



This paper provides a baseline for location analyses of percussion marks. Their dataset may therefore be regarded as a null hypothesis according to which the archaeological data could be tested. If Paleolithic patterns of percussion marks differ from Vettese et al.'s [3] “intuitive” patterns, then the null hypothesis is disproved and one can argue in favor of a learned pattern. The latter can be a result of “culture”, as Vettese et al. [3] phrase it, in the sense of nonrandom action that draws on transmitted knowledge. Such comparisons bear a great potential for understanding the degree of technological behavior in the Paleolithic by factoring out the “natural” constraints of bone breakage patterns. Vettese et al. [3: fig. 14] started this discourse by comparing their experimental dataset to some Middle and Upper Paleolithic faunas; we are confident that many other studies will follow.

## Bibliography

[1] Pickering, T.R., Egeland, C.P., 2006. Experimental patterns of hammerstone percussion damage on bones: Implications for inferences of carcass processing by humans. *J. Archaeol. Sci.* 33, 459–469. <https://doi.org/10.1016/j.jas.2005.09.001>

[2] Blasco, R., Rosell, J., Domínguez-Rodrigo, M., Lozano, S., Pastó, I., Riba, D., Vaquero, M., Peris, J.F., Arsuaga, J.L., de Castro, J.M.B., Carbonell, E., 2013. Learning by Heart: Cultural Patterns in the Faunal Processing Sequence during the Middle Pleistocene. *PLoS One* 8, e55863. <https://doi.org/10.1371/journal.pone.0055863>

[3] Vettese, D., Stavrova, T., Borel, A., Marin, J., Moncel, M.-H., Arzarello, M., Daujeard, C. (2020) A way to break bones? The weight of intuitiveness. *BioRxiv*, 011320, ver. 4 peer-reviewed and recommended by PCI Archaeology. <https://doi.org/10.1101/2020.03.31.011320>

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Revision round #1

2020-05-04

Dear Authors,

We have now received three reviews for your preprint entitled “A way to break bones? The weight of intuitiveness”. We are pleased to say that these were all positive and agree that this is an important paper, which makes a relevant contribution to the study of bone breakage for marrow extraction and, by inference, of complex behaviours e.g. in the Palaeolithic. Furthermore, the reviewers concur that the methods are sound and the conclusions justified and, as such, they recommend publication, provided some minor issues are addressed: - The clarity of the manuscript could be improved by subjecting it to another round

of editing. Sometimes your arguments are not sufficiently clear or ambiguously phrased. For example, what is “quasi-systematic” marrow extraction or “quasi-systematic conservation of the epiphyses”? - The presentation of the figures and their captions needs to be improved - Various edits and requests for clarification should be dealt with (see below) Reviewer #1 (Alan Outram) points out that more space could be devoted to discussing future avenues of research, e.g. the impact of cultural preferences on this practice (cooking methods, different implements, presence/absence of periosteum...). Furthermore, reviewers #2 (Terry O'Connor) and #3 have provided more detailed comments, which are listed below.

We would ask you to revise the manuscript within one month, according to the comments of the three reviewers, and to submit the revised preprint, along with a detailed point-by-point response. We shall be happy to recommend it, pending suitable minor revision.

Looking forward to receiving your revised manuscript.

Kinds regards

Beatrice Demarchi & Reuven Yeshurun

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*Reviewed by [Terry O'Connor](#), 2020-04-09 19:40*

A way to break bones? The weight of intuitiveness – Review

This is an informative report of detailed and thorough actualistic research. The larger research context is topical and timely, given the current interest in complex behaviours inferred from Middle Palaeolithic assemblages. Such experiments are never simple to organise and can usually be criticised for having too little replication of experimenter, material and procedure. However, this team developed a good pragmatic compromise between replication and practicability: it would be foolish to require hundreds of volunteers smashing many thousands of bones even if that did increase statistical rigour. The working methods are described clearly, with enough detail to allow the work to be repeated but without making the text longer than necessary. Similarly, the results are set out well and are properly separated from the discussion of those results and inferences drawn from them. The conclusions are, to be honest, not surprising. If asked, I would have

expected the morphology of the bone to be a strong predictor of the pattern of pits and fractures, and that different people would tackle the task in quite different and probably inconsistent ways. However, that would have been informed speculation, at best, whereas this paper presents firm supporting evidence. The conclusion regarding counter-blows is particularly important, and often neglected in studies of ancient material. The last paragraph could, perhaps, have said more about the experimental procedure and how it might be refined or modified in any future research? In all, I enjoyed reading this paper and learned from it. Below, I have a few comments on particular details.

Abstract: This needs to be more concise, and I strongly recommend that the authors edit the Abstract with the support of a first-language English speaker to ensure that the text says exactly what they intend. As it stands, I am not sure that this is the case. NB this applies to the Abstract, not to the paper as a whole. 133-4: why give the standard deviation of the volunteer's ages? Is there any reason to think that they would be approximately normally distributed? "Eight men aged xx to yy" would be enough? Methods statement: good. This is thorough and detailed without including tangentially relevant material. It would enable another researcher to repeat the process with confidence. 216-236: nice account of the stats analysis, and sensible to have used Fisher's exact test. 241-255: although this is interesting and perhaps unsurprising, all of the volunteers came from a culture with chairs and other forms of seating. It is questionable, therefore, whether these results can be used to propose or compare positional behaviour in any non-chair group such as Neanderthals. 312-325: I realise that this would have been difficult to parameterise! There is a difference between a task that is 'hard' because it is difficult to undertake and one that is 'hard' because it requires a lot of physical effort. For example, lace-making is 'hard' because it is complex and requires intricate hand movements, but not 'hard' because it is tiring or requires strength. Chopping logs is the reverse. Does the scale of difficulty used here integrate those two rather different forms of 'easy/hard', or is it one more than the other? 340-346: that's really interesting. 383-6 and 418-23: does this indicate a significant degree of inter-observer variation in the recording? If so, this needs to be said more explicitly. Figure 3: that's good. Not only the PCA plot, but indication of which variables were loading which dimensions of the plot. 444-6: check the caption to Fig 5. The labelling does not match the figure. 484-6: caption to Fig 9 is quite difficult to understand. Can this be improved by labelling specimens within the fig? Same for Fig 11. 538-9: how did experimenters 'clean' their hands? I would expect them to rub their hands with dust/dirt to congeal and remove fat plus sweat. Maybe that's just me.

*Reviewed by [Alan Outram](#), 2020-04-03 16:56*

This paper is impressive in a number of respects. Firstly, the fracture experiment sample sizes are very considerable and this constitutes a very substantial volume of work and one of the larger datasets in bone fracture experimentation. Secondly, the GIS recording method is innovative and useful and likely to be adopted by others. The methodology used is very rigorous and the data are displayed well. The paper usefully recognises the value of investigating impact placement on bones as a way of understanding the practice and cultural/cognitive differences between different hominins and different groups of AMH. The paper establishes an expected pattern based upon the intuitive behaviour of modern human subjects. These experiments usefully establish that bone morphology has a greater effect upon placement of impact for some elements than others and therefore provides a useful comparative framework for whether behaviours observed might be pragmatically determined or the result of specific cultural learning or particular activities. Clearly the data need to be used as a point of reference rather than deterministically. The authors fully appreciate this.

It is clear that this body of work is of value and the methods may be applied by others. The paper is clear about the parameters used, but could potentially, in discussion, flag up further issues that might come into play in interpretations of patterns and areas where further experiments are needed. This set of experiments is aimed at marrow extraction on fresh bones, with periosteum present using a hammer stone. This practice could, indeed, be an expression of cultural preference, just in the same way that meat butchery patterns can represent a form of material culture. However, further discussion could be made (flags for future investigation) of issues such as various cooking methods (roasting or boiling rather than only fresh bones), different implements for breakage, effect of prior periosteum removal (noted in several ethnographies) and breakage function being directed at grease rendering, bone meal, craft activity or bone fuel etc. instead of marrow extraction alone.

In places, some idiomatic edits could be made to the use of English.

*Reviewed by anonymous reviewer, 2020-05-01 09:48*

In this important paper, Vettese and colleagues argue that there is an intuitive way to break a bone in order to obtain its marrow. They employ unexperienced people to break the bones. Then, by applying GIS method to identify percussion mark concentrations, they point to the existence of bone morphology constraints and thus to an intuitive way to break the bones. The manuscript is very interesting and the fundamentals of the work – i.e. the experiment execution and the taphonomic and GIS analyses – are of high quality. The methods and data

presentation are mostly detailed and the conclusions are justified by the data. I recommend publication if two main issues are taken care of:

- 1) The paper is not clearly written and thus, very difficult to follow. I strongly recommend subjecting the text to professional language editing.
- 2) Every figure and table (including supplementary) must be independently understandable and the legend should include a title, clear explanation, abbreviations and reference to the source of the data.

In addition, I have several suggestions and points to fix, detailed below.

Terminology – use of terms interchangeably (e.g., Individual - experimenter - volunteer). Please be consistent.

Figures 4–13 – n blows is needed at least for every bone if not in all elements.

Figures 4–5 – it is impossible to differentiate between the shapes and colors.

Figures 6–13 – 8 figures is way too much, at least half should removed to the supplementary.

Experimenter interviews – are missing, you could obtain a lot of supporting data (e.g. why did you hit/hold/use anvil etc.). Why didn't you ask those questions? If you did, please add the information where needed or add questionnaires to supplementary.

SI – references to SI are unclear (e.g. 286 "Supplementary Information 1, 2, 3 and 4") is it table or figure 1? And be more precise, you can't refer to 4 figures). Some figures referenced in the text are missing (358–359 Supplementary Information 6).

Abstract

Too long (500 words)

35–36 – "Indeed, a previous study was able to show the possible existence of intuitive patterns of distribution of these traces according to the elements" – then I expect an explanation what have you added on it.

43 – what is innovative in the method?

Introduction 72–75 - wasn't noted before 2012?

79 – after 1992 - ) missing

101–102 – "The definition of a butchery tradition is a systematic and counterintuitive pattern shared by a same group" – First, add reference or detailed explanation. Second, why must a tradition be counterintuitive? If a group applies systematic and intuitive patterns for generations, you will not define it as tradition?

106–107 – ditto.

Material and Methods When and where were the experiment carried out?

Is it a different experiment then the one reported in Stavrova et al. 2019?

145 – Shouldn't the protocol be a part of this paper? Why put it separately?

153 – size = height?

162 – of each experimenter or each bone?

183–186 what is the difference from Marean and Spencer 1991? Why reinvent the wheel? If not it, why not following other known methods which compatible to other research? (e.g. Dobney and Rielly 1988; Lam et al 1999 ect.) and if using new one, why not following Saladié et al 2011 which have done the same based Pales?

Moreover, Castel and Grunwald are missing in the bibliography and anyhow referencing a private website is unacceptable.

241 – Figure with photos will be very helpful. Consider adding videos like Camaros et al 2013 for example. Camaros, E., Cueto, M., Teira, L.C., Tapia, J., Cubas, M., Blasco, R., Rosell, J., Rivals, F., 2013. Large carnivores as taphonomic agents of space modification: an experimental approach with archaeological implications. *Journal of Archaeological Science* 40, 1361–1368 <https://doi.org/10.1016/j.jas.2012.09.037>

256–260 – may be partly belong to the discussion? clarify

276 – général - change to English

280–281 - isn't it expected since it contains the lowest amount? Shouldn't it be the amount of the possible marrow to extract? See line 349 below too.

286 – too general.

289 – what is the title of this table?

300–301 – discussion?

316 – beginning In summary?

320–322 – discussion.

323 – how do you infer it out of table 5?

341 – percentage or numerical? Consistency.

349 – You calculate efficiency by blows to marrow weight, but since each bone type (i.e. femur, radius) contains a different amount of marrow to begin with, your index should be normalized by bone marrow content.

357–358 "the Wicoxon signed rank test did not show a significant difference between the first five and the last five tries or between the first three and the last three tries." There was no significant difference between the first five and the last five tries or between the first three and the last three tries (Wicoxon signed rank test;  $p < 0.05$ ).

358 – SI table or figure? Anyhow it is missing.

366 - add number of other elements

370 – SI figure 4, should rotate the figure properly.

375 - SI 7 missing

Bone fragmentation – I miss fragment length which is a very common index in Paleolithic archology.

434–436 why did you separate the different PM?

453 – omit parentheses

455–456 you can make it easier for the reader and refer to figure 6c.

Discussion It is improper adding new information that was not mention in the results – e.g. 1 526–528; Some volunteers applied reduced force when they hit the bones, especially during the first attempts. e.g. 2 536–539; it is important to note that sometimes the hammerstone slipped on the cortical surface or slipped out of the experimenter's hand. This happened especially when the hammerstone and hands were covered in grease after several tries. When an individual broke several

bones, he/she had to clean the tools or his/her hands to pursue marrow extraction.

566 - it is the assumption of the observer or they actually said it?

References Consistency - choose your preferred bibliographic style and follow it e.g. Is it ordered later-earlier or the opposite? See Binford 1981, 1978 but Costamagno 1999, 2009, 2014. While Blasco 2014, 2012, 2013?!

Castel appear twice.

Stavrova et al 2019 is missing.

922 - Vettese - different font.

*Author's reply:*

[Download author's reply \(PDF file\)](#)