**Response to Reviews**

The authors of this manuscript would like to thank the editors and reviewers for the amount of time and effort that they have put into the feedback they have provided on the original version of this manuscript. These comments served to be invaluable in improving the quality and clarity of this manuscript. We have now carefully considered the comments of the reviewers and revised the manuscript accordingly. This has resulted in the substantial revision of the length and framing of the paper’s results. The original paper was 9500 words in length. The new revised manuscript is not 6500 words. In addition to the overall shortening of the paper, much of the text has been substantially rewritten to improve the clarity of the points made in the paper. These modifications have not only substantially improved the clarity of the document but also has made the paper more focused.

One of the major concerns of the paper was the lack of access to the data underlying the statistical analysis of the paper. We have now provided a materials section that describes the dataset that underlies the analysis. These data are also summarized the core and flake assemblages in two tables in the methods section. This provides information on the number of cores and flakes as well as summary statistics of the various attributes that are included in the analysis according to raw material. **In addition, we have made the dataset that underlies the statistical analysis as well as the R code that was used conduct the analysis available.** All datasets and documents can be downloaded from the link provided in the manuscript (also [here](https://www.dropbox.com/home/Models_and_Runs/Kanjera%20South)). If this paper is accepted for publication, these data will also be made available for download on Github.

In sum, we feel this version of the document is a tighter more concise version of the original manuscript that better highlights the novel contribution of the work. We have attached our responses (in bold) to the individual reviewer comments below.

Associate editor comments:

1. There are no raw or underlying data in this manuscript, either in tables,  
   supplementary information, references to publically-available datasets, etc.

**We have now added additional tables summarizing the data included in this study. In order to further encourage reproducibility a link to a the data has also been included so that researchers can reproduce the analysis presented in the paper.**

1. The manuscript contains typographic errors, repetitive sections,  
   unnecessarily long discussions, and lack of attention to some key aspects of  
   the methods and materials (refer to specific reviewer comments, although I  
   do not concur with Reviewer 1 that there is too much background provided; I  
   feel it is a useful summary). In short, the writing itself needs to be  
   carefully revised throughout. Even Reviewer 3, who considered the manuscript  
   to be easy to read, lists numerous areas that require correction.

**The writing has been carefully revised and paper has been substantially shortened.**

1. There are some concerns about the theoretical framing of the study. I  
   found the discussion of “ecological” versus “technological” approaches to be  
   lucid and useful, but as the reviewers note, it is problematic to pose them  
   against one another. Indeed, the purpose of this paper appears to be to show  
   that both are contributors to Oldowan stone tool variation.  
   “Socio-cognitive” approaches, “ecological” approaches, and “chaîne  
   opératoires” are concepts that should all be clearly defined before they are  
   used, and the authors should make clear how they relate to the test  
   implications of their models.

**The background has now been combined with the introduction to better frame the context of this work within various theoretical approaches to Oldowan research. We feel that this new structure to the paper helps focus the content more on the results and implication as opposed to setting up theoretical dichotomies that are potentially problematic**.

1. The paper seems to be simply too long and detailed to conduct the  
   analyses and report the results. As per point 2) above, it could be  
   substantially shortened and the authors could arrive at the main point much  
   more expediently – especially given the lack of newly-reported data and  
   newly-revealed conclusions. In effect, the conclusions are not novel enough  
   to warrant the amount of effort devoted to their generation.

**The revised version of the paper has been substantially shortened. The initial submission was 9000 words. The revised submission is 6500 words. While a small subset of the data was originally published in Douglass et al (2018), the majority of the data included in the manuscript, has never been published. We have revised the discussion to further highlight the novel aspects of the findings.**

**Reviewer 1:**

1. Style:

- Editing of the manuscript is needed; leaving typos aside (which are  
abundant), reading flow is not always fluent, to the point that some  
sentences don't make sense (e.g. Lines 65-66, sentence 'Understanding the  
role of function…'; beginning of caption in Figure 3; even the sentence in  
bold in lines 471-72 is difficult to understand and contains typos).  
- There are also some repetitions. In the methods section, for example, a  
few things are reiterated several times, including some (e.g. reduction  
intensity according to Douglass et al 2017) that are repeated almost  
identically in consecutive paragraphs (e.g. lines 246… and lines 269…).  
- Referencing style is careless and a little chaotic.

**See comments above.**

1. Organization:

-The introduction and background sections are not clearly differentiated,  
and it is not always easy to follow the thread of ideas presented in each  
paragraph (from functional analysis there is a leap to ecological  
perspectives to variables used to measure variation…).  
-These introductory sections are a bit vague overall, presenting a  
disjointed overview of Oldowan literature. The introduction, for instance,  
does not really address what fresh data exactly is going to be presented in  
this manuscript, and it is not until the methods section that the reader  
learns what exactly the author/s intend/s to accomplish

**See comments above**

- In the methods section, while some variables are mentioned without an  
explanation or a linked citation, others (e.g., estimation of flake sequence  
values) receive lengthy descriptions even if they are already published.

**The method section has been revised and made more concise.**

1. **Data presentation:**

**-** There is no materialssection, so it is not possible to learn the size of  
the assemblage, the archaeological levels analyzed, categories represented,  
proportions of raw materials, etc. For instance, in the results a great  
emphasis is (appropriately) put into distance of raw material sources to  
explain core size variation, but because no systematic description of the  
distribution of such raw material is given, it is very difficult to follow  
their interpretation of results. -The results section has no tables with quantitative data, no percentages are given to support interpretation of results, etc. This is one of the most  
serious shortcomings of the paper, as the reader has no way to assess the  
sample used in the analysis.

**We have revised the introduction to Kanjera South to better describe the previous research and the landscape context of the assemblage that is studied. We have also included a materials section in the methods along with summary tables of the data included in the study. The raw data is also made available on Git-hub. It is important to point out that the review says that our results focus on explaining core size variation. This is incorrect. Even in the initial draft we explain that the analysis of the cores predicts the percentage of mass that has been lost from the core as proxy for reduction intensity. This is not only noted in the methods section but also in the Y axes in the figures in the results. Though it may seem pedantic this an important point to raise in light of the reviewers issues with the originality of the findings (See below).**

1. Originality:  
   - Figure 1 is based on an illustration already published and thus is  
   probably unnecessary, while Figure 2 is rather vague and contains much less  
   detail than any other map of Kanjera that I have seen published before.

**Figure 1 has been removed from the text. The main point of figure 2 is not provide a comprehensive illustration of geographic context of Kanjera South as several maps that serve this purpose have been published before. The information portrayed on the map is kept simple to more clearly illustrate the provenance of the raw materials.**

- Background to Kanjera: while it is important to provide a context to the  
study, this background takes three pages of the manuscript which present no  
new data but just an overview of already published materials.

**The background has been revised to focus more on contextualizing the analysis presented. See comments regarding the originality of the data.**

- The main conclusion is that raw material distance is an essential factor  
explaining core reduction intensity in Kanjera, but that a distance-decay  
model does not fully explain hominin behavior. This is certainly important,  
but most of this has already been well demonstrated in several papers by  
Braun and colleagues. It does not get clear at all how this manuscript  
furthers earlier work.

**We have revised the discussion to better illustrate how this work builds on other research.**

- Better justice could be done to classic work on the Oldowan. For instance,  
Potts 1988 is an important source for spatial dynamics in the Oldowan, while  
Toth (1982) -and also Potts (1994)- had already long proposed the natural  
progress to bifacial and multifacial cores from unifacial cores, well before  
Moore and Perston.

**Potts (1988) is now cited. While Toth (1982) is not cited in the paper, Toth (1985), which is a concise overview of the main conclusions in this dissertation, was cited. We have now included Toth 1982 in the bibliography. However, while reviewer 1 is right to point out that Toth and Potts suggested that core variation in Oldowan may reflect a continuum of use, these studies focus on explaining the differences between Mary Leakey’s tool types. Toth’s (1982) proposal of the existence of a reduction continuum, like Moore’s (2016), are based primarily on experimental data. Potts (1991) proposal is based on differences in mass between Leakey’s tool types. Mass as a proxy for core reduction intensity is problematic because not all rocks that become cores start at the same size. Our work builds upon these initial proposals by explicitly quantifying core reduction intensity through the estimation of the amount of mass lost from each core. The findings of the paper here are far more original than reviewer 1 suggests as it explicitly tests and validates what has not been previously tested. Moreover this work further compares reduction intensity against commonly used technological classifications that are often argued to intentionally imposed flake removal strategies (de la Torre and Mora, 2005, 2018; Stout et al., 2010, 2019; de la Torre Ignacio, 2011; de la Torre et al., 2021) as opposed to the Leakey’s tool types which are, at present, seldom used to describe Oldowan lithic variation.**

- Caption of Figure 6. Have these reduction strategies been defined by the  
authors or follow a published source?

**Yes (de la Torre and Mora, 2005, 2018; Stout et al., 2010, 2019; de la Torre Ignacio, 2011; de la Torre et al., 2021).**

**Reviewer 2:**

The paper amasses a lot of fancy statistics to make its point (more on this later). The results are precise and very meticulously tested. I was surprised that there are no basic data whatsoever in the paper. (the exception being the list of raw materials of which the artifacts were made; curiously for such a number-oriented contribution, the quantitative distribution of these materials is not presented…). I get that the paper is about applying a formal quantitative model to the archaeological material (to a large degree, this is a repetition of the test case presented in Douglass et al. 2018), but given that this is an actual rather than model case study, it is imperative that the data be presented. I would have liked  
to see data on the distribution of raw material in the assemblages (and by which unit of comparison - it can be weight, number of cores, number of flakes or whatever, but it needs to be transparent. Since the paper is about transport, use, technology and how they may or may not mesh together, information about size and mass, how the various materials were flaked (by whichever proxy is chosen) is important.

**This has already been addressed above.**

It would also be useful to be given some information about the size distribution of cobbles in the local vs. non-local sources, to better assess some of the patterns that may be related to transport over distance.

**This has been addressed in the methods section of the test. There are generally few differences in terms of the size distribution of cobbles between local and exotic materials.**

The author elaborated: (lines 456-460): "…. flakes produced on local rock types have shorter use-lives because their edges dull more quickly. The consequence of this, is that for any given use, more flakes of a local raw material are required. In this sense, the high abundance of these materials and the relatively quick dulling of their cutting edges may explain the high frequency of local raw materials in the Kanjera South assemblage." Given the properties of the raw material as described, is it possible that attempts to extend a core's use life would be more costly than simply picking up another cobble and start reducing it? This is surely a behavioral pattern that is known in sites located near raw material sources   
(as discussed in the final section of paper by the author himself), from all periods. In such a scenario, it is not the functional properties of the flakes but the lesser flaking properties of the cores that dictate the high abundance of local raw material. In the absence of the basic distribution data on cores and flakes according to raw material, the term "abundance of raw material" is opaque, and it is hard to  
evaluate the possibility, let alone refute, either one of the hypotheses.

**The addition of new tables and supplementary information will allow the reviewer to better assess the concerns stated here.**

Another outcome of the study is that flakes associated with exotic materials are often from later in the sequence than those associated with local raw materials. Contrary to expectations from a distance decay model variance in the amount of reduction of the exotic cores is as high as in the local cores, which would for intentional attempts to increase the use life of artifacts from distant sources. The author suggests, based on previous models (but one in an unpublished thesis) that this indicates directional movement towards KS. This is an interesting view that speaks to the decision  
making of hominins. It would have been good to emphasize how, exactly, the paleo-environment of KS differs from other parts of the Homa peninsula (especially those located between the assumed sources of exotic raw material and KS) that make this a particular and specific magnet for hominins and why would they direct their movement to this particular location.

**This is an interesting suggestion and would further elucidate whether the Homa Peninsula was simply an attractive place or whether there was something special about Kanjera South. Unfortunately no data that could contribute to this point is available. This is largely due to the fact that faulting on the Homa Peninsula makes it difficult to carry out landscape scale studies of a specific time period. For example, Kanjera North which is only a few hundred meters from Kanjera South, is thought to be younger in age.**

The faunal assemblages and the results of use wear studies are enlisted to explain how KS was a location of attraction. The faunal material is indeed exceptional for the Oldowan earlier than 2 Ma in the amount of anthropogenic modifications that suggest direct and intensive hominin involvement in the accumulation of the assemblage. Interestingly, the use wear studies do NOT indicate that this was the major focus of stone tool usage, whereas other types of use are more frequent. This is not easily reconciled with the argument that hominins schlepped stone tools on purpose from distance sources to KS in order to exploit the fauna. As to other uses, they are defined rather broadly by the use wear analysis, and likely also occurred in other sites where the patterns of lithic reduction are less clear (or clearly different.).

**The point of this section of the paper is not to suggest that faunal exploitation was the primary driver of hominin activities at Kanjera South. Rather, given both the use-wear and faunal data, we suggest that Kanjera South was an attractive place to use stone tools given the breadth of activities that seem to have been carried out there. We hope that revisions to the discussion better emphasize this.**

The formal modeling and data manipulation in this paper do serve the purpose of fleshing out the result of many previous informal models that reached similar interpretations about core reduction, transport, cultural transmission, site functions and mobility. I fully concur with the conclusion of the paper, that "studies [of Oldowan technology] should examine the Oldowan within the broader socioecological system.", for which the author cites many other examples. This study is a valid attempt at a better understanding of the Oldowan. That the take home message is the same as those from many other studies dealing with this earliest technocomplex is a bit of an anticlimax to all the effort invested in the analytical tools.

**This has been addressed above.**

Line 36: consider adding (Hovers, 2009), where the issue of flaking principles is addressed, though from a slightly different perspective than the other cited papers.

**This has been added.**

Line 39: add (Goldman-Neuman and Hovers, 2012) to the citations pertinent to raw material properties/selectivity.

**This has been added.**

Line 45: "While socio-cognitive approaches only require examining technological strategies present in the artifacts themselves, ecological analyses require integrated ecological and functional data sets. Demonstrating the influence of ecological parameters on stone tool use requires establishing spatial relationships between measures of stone tool utilization and landscape features such as raw material sources and paleogeographic settings." I am not sure what "socio-cognitive" approaches. Minimally, these should be well referenced as are the ecological approaches cited at the end of this sentence. If the author refers to the châine opératoire approach, this statement is incorrect. The CO is considered a techno-economic view of lithic production and use. For a very good exposition, the author should check Soressi and Geneste (2011). They are cited later on (line 107) in a different context, but their review, and the references therein, make it amply e evident that the CO is focused on economic/ecological factors as much as on social and cognitive ones.

**This section has been completely revised.**

Line 76: The saying that "tools as an extra-somatic mechanism for solving  
environmental problems (Binford 2001)." Is a paraphrase on Leslie' White,  
Binford's own teacher, and not Binford's himself. Attribution should be made  
to him, perhaps

**This line has been deleted.**

Line 126: Stout et al. 2010 did talk about ecological differences that underlay the cultural transmission processes. It is not entirely correct to align their papers (especially the earlier one cited) with exclusively the "cultural school" of Oldowan stone tool variability.

**This has been updated.**

Line 138: "Toth types" is a term that is well-known to researchers of the Oldowan but needs to be elaborated upon in a formal paper.

**This change has been made.**

Line 152: "Moreover, it may be that Oldowan stone tool variation may be the result…" language edit is needed.

**This change has been made.**

Lines 180-199: The context of the locality (/ties). Should be better clarified. Artifacts are said to derive from two occupation horizons KS-1 and KS-3, and then it is stated that the bulk of material is from one of them (presumably KS-3). It would be useful to have information on the lateral extent, thickness and N of lithic artifacts (above 20mm in size? All of them?) per occupation horizon, and - since the fossils are brought to bear on the interpretation of the paper - the same would be useful also for the fossils or at least the most abundant classes, body size groups or any other category chosen. This can be in the text or in tabular format, but I think it is necessary for the message this paper is trying to deliver.

**This issue has been addressed in both the revisions to the methods and discussion.**

Lines 201-203: Figure caption needs editing.

**Edited.**

Lines 204-213: "…. two features that distinguish the Kanjera South assemblage from many other Oldowan assemblages. …. The second is the systematic selection of rocks that have specific mechanical properties (D.R. Braun, Plummer, Ferraro, et al. 2009)." This statement needs to be phrased differently. All the studies that focused on Oldowan raw materials actually identify this exact characteristics. The selection of a lithology is not due to preference of a rock type per sse, but to its mechanical  
properties - size, angularity, flaking properties. This has been documented in the preferences of phonolites over other raw materials in Lokalalie 2c, the over-representation of good quality rocks (regardless of geological definition of rock lithology) in Hadar, and the differential use of lava and  
quartz at Olduvai (de la Torre and Mora Torcal, 2005). I would agree that Braun et in their numerous studies of KS raw materials were the only ones that used engineering tools to provide  
a quantitative description of these properties during use. If this is what the author means, it should be phrased clearly.

**This sections has been revised.**

Lines 226-227 : "Most of these actions involve cutting, and the different raw materials appear to be used on different substrates." However, Lemoriniet al (2019) state explicitly about Kanjera South that "Data assembled so far suggest that tools fashioned from non-local and local stone were, with  
one possible exception, used to process similar materials.", which seems to  
contradict the author's statement.

**This has been fixed.**

Lines 227-228: please correct grammatical error.

**Fixed.**

Lines 228- 231": Combined with the transport information, the sum of the  
information about stone artifacts at Kanjera South suggests that stone  
artifacts represented a significant component of the extractive foraging  
adaptation of Oldowan hominins." I am not sure what 'significant' means in  
this context. Also, it is not clear if the author argues that this is a  
unique facet of KS, which I would then consider a false statement, or this  
is simply a general statement regarding the Oldowan (and probably each  
lithic assemblage in most periods, certainly the earlier ones), which then  
needs to be better phrased.

**This section has been revised.**

Lines 247: the references should be (Douglass et al., 2018)and should be  
updated throughout the manuscript.

**This has been updated.**

Lines 277-279: "The number of exploitation surfaces refers to the number of  
areas of the core where flakes were removed along a similar axis. This  
variable is related to core rotation which is argued to increase as core  
reduction increases (e.g. Delagnes and Roche 2005).". This definition  
follows from Dougalss et al. 2018, but it is problematic. The axes from  
which flakes are removed on A SINGLE surface are indeed related to the  
rotation of the core during the flaking process. Flaking surfaces "happen"  
when the core is flipped, unrelated to rotation (i.e., it may be rotated or  
not). Please consider changing terminology.

**This terminology comes from a previous published source as noted. We wish to keep the terminology unchanged for the sake of the method’s reproducibility.**

Line 337: replace 'tacks' with 'tasks'.

**This has been revised.**

Lines 386-387: "When core reduction intensity is compared according by core  
reduction strategy an interesting pattern emerges." Correct grammar.

**This has been revised.**

Lines 387-391: Unifacial and unipolar, core reduction strategies result in less reduction than strategies that require bifacial, multifacial or polyhedral strategies (Kruskal Wallis, P-value: < .001) (Figure 6). In other words, core reduction strategies that require fewer core rotations, such as unifacial and unidirectional strategies, are less reduced than those that involve more complex rotation strategies." This sentence is problematic, and I think this is because of the inaccurate way that surface and rotations of the core are linked (see comment above): bifacial, multifacial and polyhedral strategies involved flipping of the core and exploiting more of its volume as a result. Each of the surface can be exploited unifacially (for that specific surface) by rotation, but the core would still be heavily reduced.. The sentence compared two not-of-kind properties of reduction.

**This is not necessarily true in all cases. While rotation and reduction are undoubtedly linked, the amount of material lost from the core is related to the size or volume of the flake removed and not rotation. For example, if a unifacial core, bifacial core, and multifacial core all have 10 flakes removed, the amount of volume removed will be dependent on the size of the flakes removed. If the size of the flakes removed all the same, then these three cores will be reduced the same amount regardless of the fact they have been rotated different amounts.**

**We do agree that more flakes must be removed to produce a polyhedron than a unifacial core but then the issue becomes whether the continuous flipping of the core is intentional or not. Studies have shown that polyhedrons can arise due to both intention and non-intentional reduction strategies (Toth, 1985; Texier and Roche, 1995; Moore and Perston, 2016).**

Lines 515-516: "Interestingly the different reduction strategies present at Kanjera South seem to be related to reduction intensity". I thihk this is a tautological and therefore meaningless statement. The reduction strategies are WHAT creates the reduction intensity. This is a point that is amply discussed in the work on Peninj and the classification of the cores (de la Torre et al., 2003).

**The broader point that this statement attempts to make is that given the relationship between reduction intensity, raw material transport distance and reduction strategy in the Kanjera South assemblage, broader ecological factors may be conditioning the technological strategies applied to stone at Kanjera South. We hope that the revisions to the discussion clarify this point.**

**Reviewer 3:**

This is a very nice paper on a very high quality data set from Kanjera South. The paper is tight and succinct and easy to read. I think the analysis is good, and it makes some very interesting points. I definitely recommend publication. My greatest concern is with the background section. As I discuss some below, I think the authors have mischaracterized a bit the technological approach and appear to position it in contrast to an ecological approach. I would say that most practitioners of the technological approach think that ecology (often called economy - maybe not exactly the same thing but pretty close in its application) is already baked into their analysis. What I think the authors are really trying to say is that there is more work to be done at the ecological level, especially as regards landscape variation and raw material transport, and that this additional work may shed light on variability that up to now has  
been attributed to higher orders of explanation. I would encourage them to revisit this part of their presentation and modify a bit.

**See the comments addressed above.**

What follows are some more details points and edits (there are frequent  
typos):

**See comments above.**  
  
Is this statement - From this perspective, the variation seen in all  
technology stems from socially mediated images and thoughts (Inizan et al.  
1999) - an exaggerated characterization? I think most practitioners of this  
approach would not attribute all variation to socially mediated images and  
thoughts.

**This section has been revised**  
  
  
"At Gona, differences in the proportion of reduction strategies between  
sites have been  
argued to reflect cultural traditions of specific groups (Stout et al. 2010,  
2019)." - Yes, but he did this after trying to eliminate other types of  
explanations, including ecological, right? In other words, it is hardly an  
example of Inizian in the way he reached this conclusion even if the end  
result is the same. I think the same could be said for Delagnes and Roche.  
I think they had trouble seeing what other variables might account for the  
differences they saw rather than starting from the perspective that it must  
be species or culture (see also Harmand 2009).

**This section has been revised.**

In a way, this is problematic because as figure demonstrates - missing  
figure reference the factors that contribute to stone tool variability are not mutually  
exclusive and likely interact with one another - missing period at the end  
of the sentence and I wonder if Oldowan researchers really see things as  
this black and white.

**See comments above on the restructuring of the background.**

In other words, without considering the broader ecological context of the  
Oldowan it may be impossible to disentangle technical competence of Oldowan  
knapppers from technological variability (or vice versa). - Again, maybe  
others have done a bad job of it, but I think that most Oldowan papers do  
give some consideration to ecological variables before reaching larger  
conclusions.

**See comments above.**

This paragraph is confusing - The lithic assemblage recovered from  
exacavations at Kanjera South represents one of the largest single  
accumulations of Oldowan artifacts in association with modified fossil bone  
(Plummer 2004). The largest site (169 m2) yielding the bulk of the  
archaeological finds was Excavation 1. The frequencies of different bovids  
in these beds indicates that the Kanjera South landscape, unlike the setting  
of most of the Oldowan sites, was dominated by a grasslands as opposed to  
trees. - The second sentence introduces Excavation 1. But are the remaining  
sentences also talking about Excavation 1 or just in general?

**This section has been revised. References that point to more detailed descriptions of the Kanjera South assemblage have also been provided.**

I feel like these paragraphs giving the background could be revisited and  
better organized into units of information (e.g. geology, dating, site  
formation, paleoenvironment, fauna, lithics). For instance, line 181 is  
about site formation. But then again so is line 193. At one point early 3  
m of sediment are mentioned and then a few sentences later 30 meters of  
sediments are noted.

**This section has been revised.**  
  
I am wondering what the basis of this statement might be - The selection of  
certain rock types exceeds that which is seen in other Oldowan assemblages.  
I know that at other Oldowan sites it has been demonstrated that particular  
rocks are being selected. Why is it that here it exceeds what is seen at  
other sites?

**This section has been revised.**  
  
Is Kanjera alone the same thing as Kanjera South? (e.g. 224 but also  
elsewhere)

**Kanjera has been changed to Kanjera South throughout the manuscript**  
  
This model considers, the number of flake scars, exploitation surfaces, the  
number of exploitation surface convergences, the proportion of cortex, and  
average platform angle to estimate core reduction intensity. [should be - To  
estimate core reduction intensity, this model considers the number of flake  
scars, the number of exploitation surfaces, the number of exploitation  
surface convergences, the proportion of cortex, and the average platform  
angle. ]

**Fixed**

I am not sure about the use of "intuitively" in this sentence - The number  
of flake scars intuitively refers to the number of previous flake removals  
present on the core. It would seem to suggest a follow up sentence that then  
explains why it is not this way. Also, is there a minimum size of a scar  
that gets counted?

**Revised**   
  
Here again I wonder about the use of intuitive - The proportion of cortex  
has an intuitive relationship with core reduction intensity. And is it said  
somewhere how cortex is measured?

**Details regarding how cortex is measured can be found in Douglass et al. (2018).**

On average platform angle, this is an average of what exactly? Every flake  
initiation scar on the core is measured and then this is averaged?

**The sections describing these attributes has been revised. These sections should be clearer. Details regarding the specifics of this measure can also be found in Douglass et al (2018).**  
  
The part starting with "Specifically" on line 287 might be best placed at  
the end of the previous paragraph and then some sort of helper sentence can  
start this paragraph that gives more details on the components of the  
Douglass model. In any case, the specifically used here looks out of place.

**Clarified.**

Line 305 - maybe delete "clearly" as it makes it sound like a reviewer  
response file.

**Deleted.**   
  
Line 308 - Flake sequence estimates have a maximum error between +/- 8  
sequences. Where does this come from? The Braun Tactikos paper?

**The Braun Tactikos reference has been added to this line.**  
  
Line 309 - I am having trouble with squaring these sentences with the  
criticism of Toth which says that his model give only the relative position.  
Seems like both approaches arrive at the same place.

**This has been clarified in the methods**  
  
In the edge to mass section, seems like some reference to the work of Rezek  
et al. 2018 would be appropriate.

**This citation has been added.**

Line 326-327. Mass and volume are nearly the same, but not quite. Are  
these two sentences suggesting two separate methods? Is the volume comment  
relevant if mass is what these authors are using or can the wording of the  
volume sentence be nuanced?

**This has been clarified.**

Line 328 - measure of edge length?

**Fixed**  
  
Line 329 - edge length estimate?

**Fixed**  
  
Line 332 - It isn't clear to me why log transforming mass corrects for the  
fact that small flakes are not necessarily the most efficient at certain  
tasks. I think you are log transforming because as size increases, mass  
increases more quickly than does edge length. I think the difference should  
be a power of 3, but I'm not sure. So whether a log scale (presumably a log  
10) or whether the cube root is better, I'm not sure.

**Given other research that deals with size effects of lithics, it does make sense to cubed root mass as well. In fact, using a cubed root as opposed to a log transformation increases the differences observed between the exotic and local assemblages. However, the log transformation follows previously published work on the method (Braun and Harris, 2003; Dogandžić et al., 2015).**   
  
When core reduction intensity is compared according by core reduction  
strategy an interesting pattern emerges. [delete according]

**Fixed.**

Unifacial and unipolar, core reduction strategies result in less reduction  
than strategies that require bifacial, multifacial or polyhedral strategies  
(Kruskal Wallis, P-value: < .001) (Figure 6). [delete first comma]

**Fixed.**  
  
Line 419 or so - The flakes that come from far away - they arrived on site  
as flakes or they were knapped on site from cores that arrived from far  
away? If the former, could it be that these are the flakes that are  
selected for transport and not that they were specifically made that way  
because of their raw material? The way it is written, one could think that  
the authors are suggesting that the need to transport was anticipated and  
that the cores were reduced in a different way as a result. Is that really  
what they mean to say? And if the flakes were made on site, could it be  
that efficiency just naturally varies with reduction intensity? In other  
words, do more intensively worked cores produce more efficient flakes? Is  
there are correlation in their dataset between stage and efficiency?

**Our general argument is that the flakes produced on more distant materials were made from cores that had arrived on site. The notion that more reduced cores naturally produce more efficient flakes is a really interesting point. This is something that I have often wondered about as well. We have added a panel to the figure and text in the edge to mass ratio section to investigate the relationship between flake sequence and efficiency. This shows, at least within the Kanjera South assemblage, that there is no relationship between edge to mass ratio and flake sequence.**   
  
Line 483 - This paragraph is interesting. But then what are you saying?  
When you look at materials with the same material properties, one local and  
one exotic, you still see more reduction on the exotic even though their  
movement from the source to this site is directed? So they place a higher  
value on the exotic even though the properties are the same? Or this is  
just a stochastic variant on the distance-decay model (distance-decay but  
with direction)?

**We have revised this section of the manuscript. We hope that our argument is articulated more clearly.**   
  
Line 573 - This is all good, but I am having trouble integrating it with the  
finding that variation (as measured by IQR) is the same between local and  
exotic. Distance-decay removes intentionality from some raw material  
patterning. However, here distance-decay doesn't quite fit. But it is not  
intentionality because it is distance-decay plus directed movement? And  
that gets you more reduced cores and flakes but similar IQRs?

**We have clarified this section in the revised manuscript.**

Line 586 - I think the Iovita paper is about later period bifaces, right?  
And I worry about calling these LCTs. And I think the point of that paper  
was that different reduction trajectories were present in the assemblages  
they looked at. Is that really the right example here?

**We have replaced this reference with those that are more relevant to the submission.**  
  
Note to the editor - My review copy included a Latex dump at the end of the  
manuscript that revealed the authors of the paper in the form of the  
filename of the original submission.  
  
Figure 6 - Do I understand correctly that the right figure is also the color  
key for the left figure? And could the authors actually use colors instead  
of shades of grey?

**We have updated the caption and color ramp to help with the interpretation of the figure.**  
  
Figure 7 - So even early stage reduction flakes made on distant raw  
materials have a more efficient edge to mass ratio than similar flakes on  
local materials? A single figure showing this would be useful I think. I  
worry that efficiency increases with core reduction intensity and so these  
two measures are in effect one.  
  
Also, I guess what makes the flake efficient is a high EPA. And so on high  
quality material they use a high EPA from the start? And the high quality  
local material is also this way (because there is a least one high quality  
local if I remember correctly)? And quality is measured by edge durability  
not how knappable it is? So it is not just a question of being better able  
to maintain a high EPA on high quality material?

**I have plotted the relationship between flake sequence and the edge to mass ratio. There is no systematic relationship between flake sequence and edge to mass ratio. I have added a panel to figure 6 to illustrate this.**

So no data are included with this paper? And there is no SI? I ask because  
the reporting of the statistics is rather sparse (name of statistic and P  
value). There is no reporting of actual statistical values, rather  
everything is in the figures. On the one hand, it makes the paper quite  
readable. On the other hand, it makes it a bit harder to evaluate and to  
compare. As I was reading it, I figured the authors did not include more  
reporting and data in the paper because they had included the data as an SI.  
Apparently not? I think that if no data are included, then it might be  
required to do a more full reporting of the numbers behind each statistical  
test. As it is, for instance, I would be hard pressed to say whether my  
flakes are more or less efficient that these.

**See comments above.**

Line 60 - The results of this study contribute to the exploration of  
ecological and socio-cultural  
influences on Oldowan artifact variation. - I am not sure this paper  
contributes to the socio-cultural influences on stone artifact variation.  
In a few places it mentions that their model can't explain all of the  
variability without going further, but I have difficulty seeing the  
contribution.

**This has been revised.**   
  
Line 105 - actions and thoughts are then culture and cognition here (i.e. in  
keeping with the list on line 64)? When I read this paragraph I think about  
the Harmand 2009 paper where she compares raw material selection and  
technology between Lokalalei 2c and 1. She calls her study a technological  
approach very much in keeping with what has been outlined here. But the  
actual analysis includes an ecological approach as well (i.e. arguments that  
raw material availability and quality is the same at the two sites etc.)  
While the authors here might disagree, I suspect that Harmand would argue  
that she has included already an ecological approach and that it is not  
enough to account for the observed differences between these two sites. In  
other words, I don't think they view it as an alternative approach. Rather,  
I think they would say that ecology is already built into the approach.

**This has been revised.**  
  
This said, the partial paragraph starting on line 137 is well taken.  
Really, what I think these authors are saying is that there remain  
ecological factors (up to now not well enough considered) that may account  
for differences that have in some cases been attributed to  
social-cultural-cognitive factors. I see this as different from saying  
there are two kinds of approaches.

**This has been revised.**

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