figure 2a, Harmand et al. show that half of the purported Pliocene darkened upper stratum is actually slope deposit (see their division indicated by white lines). The intrusion of the slope deposit into the darkened upper stratum is suggestive of possible vertical migration of materials, as is commonly the case when colluvium is embedded into originally older fine-grained sediments.

In Harmand et al.´s Figure 2a, it can be seen that artifacts are present within the slope deposit even in the excavated area. There, artifacts occur at a similar vertical depth as the "*in situ*" materials. This indicates that the slope deposit surely contains artifacts on a similar horizontal plane as the "*in situ*" tools. This is clearly seen and highlighted in Harmand et al.´s Figure 2a. This distribution of artifacts, largely exposed on top of Pliocene beds, is affected by erosion and slope wash, and suggests that the provenience of the materials is allochthonous.

When combining Figure 2a and Extended Data Figure 1, it can be seen that all "*in situ*" artifacts concentrate on a narrow <3m-wide band on the front of the bottom of the hill slope where the site was found. The front half of the excavation seems to have been either exposed through erosion or only minimally covered by colluvium or debris from the slope deposit (see Extended Data Figure 5 in Harmad et al.). It is only here that the "*in situ*" lithic materials have been documented. The rear half of the excavation, towards the Pliocene sedimentary wall, is surprisingly free of stone tools (see our Figure 1). It is only in this rear part of the trench near the wall that the slope deposit is effectively separated from the Pliocene sediments occurring on the same horizontal plane as the "*in situ*" materials. This distribution of artifacts, on the most exposed part of the Pliocene beds, affected by both erosion and slope deposit dynamics, could be suggestive of a provenience of materials alien to the Pliocene silty/sandy sediments.

This interpretation receives further support from the fact that the artifacts displayed in Harmand et al.´s Figure 2a occur on the white Pliocene fine-grained sediment at a depth that suggests a low position in this stratum when compared with the background of the trench. The lack of artifacts in the background of the trench, as shown in Harmand et al.´s figures, and on the interface between the eroded front of the Pliocene sediment with the surface sediments, further suggests that the original location of the artifacts is not necessarily the one reported by Harmand et al. (2015). The reported excavated area is $13m^2$. A more extensive excavation, with the potential of finding artifacts clearly *in situ* within the Pliocene sediments, could have solved these problems and the discovery would have been unambiguously novel.

Experiments and observations of vertical scattering of archaeological materials in clay, silty and sandy substrata show that surface materials may easily migrate vertically through wet-dry cycles and trampling even into greater depths in sandy contexts than the almost superficial locations of the LOM3 materials (Gifford 1977; Domínguez-Solera 2010). Attributing an *in situ* context to the LOM3 materials only because they are partially embedded in the interface between the Pliocene beds and the surface or the slope deposit is unwarranted. The absence of any Pliocene sediment clearly separating the surface and the slope deposit from the exposed Pliocene bed where the purported tools were found makes the provenience of these materials

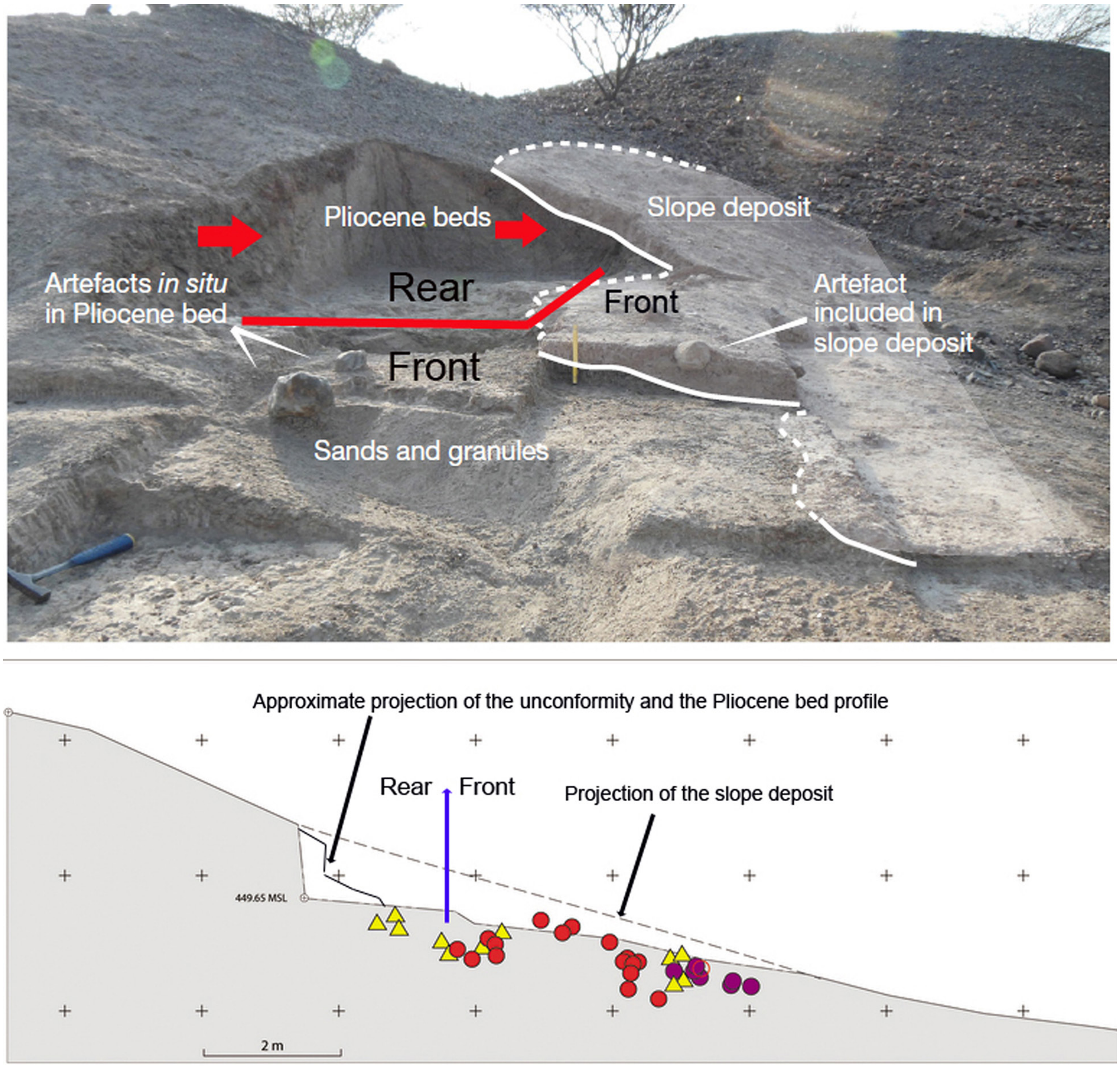

Figure 1. Upper: red arrows show the unconformity of the slope sediments and the Pliocene beds or, alternatively, between the chronologically uncertain upper layer and the underlying massive Pliocene silt where in situ "artifacts" were purportedly found. The wall profiles show severe erosion at the rear of the trench. Lower: vertical distribution of artifacts and fossils. Blue arrow separates the front and the rear of the excavation, marked by red lines in the upper figure. Virtually all in situ materials (especially artifacts, marked as red dots) occur at the front of the excavation, where the wall profile, above the horizontal plane on which the tools rest, is dominated by the slope deposit. This deposit also contains artifacts (original figures modified from Harmand et al. [2015]).

dubious. This doubt is further reinforced when observing the angular clastic matrix (resembling colluvium) that partially covered the tools, which also can be observed on the slope/surface sediments on the same spot (Extended Data Figures 5a-b, 5c-d in Harmand et al.). After removing these few centimeters of debris (colluvium?), the exposed tools lie on top of fine-grained Pliocene sediment that is debris-free (see the contrast between the sediments in Extended Data Figures 5e and 5i).

TECHNOLOGICAL COHERENCE OF THE ASSEMBLAGE?

Some of the images of the tools in Harmand et al. (2015) are very difficult to interpret. Some show extremely irregular microfractures and breakage planes that are also observed on rocks exposed for prolonged periods to weathering (Stern 1991; Rogers 1997). The slope deposit rocks are described by Harmand et al. as affected by intense thermoclastism. All artifacts shown in their Figure 5 show conspicuous traces that cannot be visually differentiated from subaerial weathering typical of thermoclastic "darkening" and "spalling". This is clearly reflected in the patchy coloration of the cortical surface. The fact that this feature is documented in several artifacts showing very irregular fractures could also support an interpretation of natural processes rather than hominin agency, although the latter cannot be rejected from the analysis of photographs alone. In addition, Harmand et al.´s Figure 5c shows a core that looks less skillfully "flaked" than the core shown in Harmand et al.´s Figure 4b (lacking exposure weathering traces). The latter shows a clear, overlapping sequence of successive extractions with minimal reflected scars. This latter feature contrasts with other "cores" which show abundant reflected percussive stigma. In addition, Harmand et al.´s Figure 4d shows a nicely knapped flake with lineal edges from successive negative scars on its dorsal surface, a skillful flaking pattern which cannot be distinguished from that documented in later Oldowan flakes both at Gona and Olduvai Gorge. This flake would support an ordered extraction of previous core flakes and a more intense core reduction sequence than the one inferred from several of the other irregular cores shown in LOM3. This degree of control in knapping contrasts with that documented in several of the other "artifacts" and would be surprising from a transitional industry prior to the Gona technology.

It may be a coincidence, but most of the artifacts showing discoloration similar to that documented in subaerially weathered rocks (like those that cap the slope sediment) are full of irregular fractures and are interpreted as either anvils or hammerstones. Our question is: can some of these "artifacts" really be differentiated from thermoclastic altered rocks from the slope colluvium?

AGE DETERMINATION FOR THE LOM3 SITE

A small tuffaceous lens is used as the fundamental layer to establish the age of the purported lithic assemblage at LOM 3. This layer is not documented in the site section, but in nearby sections. The authors correlate this tuff geochemically with the Toroto Tuff in the Koobi Fora Formation, radiometrically dated to 3.31±0.02 Ma. According to this, LOM 3 would be situated 10m above the Toroto Tuff and would be slightly more recent (3.27 Ma, according to the graph in Harmand et al.´s Figure 3). This figure also includes a photograph in which the Toroto Tuff and the LOM 3 site appear to occur on the same section. However, the stratigraphic sections of Extended Data Figure 2 lack clear correlations and section 2011-2 shows a lack of information below LOM 3. The correlation among tuffs are not clear in that figure and only the Toroto Tuff is marked in one of the several sections reported (2012-9). There, it is neither correlated with the LOM 3 section nor with the most complete section (2011-1).

For all these reasons, it is not conclusive that the small tuffaceous lens taken as a reference for the dating of LOM 3, and which does not occur on the LOM 3 section, is 3.31±0.02 Ma. We stress that we are not claiming that this is not the age of the deposit, but simply that Harmand et al. did not convincingly report that such was the case.

LOMEKWI AND DIKIKA: CIRCULAR ARGUMENTS

A reinforcing argument used by Harmand et al. (2015) to claim that the Lomekwi tools are genuine is the presence of "cut marks" on the 3.4 Ma Dikika fossils. An equally reinforcing argument used by Thompson et al. (2015: 3) to claim that the Dikika fossils bear cut marks is that "flaked stone tools have been reported" at Lomekwi. Therefore, now "it is known that in the terminal Pliocene hominins were flaking stone" (Thompson et al. 2015: 22). These statements are not supported by the data that are currently described in the publications associated with the Lomekwi and Dikika finds. Both of them could be false. As an example of the feeble foundation of this circular reasoning, let us consider the following. If the authentic Pliocene context of the Lomekwi artifacts cannot be confirmed, then it follows that the Dikika "cut marks" are not necessarily cut marks, and if there are no cut marks in the Pliocene then maybe the Lomekwi artifacts are not 3.3 Ma.

Domínguez-Rodrigo et al. (2010) published a list of reasons why most of the Dikika marks (on two bone specimens from locality DIK-55) are trampling marks and why two of them remained ambiguous. Thompson et al. (2015) emphasize the anthropogenic origin of the Dikika marks because the comparison of the DIK-55 fossils with other fossils collected in the Dikika area show that the bone surface modifications of the latter set are different. In our opinion, it should not be expected that a random collection of fossils retrieved from heterogeneous places along the landscape and from multiple depositional origins could exhibit the same traces as the Dikika marked bones documented by McPherron et al. (2010) at one specific locality. This is highly unlikely, because even bones from the same assemblage affected by trampling do not necessarily exhibit the same bone surface modifications. Abrasion marks as conspicuous as some of the marks documented on the Dikika fossils occur sporadically and are uncommon in assemblages where non-intensive trampling is experimentally replicated (Domínguez-Rodrigo et al. 2009, 2012). Given that the area sampled by Thompson et al. (2015) included fossils from sediments spanning dozens of meters of strata (Basal Member and Sidi Hakoma) and from a minimum of six different depositional environments (lake, fluvial channel, floodplain, delta channels, swamps, mudflats) one should expect that most of the modifications on these fossils should look broadly different from those of the DIK-55 locality. Despite this, Thompson et al. (2015) documented that as many as 18% of the marks on the bones collected in some of the Dikika areas are trampling marks[1]. Therefore, trampling seems to be a widespread modification on bones from the Dikika landscape across some depositional facets and time. This provides further support for the interpretation of the DIK-55 marks as resulting from trampling.

Our objections to the new arguments presented by Thompson et al. (2015) are based on their epistemological approach to hypothesis formulation (which conditions the way they designed their research and the subsequent interpretations), on the subjective criteria with which they evaluate the confidence in mark identification, and on the link between the marked Dikika fossils collected at locality DIK-55 and the thousands of fossils collected across the Dikika modern landscape.

Thompson et al. test the null hypothesis of the DIK-55 bones being trampled by: a) quantifying how many other fossils from the Hadar Formation at Dikika show marks with a similar morphology to DIK-55-2 and DIK-55-3 (from selective and non-selective collections); and, b) comparing the marks from these landscape fossils microscopically with the DIK-55 specimens. In reality, the testing hypothesis comprises one single premise: how the DIK-55 fossils compare morphologically (both micro- and macroscopically) with other bones from the Dikika area. We believe that neither of the statements in such a "hypothesis" follow the epistemological rigor of scientific hypothesis premises, since neither of them can reject the null hypothesis. The underlying assumption that for the null hypothesis to be certain all/most (what percentage?) of the landscape fossils should bear traces similar to those of DIK-55-2 and DIK-55-3 is unwarranted. It would only be supported if the same trampling agent had operated with a similar intensity over the landscape sampled by Thompson et al. (2015), across the more than one hundred thousand years represented by the fossils collected. These derive from the Lower Sidi Hakoma member, Middle Sidi Hakoma member, the Basal Member and probably other strata in the Hadar Formation sequence, since the surface location of the *ex situ* fossils collected does not necessarily represent their original depositional contexts. Thompson et al.´s assertions would also be supported if there were empirical evidence that bones in sandy contexts should systematically show conspicuous trampling marks, which frequently is not the case.

Additionally, how does the underrepresentation of similar conspicuous marks on fossils from the Dikika landscape prove that the DIK-55 specimens are not trampled and bear butchery traces? This question needs to be answered before the null hypothesis can be defined as in Thompson et al. (2015). It should be remarked that marks similar to those from the DIK-55 fossils have indeed been reported by Thompson et al. (2015) in their landscape collection[2] (Figure 2).

An important part of this circular argument is that the deposits sampled by Thompson et al. were formed in a time in which australopithecines were present in the Dikika landscape. Since no butchery mark was confidently identified in the thousands of specimens collected along the Dikika landscape, could this not be also taken as a sign that australopithecines were not butchering animals? Such an adaptive behavior, during the long time span sampled by the Hadar Formation sequence at Dikika, should have yielded further evidence of these purported butchering activities, as happens after 2.6 Ma, when the earliest cut marks were previously reported (Domínguez-Rodrigo et al. 2005). The initial discovery of cutmarked bones at Gona has been amplified by the discovery of more cutmarked bones in the same deposits with subsequent research in the area (Cáceres et al. submitted).

Figure 2. Similar marks documented by McPherron et al. (2010: butchery mark) (left) and Thompson et al. (2015: tooth mark) (right) but interpreted differently by these authors. Marks occur also in the form of a "palimpsest," meaning that they undergo biostratinomic and diagenetic processes that affect their morphology. The mark on the left preserves its shoulder intact, given the excellent preservation of the cortical surface. The mark on the right shows a substantial amount of flaking on the shoulder, which could be the result of weathering, as documented in the desiccation cracking observed on this specimen´s cortical surface. In both cases, the actor and effector could have been similar (cf. McPherron et al. [2010][left] and Thompson et al. [2015][right]. The right figure is pixelated because the resolution of the original publication is low).

Thompson et al. (2015) identified marks with degrees of certainty (high confidence or moderate confidence). We argue that this subjective categorization of mark identification should be discarded. The confidence in the identification depends on the analyst. It could be a reflection of analyst´s expertise instead of objective accuracy. We argue that this subjective categorization of mark identification should be discarded. Blumenschine et al. (1996) argued that there was a high degree of confidence in the inter-analyst identification of marks mainly because they were using modern experimentally marked bones in their test, which were mostly affected by single agents/processes or two agents with clearly opposing microscopic signatures (e.g., carnivore tooth marking and stone tool cut marking). In contrast, fossil bones frequently display different degrees of preservation and can undergo multiple modifications inflicted by several agents, resulting in a palimpsest of bone surface modifications. This circumstance results in fewer straightforward (high-confidence) identifications. As an example, the Dikika marks were originally identified with high-confidence as butchery marks by McPherron et al. (2010) because trampling could be excluded, given the absence of microabrasion. Subsequently, Domínguez-Rodrigo et al. (2010) identified microabrasion (and other microscopic features) with "high degrees" of confidence on the same specimens showing that trampling marks were present.

Domínguez-Rodrigo et al. (2010, 2012) argued that the DIK-55 fossils had undergone clear trampling, with only two marks being ambiguous. These authors argued that trampling should be presented as the null hypothesis, because both fossils with marks had also clear traces of this abrasive process. Therefore, the two ambiguous marks could also result from trampling, since similar marks had been experimentally documented in trampling experiments. This null hypothesis had not been applied to the Gona cut-marked bones because at Gona, in contrast with Dikika, the contexts of the bones were well-known and trampling in those sediments could not produce the conspicuous abrasion necessary to mimic cut marks. In addition, the remaining morphology of the Gona marks, in absence of evidence for trampling (e.g., lack of microabrasion), fit very well the diagnosis for cut marks (Domínguez-Rodrigo et al. 2005).

Thompson et al. argue that the fact that the DIK-55 fossils were found *ex situ* does not add any evidence for the likelihood of trampling in an abrasive sediment. It, in fact, does for two reasons: one is that the fossils were originally associated with a sandy context (McPherron et al. 2010); another is that abrasive sediments (sand and gravel) were identified in several strata in the sequence from the DIK-55 locality. The argument that the modifications have an ancient origin and, therefore, that this calls into question their attribution as trampling does not need further comments, since Domínguez-Rodrigo et al. (2010) were clearly arguing about biostratinomic modifications.

The use of Domínguez-Rodrigo et al.´s (2009) methodology by Thompson et al. (2015) is also problematic. Domínguez-Rodrigo et al. compared two populations of experimentally butchered bones and trampled bones. The resulting diagnosis at the population level is only applicable to a fossil record where individual taphonomic populations can be differentiated. The Dikika landscape collection includes specimens from multiple populations of unknown origin and, therefore, the frequency distribution of marks and the frequency of their microscopic identifying features are bound to be different from experimentally controlled assemblages. No matter how many statistical tests one uses, when comparing both sets (e.g., comparing the DIK-55 locality to a large-scale landscape assemblage), differences will be significant, unless one assumes that the same taphonomic processes were homogeneously operating at a landscape scale across millennia. If one expected to find no statistical differences in mark distribution between the Dikika landscape collection and the DIK-55 locality (more specifically, two bones from this locality), to prove that the DIK-55 bones could be trampled, then one is ignoring that: a) a landscape-scale assemblage is more time-averaged and more affected by a diversity of processes than a locality-scale assemblage; b) the sample size of DIK-55 is too small to be reliably compared to the exponentially larger Dikika landscape sample; and, c) many taphonomic processes are geographically and ecologically discrete. There is no way we can know whether any of the landscape localities sample the ecologically discrete features of the DIK-55 locality.

Domínguez-Rodrigo et al. (2010) showed, using microscopic criteria, how each of the marks on the DIK-55-2 and DIK-55-3 specimens could be attributed to trampling. These authors argued that only in case of contention should one consider the whole assemblage to disprove the null hypothesis. In addition, Domínguez-Rodrigo et al. (2010) argued that a configurational approach, including knowledge of the depositional context of any given bone, and a thorough study of the whole bone surface instead of just the analysis of single marks, should be mandatory to properly interpret bone surface modifications.

In sum, nothing in Thompson et al.´s (2015) recent approach shows that the DIK-55-2 and DIK-55-3 marked bones are the result of butchery by hominins and nothing in their data shows that the marks were not caused by local trampling. In the same way as Thomson et al. (2015) do not expect all the Dikika landscape fossils to bear (or lack) butchery traces to interpret the DIK-55 fossils as cut-marked, one should not expect the landscape fossils to be highly trampled to interpret specifically those at DIK-55 as the result of trampling. Both processes, like most biostratinomic processes, are locality-specific and, more commonly, bone-specific. If Thompson et al. had found more fossils in their landscape analysis indistinguishable from the DIK-55-2 and DIK-55-3 marks, would this mean that all were trampling marks or butchery marks? Only a configurational microscopic analysis could tell (as in Domínguez-Rodrigo et al. 2010), rendering the landscape approach that these authors used to be of limited value.

CONCLUSIONS

In this work, we do not intend to argue that the Lomekwi lithic assemblage and its chronology are false. We simply underscore that the evidence presented in Harmand et al.´s (2015) publication is insufficient to claim an anthropogenic agency and a Pliocene age for the whole stone assemblage. Likewise, the evidence presented by Domínguez-Rodrigo et al. (2010) questions an anthropogenic origin for the Dikika bones.

Supporting the Pliocene origin of the LOM3 materials because nearly a thousand km away in a chronologically different context purported cut-marked bones were found (McPherron et al. 2010) is a circular argument. This does not make the claims of Pliocene tools at LOM3 more valid. Neither are the Dikika "cut marks" more securely identified because potential pene-contemporaneous tools may exist. The Dikika marks are a taphonomic problem and have to be interpreted exclusively on taphonomic grounds. The fact remains that when comparing each of the DIK-55 marks (one by one) to trampling marks, most are identical in their microscopic features (Domínguez-Rodrigo et al. 2010). Ten out of the 12 Dikika marks are clearly trampling marks. The two remaining ambiguous cut marks are equifinal and, given the abrasion documented by the other marks, they could also have a non-anthropogenic origin (Domínguez-Rodrigo et al. 2010). This hypothesis cannot be rejected by Thompson et al.´s (2015) recent work. These authors have shown that the DIK-55 marks are different from most of the background landscape population of marks, but have been unable to show that they are macro- and microscopically different from experimentally-controlled trampling marks.

Thompson et al. (2015: 22) state that "claims of butchered marks on large mammals bones in deposits where stone tools are rare or absent can be contextualized by analysis of the surfaces of substantial numbers of other (*non-contextualized*) fossils found in the same deposits, rather than relying solely on the morphology of individual marks" (our emphasis) is taphonomically unwarranted. In our opinion, this position disregards contexts. The bones in the Dikika landscape sample have not been connected to well-defined strata. They come from multiple unknown strata from a sequence that spans dozens of meters. This landscape taphonomic approach is of limited value because one is comparing *ex situ* materials from anywhere in this sequence, where no certainty exists about the provenience of such fossils from strata with potentially abrasive sediments.

In sum, for the Dikika marks to be scientifically interpreted as cut marks taphonomists need to reject the alternative hypothesis that they are trampling/abrasion marks. Likewise, for LOM3 to be accepted as an *in situ* assemblage, archaeologists need to reject the alternative hypothesis that the site is a palimpsest resulting from minimal vertical migration of materials from the surface and the slope deposit or simply, the deposition of materials from diverse origins on the eroded surface of the Pliocene beds. Our confidence in the authenticity of this discovery depends on this and, the same as McPherron et al. (2010), Harmand et al. (2015) did not provide compelling evidence for the *in situ* context of the site. In both cases, more evidence to prove the existence of a Pliocene archaeology is needed.

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ENDNOTES

1 Thompson et al. (2015) use both conspicuous (identifiable with the eye) and inconspicuous (identifiable with magnification) marks, following Domínguez-Rodrigo et al. (2009). The frequency of 18% includes inconspicuous damage.

2 Marks reported by McPherron et al. (2010) in Figure 3e, Figure 3i, Figure 3b, and Figure 2d show the same microscopic features as the marks reported by Thompson et al. (2015) in Figure 4c, Figure 4a, Figure 4g, and Figure 4a respectively. However, the actor is interpreted differently by these authors.

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