Ms. Ref. No.: JASREP-D-15-00502
Title: The need for a taphonomic perspective in spatial analysis: formation processes at the Early Pleistocene site of Pirro Nord (P13), Apricena, Italy
Journal of Archaeological Science: Reports

Dear Mr. Domenico Giusti,

The reviewers have commented on your above paper. They indicated that it requires major revisions (indicated at the end of this letter).

If you feel that you can suitably address the reviewers' comments (included below), I invite you to revise and resubmit your manuscript.

Please carefully address the issues raised in the comments.

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a) outline each change made (point by point) as raised in the reviewer comments

AND/OR

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I look forward to receiving your revised manuscript.

Yours sincerely,

Chris O Hunt
Co-Editor

Journal of Archaeological Science: Reports

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Reviewers' comments:

Reviewer #1: The authors are correct when they identify spatial analysis as an under-used tool in archaeological, and more specifically, taphonomic research. They are to be praised by bringing statistical quantification to spatial analyses, since most spatial studies in archaeology are descriptive rather than based on statistically-supported inferences.

My main concern is not the use of the statistical approach, which is correct, but the inferences derived from such a small area. Spatial statistics are only reliable when spatial samples are representative of a non-confined spatial population. Although the exact dimensions of the area analyzed are not indicated, the figures show a very small area. This is further constrained by the gravity effect. Most spatial statistics used by the authors are applied to bidimensional horizontal non-constrained population spaces. By using vertical distribution, the authors face the effects of gravity in creating inhomogeneous and/or dependent non-Poisson point patterns.

Although I praise the authors by their selection of statistical tests, some information, which would reinforce their study, is missing. For instance, only graphic information is provided but not data-based tests. The authors should first determine the nature of their samples prior to applying the tests they use. Determining whether the point patter is a homogeneous or inhomogeneous Poisson patter is fundamental. Here, they could have used chi-square, likelihood ratio tests or Cressie-Read test. Subsequently, they should have determined if the point pattern is Poisson (independent) or non-Poisson. This determines if second-order tests are adequate or not. Independence tests (Kolmogorov-Smirnov, Berman´s tests, Cramer-von Mises) would also have been a welcome addition.

The authors place special emphasis on Ripley´s K test and its derivate forms. K tests assume homogeneity (which has not been proved for the data sets presented); that is the point process should be stationary. If the point process is not stationary, deviations between the empirical and theoretical estimates should not be taken as evidence of point inter-dependence or interaction, since they may be simply attributed to variations in point density as is common in inhomogeneous point patterns. Showing in a better way is the point process is stationary/homogeneous or not would have provided more support for the K tests used. The R library spatstat used by the authors is a great resource and contains the function "allstats" which would have provided four different tests dealing with correlation (inter-dependence) and spacing: The G and F functions or the L function (which is a centred version of the K function). A combination of correlation and spacing is found in the J function.
If the authors had previously well determined if they are dealing with homogeneous or inhomogeneous point processes, they could apply standard or corrected functions: Kaplan-Meier for the G and F functions, for instance.

Given the inhomogeneous appearance of their samples, the authors could have attempted splitting the material spatial distribution by type (lithic and fauna separately) and fitting inhomogeneous Cox and Cluster models. If preserving the cross-type analysis, a greater display of the multitype K-G-F or J functions would also have contributed to the strength of their analysis.
Anyway, this manuscript is such an improvement over any other spatial analysis made on archaeological samples, that despite its conceptual incompleteness on the points raised above, it should be supported for publication.
The authors are dealing with not only a small and fairly restricted space to

apply their tests, but also the non-conformable contacts between units contribute to biasing the original samples and their spatial distribution and impose some serious edge effects. Although not clearly described in the text, cross-validation methods for overcoming edge effects should have been applied. The citation of Diggle makes me think they may have used Diggle´s corrections for edge effects. If so, this should be better specified in the text. The most important shortcoming of this manuscript is the painfully small sample size: e.g., only 147 lithic artefacts with spatial information. Small samples require even more edge effect correction and cast limitations (Horvitz-Thompson) on the reliability of inferences. The authors should acknowledge this fact.

Some suggested modifications follow:
"the earliest-dated locality with human evidence currently known in Western Europe". This is an overstatement. First, other sites with better identified depositional histories (such as Sime Elefante in Atapuerca or Fuentenueva 3 and Barranco del León in the Venta Micena-Orce) are either equally old or even older. In addition, this site has been controversial and if a claim of its ancient status is going to be made, this section should include a much more extensive description and information on its geology and dating. The biostratigraphic date provided is unsecure. Given that P13 is a fissure in a dynamc karstic system, it is very likely that re-elaboration and re-sedimentation processes may account for vertical displacement of microfaunal remains. By no means the associated biostratigraphic date and the lithic materials are secure because taphonomically no direct association exist between them. Re-elaboration/re-sedimentation can put in stratigraphic contact materials from
diverse provenience.

My congratulations to the authors for the application of the spatastat library to solve an archaeological problem.

Reviewer #2: I had the opportunity to read the manuscript "The need for a taphonomic perspective in spatial analysis: formation processes at the Early Pleistocene site of Pirro Nord (P13), Apricena, Italy" by Giusti and Arzarello. The authors apply a variety of point pattern analysis (PPA) on the distribution of faunal and lithic materials of a Lower Palaeolithic site, paying particular attention to the role of taphonomy. As the authors point out, this is a fairly neglected aspect of intra-site spatial analysis (but see Carrer 2015), and thus I commend their work and consider this is as a worthwhile contribution to the journal. That said, I think there are some issues on how the analysis are presented and occasionally how they are interpreted. I thus recommend some major revisions, primarily in terms of writing, before being considered for publication.

The paper contains a large number of different analysis and results that are presented very often in a confusing way. Part of this is due to a stylistic issue (sentences tend be too long), but I think the main problem is that the link between research question, methods, results, and interpretations are poorly structured. I suggest the authors to be more explicit when they introduce their research questions (i.e. what are the hypotheses? what are the expected results of the spatial analysis for each of them?) and use sub-sections to separate specific research questions/analysis to facilitate the navigation through their findings, as well as make better use of cross-references (e.g. pointing out to specific figures of analysis output in the discussion section), and perhaps lists or tables with the main outputs. As it stands some portions of the text is almost unreadable (e.g. lines 390 to 438 is a single paragraph). In other parts the paper reads almost like a lab-report where
new methods, analysis, and questions are suddenly introduced. For example, at lines 329-331 (in the Results section) the three-dimensional empty space function is introduced, while at lines 302-304 an ordinal classification of the lithics is presented. Both should be in the Material and Method section.

Section 2 presents introduce part of data and the background, but never really specify a research question. The subsequent sections and paragraphs describes details of the data (lines 132-160) and a review of PPA (lines 164-206), and we find an more or less explicit hypothesis phrased only at the paragraph starting at line 207. I understand the authors wanted to use a specific jargon (CSR) to better explain their null hypothesis, but I think the link between analyses and research question should be presented earlier, perhaps as a small paragraph at the end of sections 2.1 and 2.2. To further augment my point with an example, the separation between "cases" and "controls" (line 226) its far easier to grasp in relation to the data if presented when the distinction between TR1 vs TR2&TR3 are presented at lines 79-80.
Presumably lines 223 to 242 is intended as the pivotal point of the paper where key research questions are presented with the appropriate formalism of PPA and the link to specific method is illustrated. The presentation, however, is a bit vague, and there is not explanations of why a specific analysis is chosen, and more crucially, what we should expect to observe for a given hypothesis. For example, in line 228 the authors state they would explore the spatial relationship between the case and the controls with a cross-K function, but do specify why is this being done and what should we expect to observe. The subsequent sentence hints something but this it is all to blurry. Similarly at lines 291-293 the author states that they conducted a cross-K function by random labelling. They then state that their aim is to "assess whether the post-depositional fractures are the result of impacts and/or frictions with rocks already present in the matrix", but again they do not specify
what we should expect if the fractures were indeed caused by these rocks or not. After showing that they failed to reject their null-hypothesis they state that "stochastic post-depositional processes are most probably accountable of the […] distribution of the fractures". I found this unsatisfying, as it does not truly answer whether the rocks caused these fractures or not.

I have a couple of suggestions/questions regarding the methods used by the authors as well.
First of all, I am strong advocate of open-data and reproducible research, so R scripts and raw data should be deposited either as an electronic supplement material or on some online data/code repository. This is my personal view, but I think when possible this should be compulsory, as it facilitates the reviewing process, but it also enable more people to use similar method to their data. The authors do provide a fairly good description of the methods they used, but this sometime not sufficient, as some details (e.g. the choice of the edge correction formula) might not be specified.

In terms of actual analysis, I was a bit surprised by the small number of Monte-Carlo simulations (n=99) they used in their analysis. Given the deviation from the confidence envelope seems quite large, I don't think the results will change much but I would increase this for at least one order of magnitude.

I did appreciate the authors attempt to apply 3-dimensional analysis in archaeological data, but I don't think the assumptions of the standard 3D K function holds here. The problem is that "horizontal" and "vertical" dimensions are part of distinct processes that are nonetheless treated equally, as in the 3D K function distances are measured together. To put in simpler terms 3d K function equally assesses pairs of objects that are 20 cm apart horizontally (which might be the result of the same depositional event) from pairs that are 20 cm apart vertically (which might be more likely the result of two distinct events. Taphonomy, deposition rate, and the morphology of terrain further complicates things, but clearly a spherical buffer is not appropriate, nor a 3d homogenous Poisson a valid null hypothesis in my opinion. Given that the central point of the paper is not this analysis, I would just remove everything pertaining the 3d K-function. On a related note, the authors
state on line 271 that they "extruded from subsequent analyses the covariate effect of the third coordinate". I am not sure what they mean by this.

The authors use extensively the bivariate version of the K function using the random labelling (in contrast to the alternative random shift) null model. This

is a moot point, but I see random labelling as situations where positions are fixed but attributes can change, and random shift as positions can change but attributes are fixed. In this case I think the former is more appropriate, but I would like to know why the author choose one over the other.

In several points across the paper (e.g. lines 327, 400,424-425, 443-444) the authors interpret their failing in rejecting their null hypothesis as a support for the null hypothesis. This is incorrect. The fact the empirical K-function is within the confidence envelope does not mean that the pattern is random. It simply means that we cannot state that it is not random, which is very different. If randomness, or better put a homogenous Poisson process is a candidate hypothesis that they wish to select over alternative explanations, they should use an alternative inferential framework (i.e. information-criteria based model-selection, see Eve and Crema 2014, which the authors cite, for an example).

In summary, I believe there is potentially interesting outcome in this work, but it is strongly obscured by a confusing way the results are presented, and in some cases, interpreted. I advise that this paper should be properly restructured and resubmitted for another round of peer-review.

Carrer, F. 2015. Interpreting Intra-site Spatial Patterns in Seasonal Contexts: an Ethnoarchaeological Case Study from the Western Alps. Journal of Archaeological Method and Theory. Online First.