Understanding Palaeolithic adaptations through niche modelling - the case of the French Middle Gravettian

Felix Riede *based on peer reviews by* *Andreas Maier* *and* *Joao Marreiros* ⓘ

Submitted: 23 March 2020, Recommended: 20 June 2020

Published: 20 June 2020

The paper entitled "Investigating relationships between technological variability and ecology in the Middle Gravettian (ca. 32-28 ky cal. BP) in France" [1] submitted by A. Vignoles and colleagues offers a robust and interesting new analysis of the niche differences between the Rayssian and Noaillian facies of the Middle Gravettian in France. Understanding technological variability in the Palaeolithic is a long-standing challenge. Previous debates have vacillated between strong, quasi-ethnic culture-historical interpretations rooted in the traditional European school and extreme functional stances that would see artefact forms and their frequencies with assemblages conditioned by site function. While both positions have their merits, many empirical and conceptual caveats haunt them equally [see 2]. In this new study Vignoles and colleagues, so-called eco-cultural niche modelling is applied in an attempt to explore whether, and if so, which environmental background factors may have conditioned the emergence and persistence of two sub-cultural categories (facies) within the Middle Gravettian: the Rayssian and the Noaillian. These are are defined through, respectively, a specific knapping method and the presence of a specific burin type, and the occurrence of these seems divided by the Garonne River. Eco-cultural niche modelling has emerged as an archaeological application of distribution models widely employed in ecology, including palaeoecology, to understand organismal niche envelopes [3]. They constitute powerful tools for using the spatial and chronological information inherent in the archaeological record to up-scale interpretations of human-environment relations beyond individual site stratigraphies or dating series. Another important feature of such models is that their performance can, as Vignoles et al. also show, be formally evaluated and replicated. Following on from earlier applications of such techniques [e.g. 4], the

authors here present an interesting study that uses very specific archaeological indicators – namely the Raysse method and the Noaillian burin – as defining features for the units (communities, traditions) whose adaptations they investigate. While broad tool types have previously been used as cultural taxonomic indicators in niche modelling studies [5], the present study is ambitious in its attempt to understand variability at a relatively small spatial scale. This mirrors equally interesting attempts of doing so in later prehistoric contexts [6]. Applications of niche modelling that use analytical units defined through archaeological characteristics (technology, typology) are opening up exciting new opportunities for pinning down precisely which environmental or climatic features these cultural components reference, if any. The study by Vignoles et al. makes a good case. At the same time, this approach also acutely raises questions of cultural taxonomy, of how we define our units of analysis and what they might mean [7]. It remains unclear to whether we can define such units on the basis of very different technological traits if the aim is to then use them as taxonomically equivalent in subsequent analyses. There is also a risk that these facies become reified as traditions of sub-cultures – then often further equated with specific people – through an overly normative view of their constituent technological elements. In addition, studies of adaptation in principle need to be conscious of the so-called 'Galton's Problem', where the historical relatedness of the analytical units in question need to be taken into account in seeking salient correlations between cultural and environmental features [8]. In pushing forward eco-cultural niche modelling, the study by Vignoles et al. thus takes us some way forward in understanding the potentially adaptive variability within the Gravettian; future work should consider more strongly the specific historical relatedness amongst the cultural taxa under study and follow more theory-driven definition thereof. Such definition would also allow the post-analysis interpretations of eco-cultural niche modelling to be more explicit. Without doubt, the Gravettian as a whole – including, for instance, phenomena such as the Maisierian [9] – would benefit from additional and extended applications of this method. Similarly, other periods of the Palaeolithic also characterized by such variability (e.g. the Magdalenian and Final Palaeolithic) offer additional cases moving forward.

Bibliography [1] Vignoles, A. et al. (2020). Investigating relationships between technological variability and ecology in 1 the Middle Gravettian (ca. 32-28 ky cal. BP) in France. PCI Archaeology. [10.31219/osf.io/ud3hj](https://doi.org/10.31219/osf.io/ud3hj) [2] Dibble, H.L., Holdaway, S.J., Lin, S.C., Braun, D.R., Douglass, M.J., Iovita, R., McPherron, S.P., Olszewski, D.I., Sandgathe, D., 2017. Major Fallacies Surrounding Stone Artifacts and Assemblages. Journal of Archaeological Method and Theory 24, 813–851. [10.1007/s10816-016-9297-8](https://doi.org/10.1007/s10816-016-9297-8) [3] Svenning, J.-C., Fløjgaard, C., Marske, K.A., Nógues-Bravo, D., Normand, S., 2011. Applications of species distribution modeling to paleobiology. Quaternary Science Reviews 30, 2930–2947. [10.1016/j.quascirev.2011.06.012](http://dx.doi.org/10.1016/j.quascirev.2011.06.012) [4] Banks, W.E., d'Errico, F., Dibble, H.L., Krishtalka, L., West, D., Olszewski, D.I., Townsend Petersen, A., Anderson, D.G., Gillam, J.C., Montet-White, A., Crucifix, M., Marean, C.W., Sánchez-Goñi, M.F., Wolfarth, B., Vanhaeren, M., 2006. Eco-Cultural Niche Modeling: New Tools for Reconstructing the Geography and Ecology of Past Human Populations. PaleoAnthropology 2006, 68–83. [5] Banks, W.E., Zilhão, J., d'Errico, F., Kageyama, M., Sima, A., Ronchitelli, A., 2009. Investigating links between ecology and bifacial tool types in Western Europe during the Last Glacial Maximum. Journal of Archaeological Science 36, 2853–2867. [10.1016/j.jas.2009.09.014](https://doi.org/10.1016/j.jas.2009.09.014) [6] Whitford, B.R., 2019. Characterizing the cultural evolutionary process from eco-cultural niche models: niche construction during the Neolithic of the Struma River Valley (c. 6200–4900 BC). Archaeological and Anthropological Sciences 11, 2181–2200. [10.1007/s12520-018-0667-x](https://doi.org/10.1007/s12520-018-0667-x) [7] Reynolds, N., Riede, F., 2019. House of cards: cultural taxonomy and the study of the European Upper Palaeolithic. Antiquity 93, 1350–1358. [10.15184/aqy.2019.49](https://doi.org/10.15184/aqy.2019.49) [8] Mace, R., Pagel, M.D., 1994. The Comparative Method in Anthropology. Current Anthropology 35, 549–564. [10.1086/204317](https://doi.org/10.1086/204317) [9] Pesesse, D., 2017. Is it still appropriate to talk about the Gravettian? Data from lithic industries in Western Europe. Quartär 64, 107–128. [10.7485/QU64_5](https://doi.org/10.7485/QU64_5)

Reviews

Evaluation round #1

Authors' reply, 08 June 2020

We thank the reviewers for their critical readings that served to improve our paper. Below, we reply to their comments and describe our subsequent modifications. Please note that Dr. Maier identified six issues, each of which he numbered. Our replies to these issues are numbered accordingly. With respect to Dr. Marreiros, his comments are included in bold text and are followed by our responses.

Andreas Maier's comments

Concerning the introduction paragraph, we wish to clarify that the aim is not to test the hypothesis that the Garonne River Valley marks a border between two cultural trajectories. Rather, we test the hypothesis that the different cultural trajectories on either side of the Garonne River Valley are related to the exploitation of different environmental conditions (i.e., ecological niches). We have clarified this point and associated initial description of the research approach in order to eliminate confusion (l. 133 to 136).

1. As correctly pointed out, no two niches characterized using two sets of occurrences that are geographically differentiated will be identical. However, by taking into account the background areas that are used to estimate those niches, one can reliably evaluate to what degree their similarities or differences depart from what would be expected by chance. In our case, the niches have a strong degree of geographical similarity, but when compared to the distribution of similarity values of a thousand random background models, we observe that the empirical niche estimations are more different from one another than would be expected by chance. The similarity observed, although not significant, is not surprising considering the theory of niche conservatism and the fact that the niches pertain to closely related populations.

2. Occurrence data certainly influence resulting niche estimations, and it makes sense to assume that comparisons between niches will be influenced by one occurrence data set being twice the size of the other, especially with respect to predictive architectures that do not extrapolate or only minimally so. This is a potential limitation that is stated at the end of the discussion where we have added a sentence explaining why this possibility cannot be entirely excluded (l. 586 to 590). Taking into account this comment, we have modified slightly our modeling design in order to reduce such a potential bias by removing two feature classes (hinge and threshold), because they are complex response variables. Increasing model complexity while having a low number of occurrences points can result in model overfitting and increase model variability, thus rendering the comparisons less reliable (section 2.3.2). By removing complex variable responses, the variability between models has indeed decreased and therefore our comparisons appear to be even more robust. (Figure 5) The fact that the Pyrenees occurrences are distributed within a smaller area and are more clustered does not necessarily mean that their niche should be smaller. In fact, one could also expect the Pyrenees niche to be broader in some dimensions since this region is characterized by a higher degree of altitudinal variability (generally indirectly related to temperature). Concerning the removal of duplicate occurrences, the objective is to eliminate pseudoreplication caused by duplicated signal of the same pixel to the algorithm. If we do not clean duplicate occurrences, we overfit ecological niche models. We did an additional thinning with the objective of reducing potential spatial autocorrelation between occurrences that would result in oversampling in some regions as

3

opposed to others. We thus thinned the dataset so that each occurrence point is separated from another by at least two pixels. The northern middle Gravettian dataset was more impacted by the thinning than the Pyrenees Noaillian dataset because of the clustering of sites in the Northern Aquitaine region (related to research history). (section 2.3.1)

Finally, the comment that the grid size is too coarse to capture the ecological diversity of the Pyrenees would be relevant if we were reconstructing niches at a more local geographic scale. Obtaining reliable environmental layers at a higher resolution from the kind of climate simulation that we used is not feasible. Downscaling the layers further could increase environmental variability, but at the same time, uncertainty would increase. As thinning results in conserving most geographically extreme records, the risk of missing extreme environmental values (which will determine how broad niches are) is also, somewhat, reduced.

With respect to the calibration area M, we consider that the hypothesis proposed is not problematic as the region selected could have been accessible to the target populations of interest. The background points required to create Maxent models are drawn from this area (M) and help to measure how similar background environments are those known to have been occupied.

3. The fact that the niche is smaller and less broad while associated with a higher faunal diversity is not necessarily counter-intuitive. We found that the Pyrenees Noaillian niche is smaller than the Northern Middle Gravettian niche. Their multidimensional morphology in terms of width and breadth are non-extrapolative with respect to fauna. The niches might overlap with those of multiple other species, but by themselves, niches are not proxies for species richness. We did not attempt to discuss biodiversity patterns with our current data since that is largely beyond the scope of this paper. While the possibility exists that there could be an equal degree of faunal diversity in the north, we unfortunately do not possess data with which to evaluate such diversity—one would need paleontological sites where animal accumulations are not biased by human predation. We agree that this was not explained clearly enough, so we have added a sentence in the discussion paragraph to clarify this point (l. 496 to 497).

4. We cannot compare the Noaillian and the Rayssian because they are not defined in a similar manner: the Noaillian means "presence of Noailles burins" whereas the Rayssian means "presence of the Raysse method and eventually a conceptually similar blade reduction method". Hence, they are not defined as technocomplexes but rather typo-technological "faciès". This is why we chose to compare two regions that have different cultural histories, in the sense that they do not have the same facies across Greenland Stadial (GS) 5. To make this clearer, we added a note in the introduction explaining what we mean by "faciès" (note 1). The point of comparison that we chose in the discussion is armature type and associated chaînes opératoires. This was not clear in the initial submission, so we have added some text and rephrased some arguments in order to clarify (see l. 461 to 493).

Concerning the comment, "… the Noaillian and Rayssian toolkit were both equally suited for the conditions in the northern niche", we do not think that one can unequivocally make this statement because the Noaillian is not well described in the north. Therefore, we do not know whether the armatures associated with Noailles burins in the north are the same as in the Pyrenees. To date, it appears that Noailles burins can be associated with different industries (for example, those with and (almost) without Gravette points). This is paired with the fact that they seem to be used for different activities (see Calvo et al., 2019), which provides a heterogeneous vision of this typological category. Finally, we cannot say that the Raysse method was unsuccessful in the southern niche because it would suggest that it had been present in the south, at least shortly, which is not the case according to known data.

5. We cannot make the mechanisms explicit if we do not know what they are - hence the reason for doing

the study. Furthermore, this study by itself may not necessarily help us identify specific mechanisms but it certainly provides us a piece of the larger puzzle. We have explicitly defined what we mean by mechanism in the introduction by referring to the definition provided by d'Errico and Banks (2013) (l. 80 to 82). (see also response to João Marreiros' first comment)

Concerning the other questions:

a) In fact, the use of the northern area represents a broader niche compared to the south, but it does not imply any chronological relationship between the Noaillian and the Rayssian. We are not saying that the southern niche is the Noaillian niche, because the "Noaillian" is related to the presence of Noailles burins, so it is not a species and it does not have specific ecological requirements. It is more the niche of the groups using this kind of tool, which in the Pyrenees seems to be associated with other behaviors such as diverse backed blade and bladelet armatures, inferred high logistical mobility etc. The northern niche is the niche of groups having used both the Noaillian and the Rayssian across GS5. This northern niche is broader. The hypothesis that we propose to explain this difference is that the Raysse method was more successful in this context, probably because it was more suited to high mobility within geographically broad subsistence/settlement territories, which would lead to a broader ecological niche.

b) The statement that the northern niche is significantly broader in environmental dimensions is based on the results of the comparison between the NicheA models. The northern niche is significantly broader because it captures a wider range of colder and drier conditions as can be seen in the figure. Moreover, the fact that there are less prey species at an archaeological site does not necessarily mean that there were fewer species present on the landscape; it could also be the result of these populations' focusing on one species more than others.

c) This sentence in the text was indeed confusing: we did not mean that reindeer were less predictable, but that other resources, such as good quality flint, could be less predictable or less available, hence a higher degree of curation in some assemblages located away from good raw material sources (Grotte du Renne). We have made this more explicit in the text (l. 621 and 624-625).

6. We agree that such a pattern would be interesting to explore, however addressing it would be beyond the scope of this paper, and we do not think that it is appropriate to speculate here about this phenomenon observed during later time periods. Furthermore, to examine the potential role of the sables des Landes desert and the Garonne valley in cultural geography would require analyses of geological data and considerations of how best to incorporate them into the approach that we employ—work which is well beyond this study's objectives.

João Marreiros' comments

Why is the authors advocate so? Do the authors mean by "diversity" the lithic techno-typological variability? Is the lithic variability the main characteristic that makes this a perfect case study?
We agree that the word "diversity" was not sufficiently precise in this context. We have replaced it with "typo-technological variability" (l. 31), which better describes what we mean. We think it is clear, following our descriptions of these two "faciès" and the approach that we employ, why this Middle Gravettian record, with its geographic variability and potential ecological variability, represents an excellent case study for identifying some of the factors implicated in this diversity. Identifying factors is a necessary first step if we are to identify mechanisms. (see also 5th response to Andreas Maier)

Introduction. "These two typo-technological facies appear to differ both chronologically and geographically." According to the state-of-the art presented by the authors, this seems not to be the case. Both industries are overlapping chronologically and geographically. In fact, this is one of the aspects considered later in the results and discussion section. As an example, the poor absolute chronological dating of some of the sites is problematic, as discussed by the authors, and pointed as one of the main

limitations of the study.

We agree that this statement seems to be in contradiction with the rest of the paragraph, and in the end, it is not necessary. We therefore removed it and broke the paragraph into two parts: one explaining the chronological aspect, and the other focusing on the geographic distribution (l. 98 to 130).

"Although this situation is complicated by the fact that Noailles burins and the Raysse method are frequently found together within archaeological assemblages." Is this really the case? This seems to be contradictory to what the authors advocate earlier, especially on what concerns the chronological and geographical distributions of these industries. Perhaps the issue here concerns the term "found together". What is it meant here? Same site, same archaeological horizon?

We disagree that this contradicts the rest of the paragraph. We state that the Noaillian is usually described as being older than the Rayssian in stratified contexts (Pataud, Flageolet) and that they have partially overlapping territories (meaning that in some regions, there is no Rayssian and in others, there is no Noaillian). We have added text in the introduction to clarify the statement about the fact that they are "found together" in archaeological assemblages. In fact, they are often described as being associated within the same archaeological layer. At present, however, these associations cannot be considered to be cultural in nature, since studies evaluating their stratigraphic and historical contexts are sorely lacking. In fact, at many sites, they appear to be more the result of imprecise excavation methods or site formation and post-depositional processes (l. 102 to 106 and 109-110).

"The application of Ecological Niche Modeling (ENM) methods to the archaeological record is one way to test this hypothesis". In the previous paragraph the authors explain the key hypothesis that have being explored and explain the observed lithic variability. But, in this sentence is not clear to each of the hypothesis the authors are referring to.

We have moved and rephrased the research question to make the paragraph clearer (section 1.2).

Materials and methods "Taking into account these potential limitations". I think this is an over-statement. These are clear limitations which are not related to the quality or importance of this study presented here. In fact, these limitations are the reflex of the archaeological record, excavation methods, and study protocols used in the past. And these, independent of the methods applied here, could indeed constraint general interpretations.

We agree and removed the word "potential".

Discussion "The fact that the northern Middle Gravettian niche is significantly broader than that of the Pyrenees Noaillian suggests that the development of the Raysse method may be linked to the exploitation of a significantly expanded niche composed of colder, drier conditions and thus more open landscapes and associated large mammal prey species. Available archaeological data from the archaeological record support this hypothesis." I agree with the authors, but I think here the archaeological support for this argument needs to be more elaborated.

We have added a paragraph about the Raysse method explaining why we think this method is well-adapted to the exploitation of large territories (l. 461 to 479 and also 509 to 521).

Conclusion "These niche results further support the hypothesis that the Landes cold desert and Garonne River Valley corridor served to limit cultural interactions between the Pyrenees and regions to the north." I think this statement should be taken with a "pinch of salt". The study shows different technological adaptations to different ecological settings, but this does not necessarily represent cultural interaction, or in this case, the lack of it. From my perspective wither the authors explain what is meant by "cultural" processes in this context, or they should explain this scenario from a technological approach. We have clarified what we mean by "cultural interaction", because it is correct that the term was too vague. We are referring to the homogenization of technological traditions [i.e., a hypothetical (and not observed) adoption of the Raysse method by populations in the Pyrenees].

N.B.: we made some minor modifications concerning the English language after one final English proof-reading made by one of us (WB). We also slightly modified some figures so as to make them clearer (Lines

connecting sites with site number on Figure 2; background similarity and identity test legends were made changed from "Overlap" to "Similarity/Identity" to avoid vocabulary confusion on Figure 5; NicheA ellipsoids colors were changed to make them more visible on Figure 6). We also added a reference: Delvigne et al., 2020, since it was cited as "in prep." (note 6) in the previous version and has just been published.   References

Calvo, A., Perales, U., García-Rojas, M., Normand, C., & Arrizabalaga, A. (2019). Just before sewing needles. A functional hypothesis for Gravettian Noailles-type burins from Isturitz cave (Basque Country, southwestern France). Journal of Archaeological Science: Reports, 25, 420–432. `https://doi.org/10.1016/j.jasrep.2019.04.022`

Delvigne, V., Fernandes, P., Tuffery, C., Raynal, J.-P., & Klaric, L. (2020). Taphonomic methods and a database to establish the origin of sedimentary silicified rocks from the Middle-recent Gravettian open-air site of La Picardie (Indre-et-Loire, France). Journal of Archaeological Science: Reports, 32, 102369. `https://doi.org/10.1016/j.jasrep.2020.102369`
Download tracked changes file

Decision by Felix Riede, posted 22 April 2020

Recommendation - with some revisions

This is a very interesting manuscript that attempts to apply distribution modelling techniques to a particular case study from the French Gravettian. The paper is considered interesting and valuable but would also benefit from a range of amendments and clarifications that have been flagged up by the reviewers.

Reviewed by Andreas Maier, 08 April 2020

The spatial distribution of sites attributed to the Noaillian and Rayssian, two archaeological sub-units of the French Middle Gravettian, show a conspicuous spatial distribution, where Rayssian assemblages are only found north of the Garonne River. Using ecological niche modelling methods, Vignoles et al. attempt to quantitatively test the hypothesis that the Garonne River valley marks a border between two cultural trajectories with different typo-technological characteristics, reflecting the exploitation of two different ecological niches via different technological adaptations during the French Middle Gravettian. The authors place their research in the context of other hypotheses for explaining the observed spatial distribution (e.g. site function, populations with different traditions, environmental differences, problems of archaeological taxonomy) and pursue their question with a clear research design and comprehensible approach. They take a critical stance toward the archaeological data and transparently discuss their criteria for the acceptance or exclusion of assemblages for analysis. Eventually, Vignoles et al. find that the selected sites north and south of the Garonne River are indeed associated with two significantly different ecological niches and conclude that the appearance of the Raysse toolkit is an expression of the exploitation of a broader range of ecological conditions in the northern niche, associated with mobility and settlement strategies in a larger territory. They further conclude that La Picardie bladelets (products of the Raysse method) represented a technological advantage over traditional Gravettian armatures within this adaptive framework, because of easier maintenance and a better adaptation to hunting activities organized in territories with a less predictable access to resource.

This study addresses an important research question with a modern and interdisciplinary research design. It involves various kinds of data, calculations, and theoretical assumptions, and eventually offers a unified explanation of the observed archaeological pattern. This makes this study interesting research but also challenging to comprehend. The following comments may thus offer an opportunity for debate, but may also reflect my own shortcomings in understanding all details in the different lines of reasoning.

1. I am somehow under the impression that the finding of two different ecological niches is virtually inevitable. Given that geographic space is not uniform, a comparison between two sets of locations in mutually exclusive and topographically distinct areas at such a large spatial scale will in all probability result in significant

ecological differences between the two areas. Against this background, the large overlap between the two niches (Fig. 7) is maybe more surprising than their differences.

2. It is found that the "Pyrenees Noaillian niche is smaller and less broad than that of the northern Middle Gravettian". I wonder to what extent the setting of the modelling parameters is responsible for this result. The sites of the southern sample are only half as numerous (n=10) than those of the northern sample (n=20). At the same time, they are distributed in a much smaller area and, in addition, also more clustered. The strong clustering also suggests that they are more affected by the removal of duplicate site occurrences in a grid-cell prior to analysis. It also seems likely that a grid size of 11.5 km reduces the observable ecological diversity in areas with a marked topography (such as the Pyrenees) stronger than areas with a more uniform topography (such as the northern areas). Eventually, these factors may have as a result that the southern sample captures a smaller fraction of the general diversity of the habitat in comparison to the northern sample, potentially underestimating the size and width of the southern niche. I also wonder how the layout of the calibration area affects the resulting niches, given the large regions without archaeological sites in its northern part.

3. It also puzzles me that the southern niche is said to be "smaller and less broad" in its environmental conditions, while at the same time, the hunted fauna is more divers (reindeer, bovids, horse, chamois, bison, deer and fox) than in the north (predominantly reindeer). To me, it is counter-intuitive that a smaller niche with a "reduced range of environmental conditions" has a much broader faunal signal. I assume that the size and width of an ecological niche is somehow unrelated to its ecological diversity – at least as mirrored in the faunal remains. An elaboration on the relation between these parameters would be very welcome.

4. The southern sample contains only sites attributed to the Noaillian, while the northern sample is a mix of both Noaillian and Rayssian sites. Consequently, the resulting niches are termed "Pyrenees Noaillian niche" and "northern Middle Gravettian niche", rather than "Noaillian niche" and "Rayssian niche". In the interpretation, however, it seems that the findings for the northern niche are used to state and explain differences between the Rayssian and the Noaillian: - Raysse method is related to a significant expansion of the niche - Raysse technological toolkit is an expression of the exploitation of a broader range of ecological conditions - Raysse method appears to have been associated with mobility and settlement strategies contained within a larger exploited territory or territories - La Picardie bladelets (products of the Raysse method) represented a technological advantage over Gravettes / microgravettes armatures within this adaptive framework - Raysse method would have been advantageous in such contexts, because it would have been more easily maintainable and more adapted to hunting activities organized in territories where access to resource was less predictable. Conversely, the Gravettes / microgravettes armatures reflects a less maintainable hunting toolkit, which was employed in contexts for which access to resources was more predictable.

Looking at the spatial distribution of the sites (Figs. 2 and 4), all Rayssian sites are contained within the geographic range of the Noaillian sites, except for Plasenn al Lomm in Britanny (which is, however, more or less at the same latitude as La Verpillère I cave, not classified as Noaillian in the present study). Given that 4 Noaillian and 4 Rayssian (out of 22) sites are located between 46 and 48°N, the notion of a particularly northern adaptation of Rayssian sites seems not to match with the record. Given further that both Rayssian and Noaillian sites are part of the same sample set and thus (1) are likewise responsible for the size and shape of the modelled niche and (2) occur within the same ecological setting, it rather seems to me that the Noaillian and Rayssian toolkit were both equally suited for the conditions in the northern niche. If ecological difference played a role, my impression is that the findings rather seem to suggest that the Ryasse method has been unsuccessful for the southern niche. Or are these inferences based on the assumption that the Rayssian is younger than the Noaillian, since the "few available stratified contexts, [where] the development of the Raysse method is always stratigraphically younger than the Noaillian"? If this is the case, it needs to be made explicit, but would contradict the careful assessment of the chronological relation between these two units elsewhere in the text (lines 253-255).

5. The authors state that the French Middle Gravettian is an "ideal case study" for "attempting to identify mechanisms behind the diversity observed in the archaeological record". Maybe these mechanisms could be

made a bit more explicit? If I understand the text correctly then the use of the areas north of the Garonne broadened the (original?) southern niche by including colder and drier conditions. Because of these conditions, the main prey in the north is reindeer (also attested in the south). In the north, the niche was "significantly broader in environmental dimensions" and therefore hunter-gatherers had a higher (residential) mobility and exploited larger habitats. Under these circumstances, the highly standardized and curated Raysse toolkit is an adaptive advantage. I wonder - why the use of the northern area is an expansion of the niche when the chronological relation between the Rayssian and Noaillian is unclear and there are Noaillian sites in the same area? - why a niche that is colder and drier and shows a strong reduction of prey species is "significantly broader in environmental dimensions"? - why reindeer is a less predictable resource, when it is usually associated with predictable migrations (e.g. Binford 1979; Enloe 2003; Bodu et al. 2006).?

6. Maybe it is also interesting to consider that the observed border situation at the Garonne River is not exclusive to the Middle Gravettian. Comparable (although not totally similar) observations are reported for the Badegoulian (Banks et al. 2011) and also for the Middle Magdalenian (Séccher 2017), for instance. What do these observations imply for a border between two cultural trajectories during the French Middle Gravettian?

References

Banks, W.E., Aubry, T., d'Errico, F., Zilh~ao, J., Lira-Noriega, A., Peterson, A.T. (2011). Eco-cultural niches of the Badegoulian: unraveling links between cultural adaptation and ecology during the Last Glacial Maximum in France. Journal of Anthropological Archaeology 30, 359-374.

Binford, L. R. (1979). Organization and formation processes: Looking at curated technologies. Journal of Anthropological Research, 35 (3), 255–273.

Bodu, P., Julien, M., Valentin, B., & Debout, G. (2006). Un dernier hiver à Pincevent: les Magdaléniens du niveau IV0 (Pincevent, La Grande Paroisse, Seine-et-Marne). Gallia Préhistore, 48, 1–180.

Enloe, J. G. (2003). Acquisition and processing of reindeer in the Paris Basin. In S. Costamagno & V. Laroulandie (Eds.), Mode de Vie au Magdalénien: Apports de l'Archéozoologie (BAR International Series, Vol. 1144, pp. 23–31). Oxford: Archaeopress.

Sécher, A. (2017). Traditions techniques et paléogéographie du Magdalénien moyen ancien dans le Sud-Ouest de la France (19 000–17 500 cal. BP). Des groupes humains à plusieurs visages? Ph.D-thesis, University of Bordeaux.

Reviewed by Joao Marreiros ⓘD, 22 April 2020

PCI Archaeology, preprint review
Manuscript, Vignoles et al. Preprint, DOI: 10.17605/OSF.IO/35PB4
Dear editor, dear authors of the manuscript "Investigating relationships between technological variability and ecology in the Middle Gravettian (ca. 32-28 ky cal. BP) in France" by Vignoles and colleagues.

This paper presents a study that focused, on the environmental-technological correlation, aims to explain the technological organization and variability of lithic industries in the Middle Gravettian in France. In my opinion, the manuscript is generally well-written. The study is very interesting and explores a major debated topic in the study of past human technological, ecological and social dynamics. Thus, I would recommend the preprint. Nevertheless, I have some minor questions and recommendations that, when addressed, might hopefully clarify some of the authors arguments and point of view, and improve the manuscript.

Abstract. "The French Middle Gravettian represents an ideal case study for attempting to identify mechanisms behind the diversity observed in the archaeological record."

Why is the authors advocate so? Do the authors mean by "diversity" the lithic techno-typological variability? Is the lithic variability the main characteristic that makes this a perfect case study?

Introduction. "These two typo-technological facies appear to differ both chronologically and geographically."

According to the state-of-the art presented by the authors, this seems not to be the case. Both industries are overlapping chronologically and geographically. In fact, this is one of the aspects considered later in the

results and discussion section. As an example, the poor absolute chronological dating of some of the sites is problematic, as discussed by the authors, and pointed as one of the main limitations of the study.

"Although this situation is complicated by the fact that Noailles burins and the Raysse method are frequently found together within archaeological assemblages."

Is this really the case? This seems to be contradictory to what the authors advocate earlier, especially on what concerns the chronological and geographical distributions of these industries. Perhaps the issue here concerns the term "found together". What is it meant here? Same site, same archaeological horizon?

"The application of Ecological Niche Modeling (ENM) methods to the archaeological record is one way to test this hypothesis".

In the previous paragraph the authors explain the key hypothesis that have being explored and explain the observed lithic variability. But, in this sentence is not clear to each of the hypothesis the authors are referring to. In fact, this is only explained later at the end of the section:

"The aim of this study is to test the hypothesis that the typo-technological differences observed on either side of the Garonne River valley during the Middle Gravettian may reflect the exploitation of two different ecological niches via different technological (i.e. cultural) adaptations."

Perhaps this paragraph should be moved to the beginning of this section.

Materials and methods "Taking into account these potential limitations".

I think this is an overstatement. These are clear limitations which are not related to the quality or importance of this study presented here. In fact, these limitations are the reflex of the archaeological record, excavation methods, and study protocols used in the past. And these, independent of the methods applied here, could indeed constraint general interpretations.

Discussion "The fact that the northern Middle Gravettian niche is significantly broader than that of the Pyrenees Noaillian suggests that the development of the Raysse method may be linked to the exploitation of a significantly expanded niche composed of colder, drier conditions and thus more open landscapes and associated large mammal prey species. Available archaeological data from the archaeological record support this hypothesis."

I agree with the authors, but I think here the archaeological support for this argument needs to be more elaborated.

Conclusion "These niche results further support the hypothesis that the Landes cold desert and Garonne River Valley corridor served to limit cultural interactions between the Pyrenees and regions to the north."

I think this statement should be taken with a "pinch of salt". The study shows different technological adaptations to different ecological settings, but this does not necessarily represent cultural interaction, or in this case, the lack of it. From my perspective wither the authors explain what is meant by "cultural" processes in this context, or they should explain this scenario from a technological approach.