

Decision by Camille Daujeard

Given that the three reviewers are not equal in their suggestions concerning your manuscript entitled: "A 115,000-year-old expedient bone technology at Lingjing, Henan, China", I decided to ask for a revision of your article. You will find below my main recommendations. First of all, this manuscript deserves publication. This work represents an important step towards the recognition of expedient bone tools among archaeological faunal assemblages. The comparison between the faunal remains from Lingjing (China, c. 115 ka) and those coming from an experimental sample of broken bones (horse remains), constitutes an interesting and suitable approach of that question. In my opinion, the data provided in that manuscript are in themselves sufficient for publication, even without being able to answer the question of the function of these bone tools. That may represent a second step in the research process and then another article, including notably, as stated by the authors, the use wear analyses.

We thank Camille Daujeard for acting as recommender of our submission to PCI Archaeology, for her assessment of our work, and for providing us with the opportunity to submit a revised version of the manuscript.

However, concerning the experimental protocol and data presented here and the references used, I wonder whether they are sufficient to distinguish between bone tools elements and bone splinters due to marrow recovery, which is a persistent and important issue for all of us, but one that deserves a great amount of caution. Thus, following the reviewers, authors may implement their statistical tests to consolidate their experimental data, and improve some important points concerning the Material and Methods, Results and Discussion.

We thank Camille Daujeard for providing clear guidelines as to how to improve our manuscript.

For example, concerning Material and Methods, a developed and detailed nomenclature of marks is required.

Done. We provide a clear definition for the different arrangements of flake scars we have recorded.

About statistics, authors should indeed give the confidence intervals for their percentages. Using percentages for about ten remains is not relevant.

We take the liberty of disagreeing with this suggestion. Confidence intervals should be provided when testing for statistical significance. Our use of percentages aims to allow inter-sample comparison despite variation in their size. They do not represent estimate but the actual proportions for a given variable observed in a given sample. We agree instead with one of the comments by D. Vettese that the use of the term "significantly" should be supported with the adequate statistical test and modified our manuscript accordingly.

Some references are also missing, and in addition to those cited by the reviewers, I would add others such as Blasco et al., 2008 about trampling marks that could generate some 'pseudo-tools'. In the same way, concerning the state of the art, I would also add the reference of Miller-Antonio et al. (1999), which states on the existence of tools on rhinoceros dental remains in the Palaeolithic site of Dadong in China.

We have included these references.

In this paper, the authors cite also the work of Sohn (1988), who reports the modification of bone at the Middle to Upper Palaeolithic Korean locality of Yonggul (Chommal) cave. The argument has been made for bone and antler use at Zhoukoudian (Pei 1938; Breuil 1939) and for worked bone from the Lower Palaeolithic localities of Donggutuo (Wei 1985) and Xujiayao (Jia & Ho 1990).

We added these references in the revised manuscript and noted that the chronology and anthropogenic nature of these purported bone tools need, in some instances, a reassessment with modern methodologies.

Concerning the Research background, I may suggest to the authors to make a review of the articles mentioning the possible existence of expedient tools, at least for early Palaeolithic periods of the whole old world, such as La Cueva Morin (Freeman, 1978; cf. Controversy in Freeman, 1983), de Rebibbia Casal de'Pazzi (Anzidei, 2001) or Saint-Marcel (Daujeard, 2007) in Europe, among others.

Done. We have cited two review papers that contain these and other relevant references.

Taking into account these few remarks, and above all those requested by the reviewers, the authors are invited to revise their manuscript and resubmit it to the PCI as soon as possible. Authors should pay attention to the detailed reviews made by the anonymous reviewer and D. Vettese, who listed various points asked to be improved.

Done.

Concerning the points highlighted by J. Hutson, authors may consider his remarks on the abstract and discussion about the question of the tasks done with the bone tools. Indeed, the question of the function of these 'expedient tools' will be developed in a future work, and the data processed here are not sufficient to address this issue in this article. Thus, the authors may limit their current work to the simple recognition of these tools, and only conclude here that some bones from Lingjing were intentionally shaped, which is itself a very important point that deserves publication.

Done.

Reviewer #1

Below is a point-by-point response to the review of our manuscript by Jarod Hutson.

This article details an assemblage of purported expedient bone tools from Lingjing (Henan, China) and a series of bone breaking exercises designed to distinguish between bones knapped for use as bone tools and bones broken for marrow. The authors provided an excellent framework for the study of these purported expedient bone tools, with plenty of archaeological context and background research on bone tool use, and I applaud their efforts in this regard.

We thank Jarod Hutson for reading our manuscript and for his appreciation. As he aptly points out, the experimental program reported in this study specifically aimed to distinguish bone fragments knapped for use as bone tools from those produced in marrow extraction activities. We thank this reviewer for recognizing our effort in providing a framework for the study of such expedient tools as well as our dedication in detailing the archaeological context which yielded them.

Unfortunately, the authors dedicated more space to context and background than to the presentation of results and conclusions. None of the context and background qualifies as a new or original contribution (there are numerous articles by Doyon and colleagues that include most of the same information) and the results and conclusion presented are incomplete.

This first critique seems in contradiction with the first part of the reviewer's general assessment. Keeping in mind the aim of the study, i.e., to distinguish bone fragments knapped for use as bone tools from those produced in marrow extraction activities, or other taphonomic agents specific to the site, providing a clear synthesis of the archaeological context seemed necessary. We regret that,

aside from the two examples below, the reviewer didn't judge necessary to further detail aspects that could have been improved in the results and discussion sections.

I don't intend to diminish the work that the authors have already completed, but I think that the authors would agree that the results, analysis, and conclusions are incomplete. There are two quotes from the text that illustrate my point.

It is very difficult to agree with this assessment without further details. Nonetheless, we provide our response to the two purported issues raised by the reviewer below.

From the abstract: The [knapped bones] were used for tasks for which the [lithics] were less or not effective.

This is not a useful statement, especially for the abstract. We may hypothesize that bone and lithic tools could be used for different tasks, but this paper does not provide any evidence for or against this hypothesis.

We disagree with the reviewer's comment. As we clearly state in our last sentence: "The continuity gradient observed in the size of lithics and knapped bones **suggests** the latter were used for tasks in which the former were less or not effective." The very nature of the verb "suggest" implies this is a hypothesis (see definition 1c. in the Merriam-Webster dictionary, which states: "to offer for consideration or as a hypothesis"). Contrary to the reviewer's statement, we do provide evidence supporting this hypothesis, i.e., the size difference between potential expedient bone tools and the lithic tools and flakes, and the continuity in size from the former to the latter. From his comment, the reviewer seems to be oblivious of the importance to contextualize results stemming from bone technological analysis back in the larger technological system in which they were used.

From the discussion: The function of the bone tools is a topic to be explored...An experimental and use wear programme is currently being implemented to test this hypothesis.

Without further experimental and use wear studies, the reader can only conclude that the bones from Lingjing were intentionally shaped, not that they were used as cutting or hide-working tools.

This reviewer's critique seems to fall somewhat outside of the scope of our study. As he clearly understood, judging from the first part of his general comment above, our aimed was not to report an experimental program to understand how these objects were used, but rather distinguish experimentally if they were purposefully modified or not. We agree with the reviewer that experimental and use wear studies are required to established the function of these tools as we clearly state ourselves in line 655 and 656 of the original manuscript. However, as we also say in the previous sentence (lines 653-655), "considering that processing carcasses of large and medium size prey has certainly been one of the functions the site has fulfilled, it is **very likely** that these expedient tools may have served in butchery or in hide processing activities." Our use of qualifiers opens a research perspective. It is taking into consideration the function of the site, for which details have been included in the Archaeological context section.

Sometimes (but not often) bone retouchers were intentionally shaped, perhaps to reduce the size of bone blanks to fit better in the hand. These were used as tools, but not the same type of expedient bone technology that the authors are suggesting.

It is not clear what argument the reviewer is trying to make here by alluding to bone retouchers.

If we understand correctly, the argument put forward by this reviewer is that if you don't understand the function of a bone tool, you cannot claim that it is a tool indeed and pretend publishing it (this is the only reason he provides for rejecting the paper). However, everybody agrees bone bifaces were shaped by knapping during the Acheulean even if we don't know the reason for which they were

used. This also applies to stone tools. If stone tool assemblages could be published only when the functions of the tools were previously identified, the scientific literature on cultural evolution within our genus would be rather thin.

In sum, we don't think that not identifying the function of the expedient tools should prevent the publication of our study.

Since the authors mention that a use wear programme is currently being implemented, we must assume that there will be a follow up article on the results of that work. Therefore, this article will be irrelevant upon publication of the additional experimental and use wear results.

Science is done in an incremental way. Our present contribution aims to provide criteria on how to recognize bone fragments purposefully knapped to be used as tools versus those with flake scars produced during marrow extraction activities (for a similar publishing strategy to report an experimental approach to establish criteria allowing the recognition of metapodials used in knapping activities without use wear / functional study, see Jarod M. Hutson et al., 2018. "On the use of metapodials as tools at Schöningen 13II-4" in Jarod M. Hutson et al. [eds.] *The Origins of Bone Tool Technologies*, Mainz: Römisch Germanisches Zentral Museum, pp. 53-91). Never, have we set our objective to explain in detail for what activities these tools were used.

Because of these concerns, I must reject this article for publication. I hope that the authors do not take my decision as a rejection of their work overall. I believe that the article is well written and well researched, but I think that the authors would agree with me that their work is not finished. As it is written, the article does not advance our knowledge of bone technology. With additional experimental and use wear studies, which the authors acknowledge is currently being implemented, this article will be publishable and will have a more lasting impact on the study of bone tool technologies, expedient or otherwise.

Considering the aim of this study, we feel we were capable to quantitatively and qualitatively demonstrate significant differences between the bone fragments produced by our marrow exploitation experiment and the archaeological specimens from Lingjing. With the detailed archaeological context, we also ruled out any other potential taphonomic agents that could have explained the observed flaking pattern. This has allowed us to isolate 56 expedient bone tools. In and of themselves, these conclusions met the aim of our study. In our discussion, we went the extra step by comparing bone tools to lithic tools in order to propose a hypothesis as to their potential function and integration in the overall technical system, while keeping in mind the site function. This is a new research prospect, which will be treated in an upcoming manuscript.

Specific comments on the work would be premature at this point because the authors have yet to test their hypotheses on the function of these expedient tools. I would welcome the opportunity to provide a second review when the authors complete their study.

Specific comments would have been welcomed and are mandatory if we understand the code of conduct for PCI Archaeology, which states: "Reviews and recommendations should be of high quality. Reviews should be sufficiently deep and detailed for the PCI Archaeology recommender handling the recommendation process to gain a full appreciation of the qualities, defects and limitations of the article. Texts (reviews, recommendations, comments, messages to authors) will be returned to PCI Archaeology recommenders and reviewers if they do not respect these rules."

Making a fair and complete review conditional to changes to the manuscript seems unethical to us in light of the code of conduct the reviewer has agreed to comply to when accepting to evaluate our work. If this way of conduct was generalized, it would make each review process endless.

Reviewer #2

Below is a point-by-point response to the review of our manuscript by an anonymous reviewer.

The work is precise, detailed and well structured. Clear in describing the different phases of research: from the experimentation to the analysis of archaeological finds. I share the hypothesis that some bone splinters in Pleistocene sites may also have been used as tools. I have often wondered if it just represented the use of a splinter produced during the slaughtering or extraction of bone marrow or, in some cases, it was intentionally produced to be used.

We thank the reviewer for his/her assessment of our work and provide a point-by-point response below.

Row 345 - Material and Methods. I would like to point out, from direct experience, that the long bones of equids are more compact and have more trabeculae than those of bovids and cervids. This makes a greater use of blows necessary. I would ask the authors for considerations on this matter.

Done. We agree with this reviewer's comment. We have added a sentence in the Results section to that effect. It reads: "Compared to fracturing experiments done on cattle long bone (e.g., Blasco et al., 2014; Stavrova et al., 2019; Vettese et al., 2020b), more blows were required to expose the medullar cavity, which can be explained by the dense trabeculae present in horse's long bones."

Row 345, Material and Methods and fig. 2 - Another curiosity about the experimentation: is the choice of the area of the bone to be hit based on the recurrence of the points of impact on the anatomical elements of the archaeological sample or left to the decision of the operator? Some blows were inflicted near the epiphysis (e.g. radius), instead it is the point of easiest fracture or it is easier to break the bone by hitting the posterior face, as I happened to observe for the equidae radius.

Two methods were used to fracture the long bones. The experimenter's aim was to produce longitudinal diaphyseal fragments in the process. In this specific case, Experimenter #1 wanted to control to some extent the formation and propagation of cracks on the diaphysis. We have modified our description of the experimental protocol to clarify this was left to the experimenter's decision.

Row 508 5.2 Archaeological data - I find it interesting to relate the position of the blows on the experimental bones to those of the archaeological bone remains. It would be useful to have a detailed study of the fractured identified bone remains (species and element) to compare them with the experimental data.

The complete faunal analysis is still ongoing. This study led by colleagues from Shandong and Leiden University was unfortunately paused by the current sanitary crisis. We agree the comparison would be useful, however the data is currently not available.

In the figures 5-6 (sic) of the bone findings it would be interesting to indicate the likely areas of wear (e.g. with arrows) or to insert at least one or two significant remains with details of the areas that the authors find interesting as traces of impact and wear.

We have opted to add a Figure with close-ups of the bone tools to show the variability in flake scars. As per the wear, the polish extent and areas are extremely variable on the assemblage. Further research is required to understand its origins. The next phase of our research program aims to investigate this issue and is currently being undertaken.

Row 71 - Just a clarification the work to be mentioned by Polledrara where the study of fracturing is discussed is Santucci et al, (2016) - Palaeoloxodon exploitation in the late Middle Pleistocene site of Polledrara di Cecanibbio (Rome, Italy). Quaternary International, 406, 169-182

We thank the reviewer for pointing out this reference. We have made the adjustment in the text and reference list to reflect this.

Reviewer #3

Below is a point-by-point response to the review of our manuscript by Delphine Vettese. To avoid repetitions, we have taken the liberty to merge some comments when the issues raised by the reviewer were related.

The paper "A 115,000-year-old expedient bone technology at Lingjing, Henan, China" by Doyon and colleagues presents some quantitative and qualitative criteria to identify expedient bone tools based on the study of different bone fragment samples from the layer 11 of Linjing (Henan, China).

I recommend this manuscript for publication with major revision but I suggest some changes to help to improve the manuscript. I find this paper useful, however, I noticed some issues easily addressed. Please find the comments and suggestions below. Moreover, English is not my mother tongue. I do not consider myself able to judge the grammatical syntax of this text. However, the manuscript is clear and understandable.

It is highly appreciated the mention in the text of the contribution in this work of each author, and the analyses they performed.

We thank the reviewer for her thorough assessment of our research and for providing clear feedback on how to improve the manuscript. We provide below a point-by-point response on the issues raised.

Introduction and Research background: The presentation of the context study and the bibliographical review is quite complete and very well detailed. In the Research background, the paper of Mateo-Lomba et al. 2020, which developed an experimental study of bone expedient tools from long bone bovids, could complete the listing of recent work on the subject. The authors clearly show the importance of highlighting expedient tools within faunal assemblages and how complex it is to identify them so far.

We have added Mateo-Lomba *et al.*'s paper to our research background and contextualised their contributions to our research question.

Maybe, as the convention established by Schwab and Patou, 2002 is used in this paper it should be quoted in the references.

This reference relates to retouchers, soft hammers and pressure flakers. It doesn't address expedient bone tools and therefore, we didn't include it in our study.

Archaeological context: I am not a specialist in this region and context. However, I found the presentation complete, clear and very detailed regarding all the contextual aspects.

Thank you.

It could be appropriate to mention that the site is interpreted as a kill/butchery site in this part because it is used in the discussion to justify the use of bone fragments as expedient bone tools.

We provide information on the site function in lines 286-289 of the original manuscript. To emphasize this interpretation more clearly, we slightly modified the beginning of the discussion to reiterate the interpretation of the site function.

Nevertheless, I suggest adding the NISP and the MNI of the different species presented in the levels studied. I had some difficulty to find this information in the previous papers. Material and methods: - Archaeological remains The authors should clarify if the 100 fragments composing the sample of RCS are from long bones, only cortical bones or cortical with spongy bones. Similar suggestion applies to the fragments of the CCS sample. It will be instructive to add the ratio of long bone NISP (or compacta) to total NISP regarding the archaeological samples studied to know the level of compacta, spongy bones and long bones. This could permit to present better the three samples nature and composition.

As mentioned in a response to a comment by reviewer #2, the analysis of the faunal assemblage is in the process of being performed by colleagues from Shandong and Leiden University. In van Kolfschoten *et al.* 2020, preliminary results are being presented relative to the NISP (see table 1 reproduced below). As the information is incomplete at this stage, we thought it was better not to include figures that are bound to change in the near future. With regards to long bone fragments with and without spongy bone, it is mainly cortical bones for both samples. We now provide this information in the Material and Methods.

Rodentia	
Rodentia fam. gen. et sp. indet. 1	x
Rodentia fam. gen. et sp. indet. 2	x
Lagomorpha	
Lagomorpha fam. gen. et sp. indet.*	x
Carnivora	
<i>Canis cf. lupus*</i>	xx
<i>Vulpes sp.*</i>	xx
<i>Ursus sp.</i>	xx
<i>Meles sp.*</i>	x
<i>Panthera cf. tigris*</i>	xx
<i>Pachycrocuta cf. sinensis</i>	xxx
Proboscidea	
<i>Palaeoloxodon sp.</i>	x
Perissodactyla	
<i>Coelodonta antiquitatis</i>	xxxx
<i>Dicerorhinus mercki</i>	x
<i>Equus przewalskii</i>	xxx
<i>Equus hemionus</i>	xxxxx
Artiodactyla	
<i>Sus lydekkeri</i>	xxx
<i>Hydropotes pleistocenica</i>	x
<i>Cervus (Sika) sp.*</i>	xx
<i>Cervus (E.) elaphus</i>	xxx
<i>Elaphurus davidianus*</i>	x
<i>Sinomegaceros ordosianus</i>	x
<i>Bos primigenius</i>	xxxxx
<i>Procapra przewalskii</i>	xxx

Table 1: List of mammalian taxa represented in the faunal assemblage from Lingjing Layer 11 and the base of Layer 10. The list is based on the faunal analyses of the finds excavated in 2005 and 2006 (Li and Dong 2007; Zhang 2009). In more recent excavations additional species, indicated with a *, are represented. x=1-5 specimen; xx=6-20 specimen; xxx=21-100 specimen; xxxx=201-500 specimen; xxxxx=more than 501 specimen.

The authors should provide some additional information about the “contiguous” scars to clarify if it means that they are incomplete because one overlapped the other (as mentioned in Galan and Dominguez-Rodrigo 2009 as “Double overlapping notches have negative flake scars that overlap with an adjacent notch.”).

Archaeological data Lines 515-517: “With the exception of four specimens from the PBT, none of the other diaphyseal fragments with flakes removal scars bears impact scars that could be interpreted as resulting from bone fracturing”. It could be necessary to precise which criteria are used to do this difference between the two scars and detailed more this assumption.

We provide a definition of our arrangement categories in the material and methods section. We have not made a distinction between adjacent and overlapping flake scars; both are included in the contiguous category. Our use of the term contiguous is the 4th definition in the Merriam Webster Dictionary, i.e., touching or connected throughout in an unbroken sequence. We likewise clearly state the difference between flake scars – removal of compacta – and impact scars, i.e., small depression / crushing of the compacta produced by the protrusion of the object used to hit the bone.

Line 355: the number of remains studied for this paper is 1.498 while summing 127 (PBT), 100 (RCS) and 1260 (CCS), the result is 1487.

Done. We thank the reviewer for identifying this typo.

Experimental program The protocol description is clear and very well detailed.

Thank you.

Lines 446-451 are the same of the lines 386-390 in the Archaeological remain part. The authors could only keep ones and precise that the same requirements are taking into account for both materials: archaeological and experimental.

Done. We have slightly modified the sentences in lines 446-451 to avoid repetition.

I would like to know why the authors did not test the batting technique to extract marrow in addition to the hammerstone and anvil technique. Because it could have some different results in term of the fragment's size or impact scars number.

Our experimental protocol was constrained by contextual evidence. No big blocks suitable for the batting technique were found at the site and the presence of small bone flakes shows the bone fracture occurred at the site. We now provide more details about the reasoning that motivated our choices in the material and methods.

Besides, I found that the number of fractured bones and therefore the number of fragments studied was relatively reduced. Why did not you use an extra sample of bovine or cervid bones for example, from previous experiments? Since there, regarding the archaeological data, it does not seem to be any difference observed according to the size of the ungulates treated at the archaeological site.

We agree with the reviewer that higher sample size increases the potential to obtain representative results. However, our fracturing experiment on six long bones produced 317 fragments, which is a sample size generally accepted as being statistically significant. About the taxon choice, we opted for equid long bones because this is the most represented taxon in the faunal assemblage.

Results: Some results mentioned by the authors as significant should be supported with statistical tests such as spearman correlations test on the number of fragments per size interval for each sample or

possibly an Anova test per row for the data in Figure 5. For example, regarding lines 521 and 524 and 545.

We thank the reviewer for pointing out the statistical test values were missing. We have updated both our material and methods section and our results section to list the statistical tests we made and the ensuing results.

Line 512: We use the Kruskal-Wallis non-parametric test because the length values are not normally distributed. We modified the sentence to: "All samples differ significantly from one another when the size of the fragments is considered (Kruskal-Wallis $\chi^2 = 174.04$, $df = 2$, p -value < 0.000). This difference is further accentuated by the underrepresentation of small fragments in PBT and RCS compared to CCS, which can be explained by the change in recovery procedure implemented in 2017."

Line 524: The thickness values are normally distributed and we therefore used a Student's t-test. The text has been modified with the following addition: "($t = -7.3323$, $df = 166.42$, p -value < 0.000)".

Line 545: We used an Anova complemented with a pairwise comparison based on Tukey HDS. The text now reads: "When the number of the flake scars per specimen is analyzed, significant differences are observed (Fig. 5a). These differences are especially marked between PBT and all other samples ($F(3,150)=22.78$; $p < 0.000$), both archaeological (PBT:RCS $p < 0.000$; PBT:CCS $p < 0.000$) and experimental (PBT:EXP $p < 0.000$). No significant pairwise differences can be established between RCS, CCS and the experimental samples as illustrated by the overlapping value (Fig. 5a; RCS:CCS $p = 0.998$; RCS:EXP $p = 0.796$; CCS:EXP $p = 0.800$).

Experimental data Nothing, it is a complete presentation to the results.

Thank you

It could be interesting to have the number of following bifacial scars (minimum-maximum) according to the samples.

This information is already presented in Figure 5a.

Comparison between archaeological and experimental diaphyseal fragments Line 537: The authors could provide, inside the text, the number corresponding to the percentages to a better comprehension of the results.

Done.

The authors could add the number of remains with polishes and scars according to their type, maybe provide a contingency table, in the part of the results and discuss the results.

We find unclear what the reviewer is asking here. We provide the percentage of remains with polish in Table 4. This information is presented by sample and we make a distinction between fragments with and without flake scars. If the reviewer refers only to the specimens interpreted as expedient tools, the information is likewise presented in the SI Table.

In the text, once it could be more legible to write in any letter that μ means average and σ means the standard deviation.

Done.

Discussion: Lines 634-636: The arguments exposed are very accurate. Moreover, could it be also due to the tools (hammerstone) used to extract the marrow (no matter what raw material choose: bone, stone, wood...) which, could be used to retouch the bone fragment?

This comment seems more of a question inviting discussion than a recommendation to modify the manuscript.

Lines 641-642 and 646: Careful to the homogeneity, the standard deviation is noted as SD while in all the rest of the paper is noted σ .

Done.

Line 656. I suggest quoting the work of Mateo-Lomba et al. 2019, and Di Buduo et al. 2020. In addition, it could be a good opportunity to mention the complementary analyses, which will be performed, on the expedient bone tools identified.

We have added these two references in our research background section.

Besides, the authors should be changed "programme" by "program".

Done.

Inside the discussion, it could be appropriate to add some words on the retouch tools identified, particularly regarding their sizes. It could be appropriate to then compare them with the expedient tools (in addition to the lithic tools). Also in Figure 8, this information could be added.

It is not clear to which retouch tools the reviewer is referring to. If she refers to the expedient bone technologies, we already provide such information in lines 640-648. If she refers to the bone retouchers, pressure flakers and soft hammer previously published (Doyon *et al.*, 2018, 2019), we hardly understand the added value such addition would bring to the solution of the addressed scientific issue. Perhaps the reviewer could provide more details as to why she thinks this is needed.

Line 667: The authors could add some word on the chaîne opératoire of the bone, and the implication of the expedient bone tools for the site interpretation.

We have added this information in our discussion.

Figures and Tables: Regarding Tables 1, 3 and 4, it could be better to add the number at the percentages.

Given the size difference between samples, we opted for using percentages in order to facilitate inter-sample comparison.

Table 2: "Only faunal remains" How the percentages were performed is not clear. The authors could precise it in the caption.

We have modified the legend of this table.

In Table 3, to be more homogeneous, the authors should change "endosteal" by "medullar" as mentioned in the methods.

Done.

It could be interesting to mention the presence or absence of abrasion and if it is relevant, the association with the impact scars or flake scars, and the other possible alterations, like concretions, cracking and the legibility of the fragments due to all the taphonomical alterations in Table 4.

Abrasion was not observed on any of the specimens considered in the study and the legibility of Lingjing faunal material is generally excellent. We have added this information in the relevant result section.

It will be instructive if polish presence and intensity will be associated with the other criteria presented in the paper to identify the expedient bone tool.

We agree this information would be instructive indeed. However, it falls somewhat outside the scope of the present study. We have provided in Table 4 the information on polish to show the variation in its distribution. It is entirely possible that flakes without removal scars could have been used in expedient activities just as those with removal scars. At this point, further use wear analyses are required to adequately identify the functions that led to the production of a polish. The next phase of our research constitutes one step in that direction.

The authors should clarify what means "Ancient fracture" if that means dry bone fracture?

We have clarified this in the SI-Table.

Having all three sides of the bone illustrated and in a good quality value the paper. However, it might be welcome to highlight the different scars or removals (with arrows or staining).

As mentioned for a similar comment from Reviewer #2, we have added a new figure to this version of the manuscript to show the variability in flake scars.

Supplementary Information In the column of the tables, the authors should put "N" when it is necessary (empty now), or in the captions or head-table do not precise "N" means No presence (or absence).

We do not understand this comment. It is usually common knowledge that Y/N refers to Yes/No. We have removed the (Y/N) from the column headers of the taphonomic alterations to avoid confusion.

The authors should precise why in the table p13 and p14 some cells are empty (example fragment 14L329 miss the flakes n°2 and 5 and adding the explanation in the captions).

Empty cells were added only when flakes were arranged in interspersed series to signal a discontinuity between two flakes scars or series of flakes scars. We added a note to that effect in the legend of the table.

In the SI tables, replace the Fig.8 and Fig. 9 by Fig.6 and Fig. 7.

Done

In sum, this manuscript presenting criteria to identify expedient bone tools is relevant. It represents the first step to sort this type of archaeological object in a faunal archaeological assemblage. Moreover, as the authors said, this study should be completed by use-wear analyses and these expedient bone tools should be compared with additional experimental data to understand if and how they were used and for which purposes. "An experimental and use wear programme is currently being implemented to test this hypothesis."

Nevertheless, a revised version would be required, edited to reflect the comments and suggestions above, notably regarding the statistical tests to support the criteria highlighted by the authors.

We thank this reviewer for her appreciation of our work and hope that the modifications introduced meet her requirements

References: Di Buduo, G. M., Costantini, L., Fiore, I., Marra, F., Palladino, D. M., Petronio, C., ... & Sottili, G. (2020). The Bucobello 322 ka-fossil-bearing volcanoclastic-flow deposit in the eastern Vulturno

Volcanic District (central Italy): Mechanism of emplacement and insights on human activity during MIS 9. *Quaternary International*. Galán, A. B., Rodríguez, M., De Juana, S., & Domínguez-Rodrigo, M. (2009). A new experimental study on percussion marks and notches and their bearing on the interpretation of hammerstone-broken faunal assemblages. *Journal of Archaeological Science*, 36(3), 776-784. Mateo-Lomba, P., Fernández-Marchena, J. L., Ollé, A., & Cáceres, I. (2020). Knapped bones used as tools: Experimental approach on different activities. *Quaternary International*. Patou-Mathis, M., & Schwab, C. (2002). Fiche générale. Retouchoirs, compresseurs, percuteurs... Osa impressions et éraillures. Editions SPF, Cahier X, Paris, 11-19.