



together with the analysis of the distribution of the engraved lines argue convincingly for an intentional engraving. This article constitutes a key contribution to the question of HOW emerged modern cultures in Southern Africa, while calling for further research related to sites' function, environment and local resources to address the ever-debated question of WHY the MSA groups from Southern Africa developed such unprecedented inventiveness. It makes no doubt that this article deserves recommendation by PCI Archaeology.

[1] Porraz, G., Schmidt, P., Bereiziat, G., Brugal, J.Ph., Dayet, L., Igreja, M., Miller, C.E., Viola, C., Tribolo, C., Val, A., Verna, C., Texier, P.J. 2020. Experimentation preceding innovation in a MIS5 Pre-Still Bay layer from Diepkloof Rock Shelter (South Africa): emerging technologies and symbols. [10.32942/osf.io/ch53r](https://doi.org/10.32942/osf.io/ch53r)

[2] Porraz, G., Texier, P.J., Archer, W., Piboule, M., Rigaud, J.P, Tribolo, C. 2013. Technological successions in the Middle Stone Age sequence of Diepkloof Rock Shelter, Western Cape, South Africa. *Journal of Archaeological Science* 40, 3376–3400. [10.1016/j.jas.2013.02.012](https://doi.org/10.1016/j.jas.2013.02.012)

[3] Porraz, G., Texier, J.P. Miller, C.E., 2014. Le complexe bifacial Still Bay et ses modalités d'émergence à l'abri Diepkloof (Middle Stone Age, Afrique du Sud). In: XXVIIème Congrès Préhistorique de France, Transitions, Ruptures et Continuité en Préhistoire. Mémoires de la Société Préhistorique Française, 155–175.

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#### Revision round #1

2020-11-11

The three reviewers provide highly meaningful and complementary comments aiming at enhancing this paper, which is undoubtedly a key paper for the understanding of the Still Bay and Pre-Still Bay complexes from Southern Africa. We specifically recommend the authors to enrich the literature review regarding processes of innovation and cultural transmission, as suggested by E. Spinapolice and F. d'Errico. We also fully agree with the need to summarize the contextual data in the paper or to move most of them in a Supplementary data section, as expressed by these two reviewers. By keeping the most relevant new data in the main text, the paper will gain greater clarity and highlighting of the research progress in relation to previous publications. The titles should also better reflect the sections' content, as is the case for "Discoveries and legacies". As pointed by F. d'Errico, the description, interpretation and illustration of the engraved bone require further evidence and demonstration; please do refer to his advices to strengthen this part. We also ask the authors to consider and answer the very relevant questions raised by Kathryn Ranhorn. We finally recommend the authors to better connect the conclusions of the paper with its introduction as suggested by E. Spinapolice. Once these minor modifications are taken into account, there is no doubt that this paper deserves to be highly recommended.

Preprint DOI: [10.32942/osf.io/ch53r](https://doi.org/10.32942/osf.io/ch53r)

Reviewed by [Francesco d'Errico](#), 2020-11-05 18:28

The Middle Stone Age of Southern Africa has over the last twenty years become a key laboratory for understanding the emergence of modern cultures and proposing hypotheses on the mechanisms that have promoted cultural and technological changes. Any new milestone in this journey must be welcomed by the scientific community, all the more so if it comes from an important site such as Diepkloof, which has long been the object of systematic and high-quality excavations by a reputed, international, multidisciplinary team. I therefore have no doubt that the subject of the manuscript by Porraz and collaborators is of great interest and that the discoveries presented are worthy of publication. However, the article could be greatly improved if some changes were introduced. Some of these modifications, rebalancing and clarifications, seem to me essential before

recommendation by PCI Archaeology. Others would, in my opinion, help to improve the article, but their introduction is not an obstacle to its publication. I will list these suggestions for change, following as much as possible the development of the manuscript.

Summary (with implications for the rest of the manuscript). - The authors describe the incisions on the bone as "cross-hatched markings" but this description does not correspond to the definition of cross-hatched: (in drawing or graphics) shade (an area) with intersecting sets of parallel lines. On the contrary, the identified pattern corresponds perfectly to the definition of a rhomboid or even better the definition of an irregular rhomboid, considering the different lengths of the sides. I suggest that this change is introduced in the abstract and the text of the article.

- In the abstract and in main text the authors argue that cultural innovations leading to modern cultures would only appear in the region from MIS5 onwards and would reach a higher degree of complexity at the end of MIS5 (Still Bay) and at MIS4/3 (HP). In reality some innovations, such as the use of ochre, seem to have an older origin and would go back to MIS9s (e.g. Watts et al. CA). This earlier age is consistent with other discoveries made elsewhere in Africa (for ex. in Kenya and Zambia). It has also been suggested that hafting of points may have occurred quite early in time, e.g. at Kathu Pan. In my view the above evidence should be mentioned and discussed in the introduction and the discussion. It is not simply a question of age or missing evidence, but of when the process that would ultimately lead to greater cultural complexity started. The main argument of the paper is that the material culture found in layer Lynn represent the prolegomena of the cultural complexity found in the Still Bay. However, if one accepts that elements of modernity arose at the very beginning of the MSA what we find in layer Lynn represent just a step within a much longer process. Accepting a longer chronology for this process would imply that the earliest symptoms of modernity coincide with the emergence of some modern anatomical characters. Discarding their modern nature would mean disconnect the gradual process toward anatomical modernity, accepted now by most paleanthropologists as the bestfit model for the emergence of our species in Africas, from the process leading to cultural modernity, that would occur in a much shorter time span. These issues need to be discussed and one, of course, needs to cite the evidence to do that.
- Use in the summary and in the manuscript Southern Africa (geographical region) rather than South Africa (political entity).

Page 3. They used personal ornaments, captured felids for their fur and engraved utilitarian and non-utilitarian objects, illustrating their abilities to store information and to communicate with ornaments and graphic designs (e.g. Henshilwood et al, 2002, 2004; Texier et al, 2010; Val et al, 2020).

It is unclear to me in what the use of furs supports the abilities the authors mention and why they put furs, engravings and personal ornaments in the same basket. It seem to me that we find in the basket objects of very different nature. As for the personal ornaments and engravings it would be better to cite the more recent literature, which provide an update on the dimension and complexity of the phenomenon, rather than old references.

Page 3. The oldest evidence of these new practices includes notching at the site of Klasies River (d'Errico et al., 2012). The paper cited does not present any data on Klasies River. The authors should rather cite Singer and Wymer 1982 or d'Errico and Hensilwood JHE 2007.

Page 4. This section is of obvious interest but if the authors decide to embrace such a broad topic they should als, at least mention, to avoid the criticism of being parochial, some of the work of Joseph Heinrich, Alex Messoudi, Andrew Whiten, Kevin Laland.

Page 7. The legend of figure 2 only partially corresponds to what is presented in this figure. The legend does not mention the photo and the tracing of the bone. It would be preferable to present the bone and the tracing in a separate figure (see additional comments about this below).

Pages 5-27. Contextual information is essential to the publication of an object like the one presented in this study and I acknowledge that this paper is not only about the incised bone, but I feel that too much emphasis has been

placed on this section which, by the authors' own admission, is a section devoted to provide context, not results. Out of a text of 42 pages, more than half (22 pages), comprising 8 figures and 5 tables, concern the archaeological context. To this must be added 5 supplementary pages (pages 32-37) following the presentation of the results on the incised bone, that concern the heating of lithics and the strategies of débitage. It is a lot compared to less than two pages and one figure about the analysis of the incised pattern on the bone object. Some of the information given in these 22 pages is already published elsewhere, much of the contextual information given in the section intended for context is extremely detailed and is not used in the discussion of the results. All in all, the reader has the feeling of being faced with two articles, one presenting a synthesis of what is known about the site and in particular about the Lynn layer, and one concerning the discovery and analysis of the incised bone. In order to avoid this problem it would be preferable to synthesise the section on context and possibly include some of the information given in it in supplementary on-line materials. The section on the analysis of the object, on the other hand, should be expanded (see below).

Page 28. The section on the anatomy, taxonomy and taphonomy of the bone is excellent but the description and study of the incisions deserve a better iconography and a more in-depth analysis. This need is all the stronger as when looking at the photos some microscopic features may be interpreted in a different way from how they are interpreted by the authors and some key evidence is missing.

As far as iconography is concerned, the only photo showing the incisions in this section is a photo of the whole bone. It is the same photo presented in one of the figures in the section on the archaeological context, but in the photo presented in the results section the engravings are even less visible because rectangles have been superimposed on them to identify the four areas photographed at the SEM. On the other hand, the SEM photos are too small to identify with certainty diagnostic features. I consider adding a good macro-photo of the incised pattern, allowing the reader to get a precise idea of it, essential.

Page 28. The authors state: "Two incisions (1' and 2') indicate that the fragment was part of a larger engraved bone piece: these two incisions are included within the geometric motif with the line 1' being parallel to 1 and the line 2' being parallel to 4. This could indicate that the cross-hatched pattern was initially part of a more complex pattern of lines." Considering the very short length of these lines, located near the fractures, the hypothesis that the engraving was spread over a much larger area seems speculative to me. The authors should take into account the possibility that these lines were the result of the passage of the same tool, i.e. that the lines considered by the authors as independent are the result of a simple change of contact of the active part of the tool producing the incisions that form the rhomboid. I have no way from the iconography presented to choose between these two hypotheses. The authors may wish to present additional photos to support their hypothesis, for example by showing with SEM or good microscopic photos that the tool used to produce the additional broken line was the same used to produce the line of the rhomboid close to it. Based on the presented data, the reasons why they lean towards the hypothesis of a more extensive pattern rather than the more parsimonious hypothesis of a simple change of contact caused by the curvature of the bone are unclear.

Page 28. The authors state: "The chronology (1 to 4) and the directions of the four incisions framing the geometric shape suggest the pattern was made in a continuous and counter-clock-wise rotating motion." This statement needs to be further substantiated. It is understandable that the chronology of the incisions is inferred from the analysis of the crossings of lines, but what criteria did the authors use to establish the direction of the lines? No bibliographical reference is given to support this assertion, nor is reference made in the text to morphological features visible in the SEM photos. In a SEM photograph arrows indicate "indentations" but no reference is made to these in the text to infer a direction of movement nor it is explained how the morphology of these indentations indicated a direction of movement. Features to infer direction of movement in incisions made on bone and stone exist in the literature but they are quite different from the indentations on the incisions on the Diepkloof bone.

Page 28. The authors state: the incisions present an asymmetrical V-section (Fig. 9, SEM B), indicating that they originate from contact with the sharp and raw (unretouched) edge of a stone tool. This is another statement that needs to be better substantiated. An engraving made with a point very often produces a line with an asymmetrical V-section. Thus, this is not a criterion to identify the use of a cutting edge. How can the authors say that in this case the line was produced by an unretouched edge rather than by a point? The question arises all the more because the same authors point out that some lines have a sinuous appearance, which would argue in favour of a point rather than an edge, and by the fact that some sudden changes of direction, corresponding in some cases with discontinuities in the bone surface morphology, visible in all SEM photos of figure 9, contradict

the hypothesis that the incisions were made by an edge and rather indicate the use of a point. The issue of the identification of the tool responsible for the production of the incisions may appear trivial but is not. When using a point the engraver will have a better control of the starting and ending of the incision, which will be critical to precisely join lines of a geometric figure as the rhomboid present on the Diepkloof bone. When using a cutting edge the engraver will have more difficulties to stop the incision at a given point as the point of contact between the cutting-edge and the bone surface will vary during the work and it will be more difficult to visually identify where the point is engraving the surface. If a point was used in this case, as some clues indicate, and the goal of the engraver was that of producing a rhomboid, why the engraver did not precisely start/stop the line he/she was engraving at the point of contact with a previously engraved line? This is a key issue that needs to be discussed in the paper.

Page 31-32. The authors state: “The incisions are deep, they have a smooth aspect and they are slightly undulating (Fig. 9, SEM A: incision 1’, SEM D: incision 1). This is characteristic of striae made on fresh bones with probably still fresh periosteum covering the surface.” Of the three features mentioned (depth, smoothing, undulations) only the second may be considered diagnostic to propose that the incisions were made on fresh bone. However, some more diagnostic features visible in the SEM photos contradict this hypothesis. The edges of the incisions in SEM photos A, B and C are fringed and the presence of the indentations rather suggest that the bone was not fresh when the incisions were made.

Page 31. The authors state: “Several arguments encourage us to reject the hypothesis that the incisions are randomly organized and that they result from butchering activity”. I agree that the incisions do not easily fit the cut-marks interpretation. However, more evidence may have been put forward to support this view. After all, if the incisions were made by a cutting edge (and not a point as one can suspect) and on fresh bone, as the author say (but we showed above that this is also unsure), which mean what one would expect when producing butchery marks, how can the authors discard the possibility that we are not just facing a case in which two subparallel cut-marks, exerted in two different directions, happened to be superimposed on a bone? In order to (partially) discard this hypothesis the authors may have shown that the the four main lines were produced by the same tool in rapid succession. The well preserved internal morphology of the incisions, visible on the SEM photos they provide, may have been instrumental to do that. The issue of the use of the same vs different tools is not mention at all in the description of the incisions not clear photos are presented to tackle this key topic.

In conclusion, I think that the section on the analysis of the incisions needs rewriting and the presentation of a better and more demonstrative iconography.

Page 37. “The engraved bone from the pre-SB Lynn coincides chronologically with the oldest engraved ochre pieces from Blombos Cave and Klasies River, but differ from other more recent engraved bones from South Africa where parallel and perpendicular incisions dominate (d’Errico and Henshilwood, 2007; d’Errico et al., 2001; 2012; Henshilwood et al., 2009)” Can the author add a reference for the engraved ochre piece from Klasies?

Page 32-49. I found this section well written and interesting. There is however an issue that I think the authors have not considered and that should be integrated in the discussion. According to the cultural exaptation scenario one would expect that the pattern incised on the Diepkloof bone would give raise in some way to more complex and consistent cultural traits in the same behavioral domain, which apparently is not the case at Diepkloof before the HP. How do the authors explain this substantial gap? Should we see the pattern, and perhaps other inventions, as behaviours that have not become innovations? Or perhaps this transition from invention to innovation (accepted by a community) happened only in some area of the Still Bay and not in others? It is an important issues. We have to envision continuities but also possible discontinuities in cultural transmission as we are dealing with cultural traits, which can be lost.

*Reviewed by [Enza Elena Spinaplice](#), 2020-10-20 09:39*

Experimentation preceding innovation in a MIS5 Pre-Still Bay layer from Diepkloof Rock Shelter (South Africa): emerging technologies and symbols

The paper “Experimentation preceding innovation in a MIS5 Pre-Still Bay layer from Diepkloof Rock Shelter (South Africa): emerging technologies and symbols” by Porraz and colleagues presents the results of the excavation of the pre- SB Lynn unit in terms of cultural innovation.

I admire the work that has been done on Diepkloof Rockshelter in the last years, and I think this is the perfect context to test for cultural transmission models, and the US Lynn, with its short occupation, is ideal in this sense.

The paper is well written, and I enjoyed reading it. My remarks concern the ways to make it better, i.e. more useful for the scientific community and clearer in its theoretical background.

First, the structure of the paper is a bit confusing: the method section is summarized at the end, and the results are presented straight after the chapter that presents the site. The section “Discoveries and legacies” sounds obscure to me. Why not simply use “Discussion”? This unusual structure confuses the reader and this is coupled with the maybe excessive length of the paper. Some restructuring of the index may help, or in alternative, a clear statement about the structure and goals in the introduction.

The tracking of cultural evolution through processes of innovation and cultural transmission is an extremely interesting and powerful topic in the area of human evolution nowadays. Despite it is largely acknowledged that this path is worth exploring, methods of reconnaissance of cultural transmission are still very variable. A remark I wish to address is that for such an innovative attempt, associated with a well-known and rich site, the literature review on this topic is poor. Classic papers regarding cultural transmission, such as Mesoudi 2016, are not cited. Other papers worth a mention could be Premo and Solnik 2011, Foley and Lahr 2011, Collard et al 2005, Charbonneau 2015, 2016, Di paolo and Di Vincenzo 2018. Furthermore, a Springer book on cultural convergence, full of interesting contribution about MSA has recently been published (Groucutt “Culture history and convergent evolution: Can we detect populations in prehistory?” 2020) and in my opinion, some contributions are worth to be mentioned in the paper.

The work on lithic technology is of high level and high impact. I have a question about the seven chaînes opératoires described: some of them seem to emerge from few pieces, furthermore they are poorly illustrated. Why the authors did not attempt to connect some of them and identify a lesser number of reduction sequences? Or did they and still this classification stands? In this case, let the reader know. The incompleteness of the chaînes opératoires, as illustrated in the raw material economy chapter also could justify a low resolution of the understanding of the various steps. It is interesting the inclusion of the pieces esquillées among the formal tools, however this is hard to assess for a reader, because the determining criteria are not clear, and the illustration are few. The relationship between reduction sequences and raw material economy emerges very well from the paper.

The analysis on the bone engraving is clear and convincing (however, somehow long), as is the discussion about the engraving in South Africa.

In my opinion, the weakest part of the paper is the final one, with the attempt to organize the innovations of the Lynn’s layer. The connection between the engravings and the innovation in the lithic technology stays at the level of a suggestion. The model about human group structure and mobility it is very promising and could be better stated and associated with larger literature review. I would give more space to this part in the paper, and better connect it to the introduction, as anticipated in the title. And I would shorten the results on lithics and bones.

I overall think that PCI archaeology is the right place to submit this paper, for its originality, and warmly hope that my suggestions may help to improve the final version of this work.

*Reviewed by [Kathryn Ranhorn](#), 2020-11-02 21:30*

[Download the review \(PDF file\)](#)

### Author's reply:

## Decision

\* by Anne Delagnes, 2020-11-11 09:05 Manuscript: 10.32942/osf.io/ch53r\*

### To be recommended after minor revisions

The three reviewers provide highly meaningful and complementary comments aiming at enhancing this paper, which is undoubtedly a key paper for the understanding of the Still Bay and Pre-Still Bay complexes from Southern Africa. We specifically recommend the authors to enrich the literature review regarding processes of innovation and cultural transmission, as suggested by E. Spinapolice and F. d'Errico. We also fully agree with the need to summarize the contextual data in the paper or to move most of them in a Supplementary data section, as expressed by these two reviewers. By keeping the most relevant new data in the main text, the paper will gain greater clarity and highlighting of the research progress in relation to previous publications. The titles should also better reflect the sections' content, as is the case for "Discoveries and legacies". As pointed by F. d'Errico, the description, interpretation and illustration of the engraved bone require further evidence and demonstration; please do refer to his advices to strengthen this part. We also ask the authors to consider and answer the very relevant questions raised by Kathryn Ranhorn. We finally recommend the authors to better connect the conclusions of the paper with its introduction as suggested by E. Spinapolice. Once these minor modifications are taken into account, there is no doubt that this paper deserves to be highly recommended.

Dear Anne Delagnes,

Thank you very much for recommending our paper to PCI after revisions. You will find below the detailed answers to each relevant point addressed by the three reviewers, that we thank very much. We acknowledge their comments and remarks and have modified the manuscript accordingly. We have notably enriched the literature review regarding processes of cultural innovation and cultural transmission, we have addressed the questions raised by Kathryn Ranhorn, and we have modified the description of the engraved bone, benefiting from the experienced and much valuable eyes of Francesco d'Errico. We have also modified the illustrations as recommended. However, we do not fully agree with the need to summarize the "contextual data", which has been recommended by Francesco d'Errico and Enza Spinapolice, but on two different lines. We acknowledge, as pointed out by Enza Spinapolice, that the structure of our paper could be improved and we have positioned the bone description chapter at the beginning of our paper, before the lithic technology chapter. We have also inserted the "Methods section" within the manuscript, as recommended by the same reviewer, and shorten the synthesis on the previously studied botanical and faunal remains. Our paper presents a newly discovered engraving from the Pre-SB that adds a new evidence on the set of novelties that characterize the MIS5 in Southern Africa. We also present a detailed set of other data on heat-treatment, bladelets and bifacial reduction sequences, adhesive residues, macrofractures on pointed forms, etc., which have never been published so far. These different results do not intend to simply provide an "archaeological background" to the engraved bone. Our main line of argumentation is precisely (1) not to separate the study of the bone from the study of other archaeological proxies and (2) to promote an historical scenario that include the full set of behavioral novelties that are recorded during that specific interval of time. We hope that the modifications we have provided are acceptable from your point of view and we do thank again very much the reviewers for their time and in-depth comments.

With our best wishes,

Guillaume Porraz and co-authors.

## Reviews

### Reviewed by Francesco d'Errico, 2020-11-05 18:28

- The Middle Stone Age of Southern Africa has over the last twenty years become a key laboratory for understanding the emergence of modern cultures and proposing hypotheses on the mechanisms that have promoted cultural and technological changes. Any new milestone in this journey must be welcomed by the scientific community, all the more so if it comes from an important site such as Diepkloof, which has long been the object of systematic and high-quality excavations by a reputed, international, multidisciplinary team. I therefore have no doubt that the subject of the manuscript by Porraz and collaborators is of great interest and

that the discoveries presented are worthy of publication. However, the article could be greatly improved if some changes were introduced. Some of these modifications, rebalancing and clarifications, seem to me essential before recommendation by PCI Archaeology. Others would, in my opinion, help to improve the article, but their introduction is not an obstacle to its publication. I will list these suggestions for change, following as much as possible the development of the manuscript. Summary (with implications for the rest of the manuscript). - The authors describe the incisions on the bone as "cross-hatched markings" but this description does not correspond to the definition of cross-hatched: (in drawing or graphics) shade (an area) with intersecting sets of parallel lines. On the contrary, the identified pattern corresponds perfectly to the definition of a rhomboid or even better the definition of an irregular rhomboid, considering the different lengths of the sides. I suggest that this change is introduced in the abstract and the text of the article.

*We do agree that the term "rhomboid" fits better the geometric form engraved on the bone and we have changed it accordingly in the abstract and in the text.*

- In the abstract and in main text the authors argue that cultural innovations leading to modern cultures would only appear in the region from MIS5 onwards and would reach a higher degree of complexity at the end of MIS5 (Still Bay) and at MIS4/3 (HP). In reality some innovations, such as the use of ochre, seem to have an older origin and would go back to MIS9s (e.g. Watts et al. CA). This earlier age is consistent with other discoveries made elsewhere in Africa (for ex. in Kenya and Zambia). It has also been suggested that hafting of points may have occurred quite early in time, e.g. at Kathu Pan. In my view the above evidence should be mentioned and discussed in the introduction and the discussion. It is not simply a question of age or missing evidence, but of when the process that would ultimately lead to greater cultural complexity started. The main argument of the paper is that the material culture found in layer Lynn represent the prolegomena of the cultural complexity found in the Still Bay. However, if one accepts that elements of modernity arose at the very beginning of the MSA what we find in layer Lynn represent just a step within a much longer process. Accepting a longer chronology for this process would imply that the earliest symptoms of modernity coincide with the emergence of some modern anatomical characters. Discarding their modern nature would mean disconnect the gradual process toward anatomical modernity, accepted now by most paleanthropologists as the bestfit model for the emergence of our species in Africa, from the process leading to cultural modernity, that would occur in a much shorter time span. These issues need to be discussed and one, of course, needs to cite the evidence to do that.

*We do acknowledge the importance of the question, which aims at promoting a longer-term evolutionary perspective. The SU Lynn and at large the MIS5 surely represents a step within a much longer evolutionary process. Yet, the present archaeological record suggests the MIS5 signs a major cultural step in the way populations were socially and economically structured.*

*We do acknowledge in our first chapter of introduction that the MIS5 does not record all the innovations but many of them ("The onset of many of these new practices took place independently during the Marine Isotopic Stage (MIS) 5 in northern and southern Africa."). We have added (Watts et al. 2016) in the generic list of references aiming at characterizing the MSA at the very beginning of the introduction and added a short paragraph in discussion to recall the importance of the question.*

*We do acknowledge that such historical discourse shall also promote a longer-term evolutionary perspective paying attention to earlier innovations. Ochre processing (Watts et al., 2016) and hafting technology (Wilkins et al. 2012, but see: Rots and Plisson, 2014) provide examples of early cultural changes, which enlarge the chronological frame in which we shall perceive the cultural evolution of AMHs societies. These early discoveries do not reduce the nature and importance of the set of behavioral changes recorded during the MIS5 in southern Africa, which marks a step in the way people were socially and economically organized.*

- Use in the summary and in the manuscript Southern Africa (geographical region) rather than South Africa (political entity).

*Change has been done accordingly.*

- Page 3. They used personal ornaments, captured felids for their fur and engraved utilitarian and non-utilitarian objects, illustrating their abilities to store information and to communicate with ornaments and graphic designs (e.g. Henshilwood et al, 2002, 2004; Texier et al, 2010; Val et al, 2020). It is unclear to me in what the use of furs supports the abilities the authors mention and why they put furs, engravings and personal ornaments in the same basket. It seem to me that we find in the basket objects of very different nature.



*As defended in Val et al. 2020, the hunting of nocturnal felids at Diepkloof fell outside of the nutritional sphere and was oriented toward the purpose of getting and processing furs. As documented in many hunter-gatherer societies, the wearing of carnivore furs is meant to be seen and incarnates a visual codification. We understand that the basket seems large to the reviewer, as other baskets (such as technology), whose intention is to go to the general and not the specifics. The papers that are mentioned all argue in favor of behavioral practices expressing a degree of abstract thought, that is why we have melted them in one basket.*

*We have changed the sentence as follow: "They perforated marine shells to be used as ornaments, exploited felids for their fur and depicted geometric forms on utilitarian and non-utilitarian objects, all diverse proxies illustrating the abilities of MSA groups to integrate and transform natural elements into a new culturally encapsulated environment (e.g. Henshilwood et al., 2002, 2004, 2018; Texier et al., 2010; Val et al., 2020; Vanhaeren et al., 2013)."*

- As for the personal ornaments and engravings it would be better to cite the more recent literature, which provide an update on the dimension and complexity of the phenomenon, rather than old references.

*We have updated the references as suggested (see quotation above):*

- Page 3. The oldest evidence of these new practices includes notching at the site of Klasies River (d'Errico et al., 2012). The paper cited does not present any data on Klasies River. The authors should rather cite Singer and Wymer 1982 or d'Errico and Henshilwood JHE 2007.

*We have changed the references accordingly.*

- Page 4. This section is of obvious interest but if the authors decide to embrace such a broad topic they should als, at least mention, to avoid the criticism of being parochial, some of the work of Joseph Heinrich, Alex Messoudi, Andrew Whiten, Kevin Laland.

*We have added the following references (see also comment of Reviewer 2): (see Henrich, 2015; Henrich and McElreath, 2003; Foley and Mirazon Lahr, 2016; Mesoudi, 2016; Mesoudi et al., 2004; Stuart-Fox, 2015)*

- Page 7. The legend of figure 2 only partially corresponds to what is presented in this figure. The legend does not mention the photo and the tracing of the bone. It would be preferable to present the bone and the tracing in a separate figure (see additional comments about this below).

*We have changed the legend and modified the Figure 2 accordingly, following as well the remark of Kathryn Ranhorn (reviewer 3): Figure 2. (1) Planimetry of Diepkloof Rock Shelter (in dark grey: squares where the SU Lynn has been excavated) and various photographs of the 2012-2013 excavations with detail (2) on the excavated area, (3) on the M-N7/M-N8 stratigraphic profile and (4) on the on the engraved bone at the time of its discovery in the Stratigraphic Unit (SU) Lynn.*

- Pages 5-27. Contextual information is essential to the publication of an object like the one presented in this study and I acknowledge that this paper is not only about the incised bone, but I feel that too much emphasis has been placed on this section which, by the authors' own admission, is a section devoted to provide context, not results. Out of a text of 42 pages, more than half (22 pages), comprising 8 figures and 5 tables, concern the archaeological context. To this must be added 5 supplementary pages (pages 32-37) following the presentation of the results on the incised bone, that concern the heating of lithics and the strategies of débitage. It is a lot compared to less than two pages and one figure about the analysis of the incised pattern on the bone object. Some of the information given in these 22 pages is already published elsewhere, much of the contextual information given in the section intended for context is extremely detailed and is not used in the discussion of the results. All in all, the reader has the feeling of being faced with two articles, one presenting a synthesis of what is known about the site and in particular about the Lynn layer, and one concerning the discovery and analysis of the incised bone. In order to avoid this problem it would be preferable to synthesise the section on context and possibly include some of the information given in it in supplementary on-line materials. The section on the analysis of the object, on the other hand, should be expanded (see below).

*We do not agree with the general comment that the technological data is meant to provide an archaeological context to the engraved bone, but do acknowledge a mistake in the way of explaining our goals and research perspective. The reserves of*

*Francesco d'Errico finds a partial echo with the comments of Enza Spinapolice (but see answers to Reviewer 2) and an opposite echo with the comments of Kathryn Ranhorn.*

*So far, two papers have dealt with the SU Lynn. This concerns the Porraz et al. 2013 paper where a chapter composed of five short paragraphs (p. 3382-3383) introduce the main techno-economical characteristics of the SU Lynn. And this concerns the Porraz et al. 2014 paper, where the focus is oriented toward the scenario of technological continuity observed from the MSA Mike to the SB Logan. Yet, the present paper introduces new collections (a sample of 3,5m<sup>2</sup>, while only 2m<sup>2</sup> were studied before) and new studies and collaborations that demonstrate new results, such as silcrete heat-treatment, bladelet production, adhesive confection and bifacial manufacture. All these discoveries fuel our discussion on “experimentation” and are not meant to provide an “archaeological context” to the engraved bone, which -in our perspective- represents one novelty amongst others.*

*Following as well the comments of Enza Spinapolice, we have positioned the bone description chapter at the beginning of our paper, before the lithic technology chapter, and have clarified the goals of our paper in the last chapter of introduction: “In the present paper, we develop a model based on the long and well-stratified MIS5-4 sequence of Diepkloof Rock Shelter (subsequently Diepkloof) in South Africa. We present new results from the Stratigraphic Unit (SU) Lynn that immediately precedes the SB at the site. Our new data from the Pre-SB Lynn document a set of innovative behaviors that were yet not recognized (Porraz et al., 2013; 2014), such as the engraving of geometric form(s), the heat-treatment of silcrete, the production of bladelets, the manufacture of bifacial pieces and the use of adhesive. At Diepkloof, all these novelties appear and aggregate to each other’s for the first time during this single chrono-stratigraphic event. We defend that later innovations characterizing the SB and the HP were then already in the making during the MIS5 pre-SB, though innovations were manipulated differently through time. We argue that these new practices appeared in a context of socio-economic change that fostered experimentation -i.e. the benefit of testing new ideas and of gaining experiences out of new practices- as a cultural answer to face new circumstances.”*

- Page 28. The section on the anatomy, taxonomy and taphonomy of the bone is excellent but the description and study of the incisions deserve a better iconography and a more in-depth analysis. This need is all the stronger as when looking at the photos some microscopic features may be interpreted in a different way from how they are interpreted by the authors and some key evidence is missing. As far as iconography is concerned, the only photo showing the incisions in this section is a photo of the whole bone. It is the same photo presented in one of the figures in the section on the archaeological context, but in the photo presented in the results section the engravings are even less visible because rectangles have been superimposed on them to identify the four areas photographed at the SEM. On the other hand, the SEM photos are too small to identify with certainty diagnostic features. I consider adding a good macro-photo of the incised pattern, allowing the reader to get a precise idea of it, essential.

*We have modified the iconography as suggested. We have removed the engraved bone from Figure 2 and created two Figures from Figure 9 (which are now Figures 3 and 4). Figure 3 presents a high resolution macro picture of the engraved bone and Figure 4 shows detailed SEM images (at a larger size than those previously reported) of the engravings with their location on the engraved bone. The SEM pictures are detailed in the answers below.*

*We also would like to highlight the fact that within the submission process to PCI, as the paper had to be reduced before submission to the preprint website, it might have altered the “zooming” directly on the computer files.*

- Page 28. The authors state: “Two incisions (1' and 2') indicate that the fragment was part of a larger engraved bone piece: these two incisions are included within the geometric motif with the line 1' being parallel to 1 and the line 2' being parallel to 4. This could indicate that the cross-hatched pattern was initially part of a more complex pattern of lines.” Considering the very short length of these lines, located near the fractures, the hypothesis that the engraving was spread over a much larger area seems speculative to me. The authors should take into account the possibility that these lines were the result of the passage of the same tool, i.e. that the lines considered by the authors as independent are the result of a simple change of contact of the active part of the tool producing the incisions that form the rhomboid. I have no way from the iconography presented to choose between these two hypotheses. The authors may wish to present additional photos to support their hypothesis, for example by showing with SEM or good microscopic photos that the tool used to produce the additional broken line was the same used to produce the line of the rhomboid close to it. Based on the presented data, the reasons why they lean towards the hypothesis of a more extensive pattern rather than the more parsimonious hypothesis of a simple change of contact caused by the curvature of the bone are unclear.

*We do thank Francesco d'Errico very much for the comment and the line of argumentation to follow up. We do acknowledge reserves with the possibility -for us- of identifying whether the engravings were done with the same tool or not. At best, we can*

say that the lines present similar cross-sections, which argue in favor of similar working edges. But we have clarified our line of argumentation and provide larger SEM pictures (Figure 4).

We have modified the text accordingly: “The SEM observations (Fig. 4, SEM B, C, D, F) document an abrupt termination of lines 1, 1', 2' and 3, which is due to the breakage of the bone. This indicates (1) that the selected bone that was engraved was bigger than the specimen presently studied and (2) that the engravings had a larger extension than we can observe now. In particular, we suggest that the incisions 1' and 2' are part of a larger and possibly more complex geometric motif, with line 1' being parallel to 1 and line 2' being parallel to 4. We acknowledge that parallel incisions could be the result of a reiterated passage of the same tool, as it is possibly the case for line 3 (see Fig. 4, SEM F), although small lateral striations are sometimes present on single and unique cut-marks. Line 2' extends beyond line 4 and both incisions are separated by 0.43 mm. They could correspond to two distinct motions, but they could also be consistent with a single gesture interrupted by a change in the point of contact between the tool and the bone, something that is well documented for engravings on pebbles and bones (d'Errico 1994; d'Errico and David 1993). Line 1 and line 1' are relatively far apart (1.19 mm) and we suggest that they correspond to two distinct motions, rather than to the reiterated passage of the same tool.”

- Page 28. The authors state: “The chronology (1 to 4) and the directions of the four incisions framing the geometric shape suggest the pattern was made in a continuous and counter-clock-wise rotating motion.” This statement needs to be further substantiated. It is understandable that the chronology of the incisions is inferred from the analysis of the crossings of lines, but what criteria did the authors use to establish the direction of the lines? No bibliographical reference is given to support this assertion, nor is reference made in the text to morphological features visible in the SEM photos. In a SEM photograph arrows indicate "indentations" but no reference is made to these in the text to infer a direction of movement nor it is explained how the morphology of these indentations indicated a direction of movement. Features to infer direction of movement in incisions made on bone and stone exist in the literature but they are quite different from the indentations on the incisions on the Diepkloof bone.

*Indeed, the chronology of the incisions is inferred from the crossing of the lines. On that regard, we have added references that we used as comparatives. Within the text, it is true that we don't explain how we have inferred the directions from the reading of the lines and from the indentations. We have modified the text accordingly: “We reconstructed the chronology of the incisions, from 1 to 4, as well as their directionality based on how they intersect each other, on the presence of small indentation on one edge and based on the morphology of their extremities (following for instance: Bromage and Boyde, 1984; d'Errico et al., 1994; Fritz, 1999; Texier et al. 2010) (Fig. 4). Experimental work suggests that the starting point of a line tends to be deep and narrow, while the distal extremity tends to be larger (“fan-shape”) and more superficial (e.g. de Araujo Igreja, 1999; d'Errico et al. 1994; Fritz, 1999). In addition, we considered the morphology of the indentations that characterize the external surface of the bone at the contact with the incised lines. When they occur, those indentations present asymmetric angles that we have used to infer the direction of the lines. We propose that the engraver produced the lines in a continuous rotational way, rather than as two distinct sets of parallel lines.”*

- Page 28. The authors state: the incisions present an asymmetrical V-section (Fig. 9, SEM B), indicating that they originate from contact with the sharp and raw (unretouched) edge of a stone tool. This is another statement that needs to be better substantiated. An engraving made with a point very often produces a line with an asymmetrical V-section. Thus, this is not a criterion to identify the use of a cutting edge. How can the authors say that in this case the line was produced by an unretouched edge rather than by a point? The question arises all the more because the same authors point out that some lines have a sinuous appearance, which would argue in favour of a point rather than an edge, and by the fact that some sudden changes of direction, corresponding in some cases with discontinuities in the bone surface morphology, visible in all SEM photos of figure 9, contradict the hypothesis that the incisions were made by an edge and rather indicate the use of a point. The issue of the identification of the tool responsible for the production of the incisions may appear trivial but is not. When using a point the engraver will have a better control of the starting and ending of the incision, which will be critical to precisely join lines of a geometric figure as the rhomboid present on the Diepkloof bone. When using a cutting edge the engraver will have more difficulties to stop the incision at a given point as the point of contact between the cutting-edge and the bone surface will vary during the work and it will be more difficult to visually identify where the point is engraving the surface. If a point was used in this case, as some clues indicate, and the goal of the engraver was that of producing a rhomboid, why the engraver did not precisely start/stop the line he/she was engraving at the point of contact with a previously engraved line? This is a key issue that needs to be discussed in the paper.

We do thank you very much Reviewer 1 for his comment and do agree on the whole line of his argumentation. We have modified the text pushing up the hypothesis of the point of a tool being used, rather than its edge. We acknowledge the fundamental part of the question related to why the engraver did not precisely start/stop at the point of contact with previous lines. However, we hardly find direct implications for our present argumentation. At most can we say that it seems to be the fact for most (if not all) of early MSA geometric forms found from southern African.

We have modified the text accordingly: “The incisions have a general asymmetrical V-section (Fig. 4, SEM A, C, F), indicating that they originate from contact with the sharp and raw (unretouched) active part of a stone tool. However, the incisions 3 and 4 (Fig. 4, SEM D and F) are partially indented, suggesting they may originate from contact with an active part of a tool that was partially damaged or retouched. The slightly sinuous appearance of some lines and changes in the point of contact between the tool and the bone surface (e.g. line 2), which could relate to irregularities of the bone surface, are features suggesting that the active part used to engraved the bone may have been a point rather than an edge (d’Errico et al. 1994). Lines 2 and 3 are long, rectilinear incisions, both surrounded by smaller, ancillary, incisions (Fig. 4) consistent with a shoulder effect (e.g. Shipman and Rose, 1983).”

- Page 31-32. The authors state: “The incisions are deep, they have a smooth aspect and they are slightly undulating (Fig. 9, SEM A: incision 1’, SEM D: incision 1). This is characteristic of striae made on fresh bones with probably still fresh periosteum covering the surface.” Of the three features mentioned (depth, smoothing, undulations) only the second may be considered diagnostic to propose that the incisions were made on fresh bone. However, some more diagnostic features visible in the SEM photos contradict this hypothesis. The edges of the incisions in SEM photos A, B and C are fringed and the presence of the indentations rather suggest that the bone was not fresh when the incisions were made.

We agree with Reviewer 1 and have tuned down our hypothesis. We modified the text accordingly: “SEM observations show that the cortical surface of the bone as well as the incisions are relatively fresh. The latter are well defined and well preserved, confirming the minimal impact of post-depositional damage on the engraved bone surface. The incisions are deep, relatively linear and present a smooth aspect, but are also slightly fringed and indented (Fig. 4, SEM A, D, E, F). These diagnostic features support the hypothesis that the bone was semi-fresh/partly dried at the time of its engraving. “

- Page 31. The authors state: “Several arguments encourage us to reject the hypothesis that the incisions are randomly organized and that they result from butchering activity”. I agree that the incisions do not easily fit the cut-marks interpretation. However, more evidence may have been put forward to support this view. After all, if the incisions were made by a cutting edge (and not a point as one can suspect) and on fresh bone, as the author say (but we showed above that this is also unsure), which mean what one would expect when producing butchery marks, how can the authors discard the possibility that we are not just facing a case in which two subparallel cut-marks, exerted in two different directions, happened to be superimposed on a bone? In order to (partially) discard this hypothesis the authors may have shown that the four main lines were produced by the same tool in rapid succession. The well preserved internal morphology of the incisions, visible on the SEM photos they provide, may have been instrumental to do that. The issue of the use of the same vs different tools is not mention at all in the description of the incisions not clear photos are presented to tackle this key topic.

We agree with Reviewer 1 that it is a critical part of the demonstration and that we shall open it to a wider range of arguments. As answered earlier, we agree with the fact that the engravings may likely have been produced with the use of a point and do consider the fact that the engraving may have occurred on a “dried” bone. Those two arguments, as suggested, reinforce the hypothesis that the engravings were not done at a time of butchery.

Considering the criteria of the engraver having used one or several tools, we briefly answered earlier to that hypothesis.

We recall as well that we don’t consider the motif to be only composed of two sets of two parallel lines, but that the engraved bone was initially much bigger, with as well a more extended motif. We do also include this data within our argumentation rejecting the “butchery hypothesis”.

We have modified the text accordingly: “Several arguments encourage us to reject the hypothesis that the incisions are randomly organized and that they result from butchering activity. On the contrary, several features support the intentional character of the engravings. Using actualistic observations on human marks (e.g. Galán and Domínguez-Rodrigo, 2013; Fritz, 1999; Maté-Gonzalez et al., 2015; Soulier and Morin, 2016; Thiébaud et al., 2019), we argue that the anatomical position, length and orientation of the incisions, as well as the way they intersect each other allow us to confidently exclude the possibility that they result from carcass processing activities. Defleshing marks on a distal tibia would be either long and parallel to the main axis of the bone or transverse and short. Cutmarks associated with tendon removal, in the case of metapodials, would be shorter, closer to the proximal/distal extremity and transverse. Alternatively, they could be very long and parallel to the main axis of the bone but should not intersect each other. Skinning marks on a proximal radius or on metapodials would be parallel

to the bone and a single incision is usually enough to cut through the skin. Finally, disarticulation marks would be closer to the articular surface (i.e. proximal/distal epiphysis), which is missing in the case of the Diepkloof bone. Besides the fact that the organization and distribution of the engravings do not match with the “butchery hypothesis”, we have to recall that the engraved bone only represents a fragment of a larger piece that had more extended engravings on its surface, though the exact extension remains uncertain. In addition, the likely use of a point to produce the engravings and the hypothesis that the bone was partly dried at the time of its engravings, are both additional arguments in favor of an intentional engraving.”

- In conclusion, I think that the section on the analysis of the incisions needs rewriting and the presentation of a better and more demonstrative iconography. Page 37. “The engraved bone from the pre-SB Lynn coincides chronologically with the oldest engraved ochre pieces from Blombos Cave and Klasies River, but differ from other more recent engraved bones from South Africa where parallel and perpendicular incisions dominate (d’Errico and Henshilwood, 2007; d’Errico et al., 2001; 2012; Henshilwood et al., 2009)” Can the author add a reference for the engraved ochre piece from Klasies?

*We have added the following reference (d’Errico et al. 2012b): d’Errico, F., Moreno, R. G., & Rifkin, R. F. (2012). Technological, elemental and colorimetric analysis of an engraved ochre fragment from the Middle Stone Age levels of Klasies River Cave 1, South Africa. Journal of Archaeological Science, 39(4), 942-952.*

- Page 32-49. I found this section well written and interesting. There is however an issue that I think the authors have not considered and that should be integrated in the discussion. According to the cultural exaptation scenario one would expect that the pattern incised on the Diepkloof bone would give rise in some way to more complex and consistent cultural traits in the same behavioral domain, which apparently is not the case at Diepkloof before the HP. How do the authors explain this substantial gap? Should we see the pattern, and perhaps other inventions, as behaviours that have not become innovations? Or perhaps this transition from invention to innovation (accepted by a community) happened only in some area of the Still Bay and not in others? It is an important issues. We have to envision continuities but also possible discontinuities in cultural transmission as we are dealing with cultural traits, which can be lost.

*Many thanks for the whole review and comments. We do not have a firm explanation why there is such a gap at Diepkloof between the Pre-SB Lynn and the HP. We may invoke archaeological biases such as spatial organization coupled with the restricted surface that has been so far excavated for the SB layers. We may consider the possibility that SB groups had a lasting spatial organization that differs from preceding and succeeding occupations, maybe at a time when the site of Diepkloof had a specific territorial significance yet “not compatible” with the engraving practice at the site. We have not yet explored the spatial hypothesis and have limited arguments to develop on the territorial function of Diepkloof during the SB. So far, we notice that similar diversified (mineral, organic and botanical) resources - but in different proportions- were exploited during the Pre-SB, the SB and the HP. But the question of site’s function remains to be investigated more accurately. The fact that SB shares similar technologies (between the sites of the southern tip of Africa) may speak in favor of a continuous cultural transmission between those groups, even with discontinuities from sites to sites, assuming that the SB is the “cultural entity”.*

**Reviewed by Enza Elena Spinapolice, 2020-10-20 09:39**

The paper “Experimentation preceding innovation in a MIS5 Pre-Still Bay layer from Diepkloof Rock Shelter (South Africa): emerging technologies and symbols” by Porraz and colleagues presents the results of the excavation of the pre- SB Lynn unit in terms of cultural innovation. I admire the work that has been done on Diepkloof Rockshelter in the last years, and I think this is the perfect context to test for cultural transmission models, and the US Lynn, with its short occupation, is ideal in this sense. The paper is well written, and I enjoyed reading it. My remarks concern the ways to make it better, i.e. more useful for the scientific community and clearer in its theoretical background. First, the structure of the paper is a bit confusing: the method section is summarized at the end, and the results are presented straight after the chapter that presents the site. The section “Discoveries and legacies” sounds obscure to me. Why not simply use “Discussion”? This unusual structure confuses the reader and this is coupled with the maybe excessive length of the paper. Some restructuring of the index may help, or in alternative, a clear statement about the structure and goals in the introduction.

*We have followed the changes suggested by reviewer 2 and deleted the “Method section” as it exists, to rather include the appropriate references within the text. We also changed the title “5. discoveries and legacies” to “5. Implications”. Finally we*

*modified the last paragraph of introduction to clarify the structure (see answer to Reviewer 1) and also modified the structure by reversing the lithic technology chapter with the bone description chapter.*

The tracking of cultural evolution through processes of innovation and cultural transmission is an extremely interesting and powerful topic in the area of human evolution nowadays. Despite it is largely acknowledged that this path is worth exploring, methods of reconnaissance of cultural transmission are still very variable. A remark I wish to address is that for such an innovative attempt, associated with a well-known and rich site, the literature review on this topic is poor. Classic papers regarding cultural transmission, such as Mesoudi 2016, are not cited. Other papers worth a mention could be Premo and Solnik 2011, Foley and Lahr 2011, Collard et al 2005, Charbonneau 2015, 2016, Di paolo and Di Vincenzo 2018. Furthermore, a Springer book on cultural convergence, full of interesting contribution about MSA has recently been published (Groucutt “Culture history and convergent evolution: Can we detect populations in prehistory?” 2020) and in my opinion, some contributions are worth to be mentioned in the paper.

*A similar remark has been done by Reviewer 1 and we have added the following references: (see Henrich, 2015; Henrich and McElreath, 2003; Foley and Mirazon Lahr, 2016; Mesoudi, 2016; Mesoudi et al., 2004; Stuart-Fox, 2015).*

The work on lithic technology is of high level and high impact. I have a question about the seven chaînes opératoires described: some of them seem to emerge from few pieces, furthermore they are poorly illustrated. Why the authors did not attempt to connect some of them and identify a lesser number of reduction sequences? Or did they and still this classification stands? In this case, let the reader know. The incompleteness of the chaînes opératoires, as illustrated in the raw material economy chapter also could justify a low resolution of the understanding of the various steps.

*We do agree with the remark of Reviewer 2, firstly with the fact that the lithic technology is poorly illustrated (...), secondly with the fact that the sample is limited and the chaînes opératoires uncomplete. Yet, the economy of the raw materials (sensu Perles 1992) makes unlikely that some of the reduction sequences are not independent from each other's, such as the quadrangular flakes on quartzite slab and the bipolar/pièces esquillées pieces on quartz pebbles. We have one refitting of a series of blanks that shows a bladelet reduction sequence on a flake. We have a few laminar/elongated blanks that cannot be coming from the previously mentioned reductions sequences, nor it comes from the bifacial reduction sequence. I agree that there are uncertainties regarding the clear distinction between the flake and triangular flakes reduction sequences, with the exception of the “accourcies points” category which comes from an independent reduction sequences (triangular flakes with a triangular section and -dominantly- a cortical side). In the introduction of 3.2, we recall the fact that our sample size is limited and shall be implemented by further studies: “Considering the limited size of the whole assemblage, we acknowledge that further studies will have to complete the present set of observations.”*

It is interesting the inclusion of the pièces esquillées among the formal tools, however this is hard to assess for a reader, because the determining criteria are not clear, and the illustration are few. The relationship between reduction sequences and raw material economy emerges very well from the paper.

*We included the pièces esquillées in both the lithic technology 3.2 (called bipolar-an-anvil cores) and the formal tools 3.3, as we acknowledge uncertainties with regard to the technological intention. Bipolar percussion is closely associated with quartz but does occur occasionally on heat-treated silcrete. We did include those “bipolar pieces” within both categories as several studies on the MSA support alternative hypothesis, yet not incompatible. We have added three references in the sub-chapter 3.2 to refer to our determining criteria: (see: Barham, 1987; Brun-Ricalens, 2006; de la Penã, 2015).*

The analysis on the bone engraving is clear and convincing (however, somehow long), as is the discussion about the engraving in South Africa.

In my opinion, the weakest part of the paper is the final one, with the attempt to organize the innovations of the Lynn's layer. The connection between the engravings and the innovation in the lithic technology stays at the level of a suggestion. The model about human group structure and mobility it is very promising and could be better stated and associated with larger literature review. I would give more space to this part in the paper, and better connect it to the introduction, as anticipated in the title. And I would shorten the results on lithics and bones.

*We have a somewhat different impression from Reviewers 1 and 3 but we acknowledge that the title, the introduction and the discussion should be “in line”. We hope we have improved some of the weaknesses of the paper by modifying part of its structure.*

I overall think that PCI archaeology is the right place to submit this paper, for its originality, and warmly hope that my suggestions may help to improve the final version of this work.

**Reviewed by Kathryn Ranhorn, 2020-11-02 21:30**

Thank you for inviting me to review this manuscript.

#### Summary

The paper describes new analyses of material from the ‘pre-Still Bay’ layers at Diepkloof. The authors provide a background to the site and a background to the stratigraphic unit (SU) Lynn using multiple lines of evidence including OSL dating, paleobotany and faunal analysis. While summarizing previous publications related to SU Lynn, this paper presents new material that has not been described in previous publications. The study focuses on lithic technology and provisioning strategies and summarize other evidence. They describe a variety of different kinds of raw materials that were used, of which a large proportion was heat treated. They also describe over 50 pieces of ochre that show a surprising amount of geological variability. They assess the lithic technology at the site using chaîne opératoire approaches and describe 7 distinct reduction strategies: centripetal Levallois, the production of triangular flakes, production of quadrangular points, blade and elongated blank production, bladelet production, bipolar/anvil percussion on quartz to create pieces esquillées, and the production of bifacial pieces. They note that in previous publications they argued that bifacial pieces had come down from overlying layers but state that their renewed study on more material confirmed the presence of both bifacially worked pieces and shaping flakes in SU Lynn. They describe the tool component of the assemblage in great detail, including potential evidence for adhesive residues and a pattern of bifacial and unifacial shaping of otherwise convergent forms. The authors also present a detailed micro- and macroscopic use wear study which suggests that some pieces (n = 6) show impact fractures possibly evident of use as projectiles, whereas two triangular tools showed possible evidence of longitudinal use. The authors state that SU Lynn may suggest a shift in lithic procurement strategy associated with silcrete exploitation. They describe the presence of double-patination suggesting possible re-tooling, and suggest that knapping took place on site and that exotic materials were carried in likely following individual provisioning strategies moving over 20 km. They also describe pre-SB Lynn as likely a short-lived occupation.

The authors also describe an engraved bone from SU Lynn. They suspect the specimen was from a class size IV bovid like Eland or Buffalo. Based on the geometric configuration of the marks, the authors suggest the markings represent a cross-hatched pattern, and do not appear similar to butchery or trampling marks. The depth and V-shape of the incisions lead these researchers to conclude the marks were made using an unretouched stone tool, possibly with fresh periosteum still covering the bone. They argue based on location of the incisions and SEM analysis that the piece was likely non-functional, and draw a comparison to similar cross-hatch markings from Blombos.

The authors also describe the presence of evidence for heat treatment in the SU Lynn, describing an increase in the number of silcrete pieces. They suggest that tools were heat treated at various stages of the reduction sequence. They describe one core which may show evidence of reheating, and suggest that heat treatment invention may have occurred on accident with discarded artifacts near domestic or natural fires. They call this a new hypothesis but I was under the impression that it was the null hypothesis. They also suggest that knappers during SU Lynn period were not as skilled at heat treatment as is suggested by later evidence.

We have modified to “null hypothesis”.

They summarize their results by stating the SU Lynn represents a shift in procurement strategy, highly associated with silcrete and production of bladelets. They argue that bladelets may imply composite tool use. They also describe formal tools as comprising three forms (unifacial, partial bifacial, and bifacial points) rather than a continuum. They describe ventral shaping on unifacial points possibly indicative of hafting.

The authors describe the significance of the engraved bone in relation to other known engravings, and symbolism in the 'HP'. They state that based on the similarities with Blombos and otherwise lack of evidence in this time period, it may represent a "pioneer stage" of symbolism in these populations. They conclude by stating that their results show that most of the behavioral novelties that later typify the SB and HP already existed in the SU Lynn.

#### Review

The paper is clearly organized, easy to read, detailed, and provides much-needed data about this important time period in human evolution. It also relies on multiple lines of evidence and describes a suite of behaviors from SU Lynn based on fauna, ochre, lithics, paleobotany, and an engraved bone. I have a few comments below about the interpretations, and a few minor suggested revisions. Overall however my recommendation is that this paper be recommended by PCI.

#### Comments

- I wonder if the authors can comment more about the increase in bladelet production as well as the increase in silcrete use. Is there faunal evidence to suggest a shift in resource acquisition?

*The present set of data (Cartwright, 2013; Klein and Steele, 2013) do not allow to state on clear evidences, as the SU Lynn has been merged with preceding SUs in the anthracological studies, and only represents a limited sample in the fauna analysis. Yet, no clear change in the fauna spectrum from the MSA-Mike to the Pre-SB Lynn has been noticed. We have added a sentence (3.4) to shortly address the topic: "The data described for the pre-SB Lynn recognize groups that were moving over long distances (>20km), taking the benefit of local raw material availability but carrying items from site to site to cope with unforeseen circumstances. In comparison with preceding occupations, the pre-SB Lynn introduces a techno-economic change, which might be the result of populations that had to face and adapt to new conditions, either climatic, demographic and/or socio-economic. On that regard, botanical (Cartwright, 2013) and faunal (Steele and Klein, 2013) studies provide useful information yet limited by the sample size."*

- The authors report two age ranges for SU Lynn. These two age ranges do not appear to overlap. Some discussion on why these ages appear discordant would be helpful.

*The discrepancy between the two set of ages is a long debate that has yet not find any consensus between the different research teams. This debate is partly reflected in the quoted references:*

- Tribolo, C., Mercier, N., Douville, E., Joron, J.L., Reyss, J.L., Rufer, D., Cantin, N., Lefrais, Y., Miller, C.E., Porraz, G., Parkington, J., 2013. OSL and TL dating of the Middle Stone Age sequence at Diepkloof Rock Shelter (South Africa): a clarification. *Journal of Archaeological Science* 40, 3401-11.
- Jacobs, Z., Roberts, R.G., 2015. An improved single grain OSL chronology for the sedimentary deposits from Diepkloof Rock shelter, Western Cape, South Africa. *Journal of Archaeological Science* 63, 175-92.
- Jacobs, Z., Roberts, R.G., 2017. Single-grain OSL chronologies for the Still Bay and Howieson's Poort industries and the transition between them: Further analyses and statistical modelling. *Journal of Human Evolution* 107, 1-13.

- Other than photos, there does not seem to be any evidence provided to support the heat treatment findings. This is a common issue due to the historic reliance on expert knowledge. Perhaps the authors could include enhanced images as they have done in Figure 8 to the relevant heat-treated pieces in Figures 4 and 6. The photos are well detailed but it is difficult to distinguish HINC surfaces from heat-treated ones in those photos despite the labels.

*We acknowledge that enhanced images shall support the reliance on expert knowledge, but do not have such pictures yet.*

- Isn't it likely that these results from the "pre-Still Bay" merely reflect variability which has been obscured by the reliance on named stone tool industries? It is not very surprising to me that the unit below the "SB" shows some evidence like heat treatment or bifacial shaping similar to the one above it. It seems that these results are themselves added evidence for the futility of industries in studying population-level variability. Given that the authors make statements at the end of the paper about 'general population and biological connectedness' across South Africa during MIS 5, I would like to hear their ideas for studying those connections in a technologically oriented way without reliance on those terms.



We do recognize the importance of such discussion. From our perspective, the fact to not be surprised to find bifacial and shaping elements in a SU immediately preceding the SB is right when one considers possible archaeological biases such as mixing at the contact between two units. It is based on this possibility that we initially rejected their presence in Porraz et al. 2013. But new excavations brought new data and firmly confirmed their presence within that specific unit. Yet, we do consider the presence of bifacial elements in the SU Lynn as much surprising than would be their absence. The scenarios of technological successions and transitions can take various forms, from which we may infer on various evolutionary processes that will reflect and enhance the theoretical backgrounds of the researchers. The “naming” of industries is entangled within a cultural perspective that consider technology as a social learning process, which, at various scales, may help identifying the cultural and geographical bounds that unify those groups. We do not consider that the Pre-SB Lynn reflects a variability of the SB. We try to be empirical and rely on archaeological evidences which are admittedly very few. The Pre-SB Lynn precedes the SB and as for now, belongs to the evolutionary trajectory of the SB, not its variability. Yet, we may argue and discuss on the set of bifacial industries (extending from fully bifacial industries to industries with handful bifacial specimens) that has been labelled as SB, which from our perspective does not conform to the technological theoretical backgrounds and brings confusion to the various readers. We may have alternatives on naming industries, which is what we partly tried in Porraz et al. 2013 (and following papers), which will consist on affiliating industries to their specific time and place (At Diepkloof, all “named” industries are followed by a specific name of the SU. For example, the SB is called the SB Larry). This is not to say that it solves the problem of scales in discussing cultural connectedness, but it moderates the limits and still promotes the focus of the research team which aims at deciphering processes and mechanisms of cultural changes through time. Now, Reviewer 3 may have revealed an inconsistency (from our side) with the last sentence ending the paper “It is assumed that people from Southern Africa during the MIS5, regardless of their technological differences, formed a pool of people culturally and biologically connected to each other.” Sentence that we have consequently modified as follow: “It is assumed that people from Southern Africa during the MIS5, regardless of their technological and cultural differences, formed a pool of people socially and biologically connected to each other.”

- Regarding ‘emerging symbols’: I appreciate the authors’ careful analysis and openness to multiple interpretations for the engraved bone, including one of simply ‘waste’. Their interpretations throughout underscore temporal and spatial heterogeneity and they even state that the described shifts were not monolithic. The authors do suggest however a shift in ‘social perception of graphic representations from the pre-SB to the HP’. What remains unclear, especially for those of us not working in southern Africa, is if the lack of engravings in earlier levels may also coincide with less occupation?

*This is indeed a pivotal question as demography has been promoted -in several papers- as a key mechanism behind innovations and success of cultural traits. Various aspects shall be considered when considering the set of early MIS5 and MIS6 archaeological assemblages. Yet, such examples are still very few, with some remarkable exceptions such as the sites of Klasies and Pinnacle Point 13. In addition, these periods have for now benefited from little attention and have been consecutively poorly promoted in the construction of the main synthesis. Considering potential geoarchaeological biases that have not been fully considered yet, and considering the poor archaeological visibility in terms of research interest, this sounds premature to state that this period associates with less occupation. At the site of Diepkloof, focusing on the sole sequence, there seems to be a higher intensity of occupation at the time of the intermediate HP, when we observe a change in geoarchaeological process that reflect a change in site’s occupations.*

#### Suggested Edits

- Figure 2 caption might be divided into A, B, and C or 1,2,3 to enable the reader to more easily find the description of the engraved tool in the text.

*Figure 2 has been modified, following as well the remark of Reviewer 1*

- In describing the heat treatment, the authors default to ‘he’ when describing knapper behavior. It would behoove them to change this to ‘they/their’ given that it is unlikely that they know the gender of the Pleistocene knapper.

*This is right !*

- The last paragraph on page 35 says “but differ from other more recent engraved bones” should be “but differs from...”

