Reply to the anonymous reviewer 2

We would like to thank the reviewer for his/her comments. The remarks have definitely improved the manuscript. Below you will find a point-by-point reply, with the review given in **black** and our reply given in **blue**. We hope that we have answered all questions sufficiently. The line numbers we included in our answers refer to the marked changes document.

General Comments

The paper by de Boer and coauthors compares transient records of runoff from the Sahara and the Sahel that is produced by an intermediate complexity model with the dust record from 659 and the Ti/Al XRF record from 967 across the intensification of Northern Hemisphere glaciation. They find there these is strong precession-scale variability in all of the records, and that obliquity, thought to be driven by the high latitude ice sheets, has more of an impact on the Sahara following the intensification of NH glaciation. Authors also use a clever technique of correlating the empirical records with a range of combinations of grid cells to find the most likely combination of regional impact on the sediment and look at how this combination changes through time. The methodological steps are laid out in a fairly clear way with useful figures, but often the details of the model and time series bog down the interpretation. There is little conclusion on what the lags, relationship between runoff and dust, and the change in regional input to the Mediterranean really means in terms of climate dynamics, and also what it means for humans evolving in East Africa. Please find my specific comments with more detail, as well as some technical comments on the text and figures, below.

We thank the reviewer for his comments, please find below our reply to the specific comments.

Specific Comments

There needs to be more background in the first paragraph of the introduction. There are newer studies that have good enough age constraints to examine the phase relationships between orbital properties and the MPT and other transitions. I think authors also need to make it clear that the 100 kyr cycle after the MPT is likely an average of 120 and 80, as many have recently shown. Further, the MPT is not studied in this research, so perhaps removing that from the introduction would leave more room to provide better detail on other arguments.

Yes, we agree with the MPT characteristics. We will adjust the introduction accordingly, and mention the onset of NH glaciation instead at this stage of the introduction. This is also a key point of our paper, and will be more elaborately discussed in the introduction.

There also need to be far more citations for a sentence like line 39-41, where the authors link orbitally induced variability in the tropics. This has been seen in Africa in many different records by Tierney, Lupien (both Pliocene and Pleistocene), Rose, etc. In the conclusions (line 239-241) there is a vague sentence on this as well – orbital induced variations of what? If authors are talking about precession in climate records from Africa, there need to be many more citations.

Yes, we have extended this part of the introduction by showing that the changes are far broader. The new part is (lines 41-46):
"The orbital-induced variability is not limited to the high latitudes, it is also seen over the entire African continent. North African monsoonal records are linked with runoff and precipitation (e.g. Lourens et al., 2010), which persisted throughout the Pleistocene (Wagner et al., 2019). Changes are seen during the late Pliocene and mid-Pleistocene in vegetation in northeast Africa (Rose et al., 2016), during the Pleistocene in Kenya (Lupien et al., 2018), the West African monsoon (Kuechler et al., 2018) and hydroclimate variability in southeastern Africa over the past 2 million years (Caley et al., 2018)."

In the conclusions the sentence is revised to (lines 276-277):
"Previous studies have shown the strong presence of orbital induced variations of the African monsoon, that could .."

Similarly, the third paragraph of the intro on hominin evolution is very light in citations and seems to pick specific details from Joordens 2019 rather than focusing on the evidence and mechanisms for hominin transitions at the onset of NH glaciation or the variability selection hypothesis (Potts).

Yes, this bit is indeed weighting on the paper of Joordens et al, since she was involved in the earlier part of this project, as an expert on this specific topic. Therefore, also the reasoning for this paragraph. We have tried to extend it a bit more with adding more references, although the mentioned is rather general, since not our expertise (lines 49-57) :
"Specific climate transitions, among which the Plio-Pleistocene transition, coincide with the possible emergence or extinction of hominin species (Donges et al., 2011). ...
Condition of highly variable climate and strong seasonality during eccentricity maxima would result in isolated refugium for early hominins, that would be conclusive for evolution (Trauth et al., 2007). ...
However, the evolution of the hominin species throughout the Pleistocene is a highly complex process (Mounier and Mirazón Lahr, 2019)."

'Continental runoff (i.e. p-e)' is a bit misleading. This p-e balance is not equivalent to runoff as water can be stored in soil, lakes, and groundwater, which is fully stated in the description of the model. This is brought up again in line 129 and onward and needs to be explained more clearly and thoroughly before the interpretations are stated.

Yes, we understand that this relationship is not as straightforward as we imply here. We will explain this in better detail in the introduction, changes are made in lines 58-59:

" In this paper we focus on the connection between the African monsoon, using continental runoff (linked to precipitation, evaporation, and water storage in the soil, lakes and groundwater)."

And lines 142-144:
" Runoff over the Northern African continent as modelled in CLIMBER-2 largely results from the difference between precipitation and evaporation over land, although water can also be stored in the soil, lakes and groundwater."

Figure 1 has the model grid cells, but they are not plotted over a map based on the 6 potential surfaces. This would be helpful.

Yes, we understand that could be helpful, but we would like to focus the map on Africa, since potential surfaces also include ice sheets and sea ice, which can only be viewed on a global map.

Why not use the principal component analysis, based on Ti/Al, from Grant 2017 for the 967 comparison? This is brought up later, and perhaps is a better estimate of different line types for the different frequencies, and in c, use blue.

The main reason behind this is that we are presenting an extension of the Ti/Al record of Site 967. The Grant (2017) wet-dry index also does not extend before 3.0 Myr ago. Secondly, the wet-dry index is a product of XRF scanning and a PC analysis, so less straightforward as the terrigeneous Ti/Al data.

Line 131: the location of the Sahel today is shown in Figure 1, but the location, spread, vegetation may well have been very different in the past, particularly in the Pliocene. How does this affect the grid cell coverage?

Following from a comment of Reviewer 1, we have added a figure that shows the distribution of the vegetation fractions over time from 3.2 to 2.3 Myr ago. This figure clearly shows that gridbox 11 shows a vegetation distribution consistent with the Sahel. When introducing the figure we will mention this, referring to Figure 1 (lines 149-151):
" Both gridboxes show high variability of the vegetation fraction, but do show a clear linkage to the present-day coverage of partially desert and vegetation for gridbox 11 and desert for gridbox 12 (Figure 1)."

The discussion surrounding the lags seems a bit tangential and perhaps unnecessary. If the lag is due to a choice in the LR04 tuning, then does this analysis tell us anything new?

Yes, a large part of the records being in phase is due to the tuning (relative to LR04). We do think it is important to inform the reviewer that when including the ice-sheet forcing an additional lag is introduced in the system. This lag is than even seen in low latitude records, we do think that that is worth mentioning.

Line 207: the power spectra should not be influenced by lack of data. Either resample the data appropriately, or put the constraints up front in the methods section. Can authors resolve precession throughout the records? If not, then conclusions based on evolutive spectra shouldn't be made, or should be modified.

Yes, we agree. this sentence has been changed to (lines 241-245):
"For the dust record of Site 659, besides the eccentricity minima a dip in the 23-kyr power around ~2.6 Myr ago is seen (Figure 7b,d), for which we would expect 23-kyr power to stay high following the 400-kyr eccentricity maximum (Figure 2a). This also underlines the non-linear link between dust and runoff as illustrated in Figure 8."

What are the conclusions of the mechanisms linking the ice sheet inception to the change in runoff region source? The conclusions section appears to be more of a summary. Try to utilize this space for connections back to the topics brought up in the introduction – what about human evolution? Would this shift, and other aspects of the findings, impact humans, and how?

Following a comment of reviewer 1 we have added a full paragraph in the Discussion on the forcing records, naturally influencing the outcome of the CLIMBER-2 model simulations. We have added additional sentences on the link to the shift after NH ice sheets increase in size, and the link to human evolution (lines 291-292):
"Although the evolution and dispersal of hominin species during this time could be linked to orbital variations (Joordens et al., 2019), a direct link with the climatic variations shown by CLIMBER-2 cannot directly be established."

Technical Corrections

"Myr age" can be replaced by "Ma" in every circumstance
We would rather stick to our common usage of Myr ago.

Line 42: replace 'deterioration'
Replaced with 'variability'

Line 42: 'central' Africa? Do authors mean East Africa?
Yes, corrected

Figure 3: the colors need to match better for clarity. Perhaps in b, use orange with different line types for the different frequencies, and in c, use blue.
We would like to stick to the colours as is, since we use the same colours in Figure 7 (which in the new version is the new figure 9).

If Table 1 could be shown in a figure, I think the point of lags would be much easier to comprehend by the reader. Perhaps use phase wheels.
Since it is only a minor part of the discussion, we would keep Table 1 as is.

Figure 6: the legend should be changed to either show the location or the proxy, not the proxy for one site, and the site number for the other.
Changed

Try not to conflate the Plio-Pleistocene transition with the onset of NH glaciation – authors have cited these at two distinct times (2.6 vs 2.8 Ma), so try to keep the wording consistent.
Yes, we have checked this through the text

There are multiple instances of awkward phrasing, run-on sentences, and misused gerunds, so further editing for grammar could benefit the clarity of the manuscript.
Yes, we will go through the new version of the manuscript thoroughly.