Response to Reviewer 2

We are very grateful to The Reviewer for taking their time to review our manuscript. Their supportive comments and suggestions have enabled us to further improve the clarity of our manuscript.
The original comments from the reviewer are displayed in highlighted blocks, and below them are our responses to each of the comments.

> **Comment 1.**
> This manuscript does not clearly articulate a valid research question.

Regarding the critical issue raised in the second point of the reviewer's general comment, we understand on the basis of the comments from reviewer 1 that the reviewer has experienced significant difficulty in understanding the primary issue, which it is our purpose to address in this manuscript. In order to make absolutely clear what our purpose is in this work, we have produced a new figure, shown on page 3 as an aid in providing the needed clarification. This figure will be included in the revision of the paper we are preparing. It shows in part a (redrawn from deMenocal et al. (2000)) dated demonstrating that the the most recent GS period began at 14.5ka and ended at approximately 5.5ka. In part b of this figure (redrawn from Roberts et al. (2011)) we provide the palynological information demonstrating that the end of the Green Sahara period was accompanied by a marked change in the land cover of the Levant, from one of forests to one of grasslands. Our purpose in this paper is to demonstrate that this is not a chance correlation, but rather associated with causation in that the end of Green Sahara conditions directly lead to a drop in annually averaged precipitation over the Levant that was sufficiently strong as to eliminate the forest cover from this region. This is a critical issue as in the archaeological literature, it has often been assumed that this change in land cover was caused anthropogenically by forest clearance required for the onset of farming (Klein Goldewijk et al., 2017, Roberts et al., 2019, 2011).

> **Comment 2.**
> And although the authors use an advanced regional modelling setup to show variability between schemes, this is already a well established result.

The reviewer has missed the point that the purpose of this paper, as discussed following the earlier comment, is to apply the well established dynamical downscaling pipeline that we have developed for applications to the Mediterranean Basin and northern Africa, to understanding the expected impact of the cessation of Green Sahara conditions on the precipitation over the Levant. Although this pipeline has been previously exercised in our most recent paper for The Holocene, its application has been significantly extended to address the issue of interest to us, as carefully described above. This has required a significant extension of the suites of ensembles of climate integrations needed to establish the robustness of our conclusions.

Comment 3.

The albedo tests labelled 'A' are far as I can understand the main advance over the authors' paper in press, but these are set up in a complex manner which is not well justified.

Because the accuracy of the representation of the Green Sahara land surface remains a significant issue, we have added to our analysis of the WRF based downscaling pipeline an assessment of the impact that an uncertainty in the representation of Green Sahara conditions could have on the drop of precipitation over the Levant due to a change in the albedo over the Green Sahara. This is intended to provide an estimate of this uncertainty, and in our view, is extremely useful.

Comment 4.

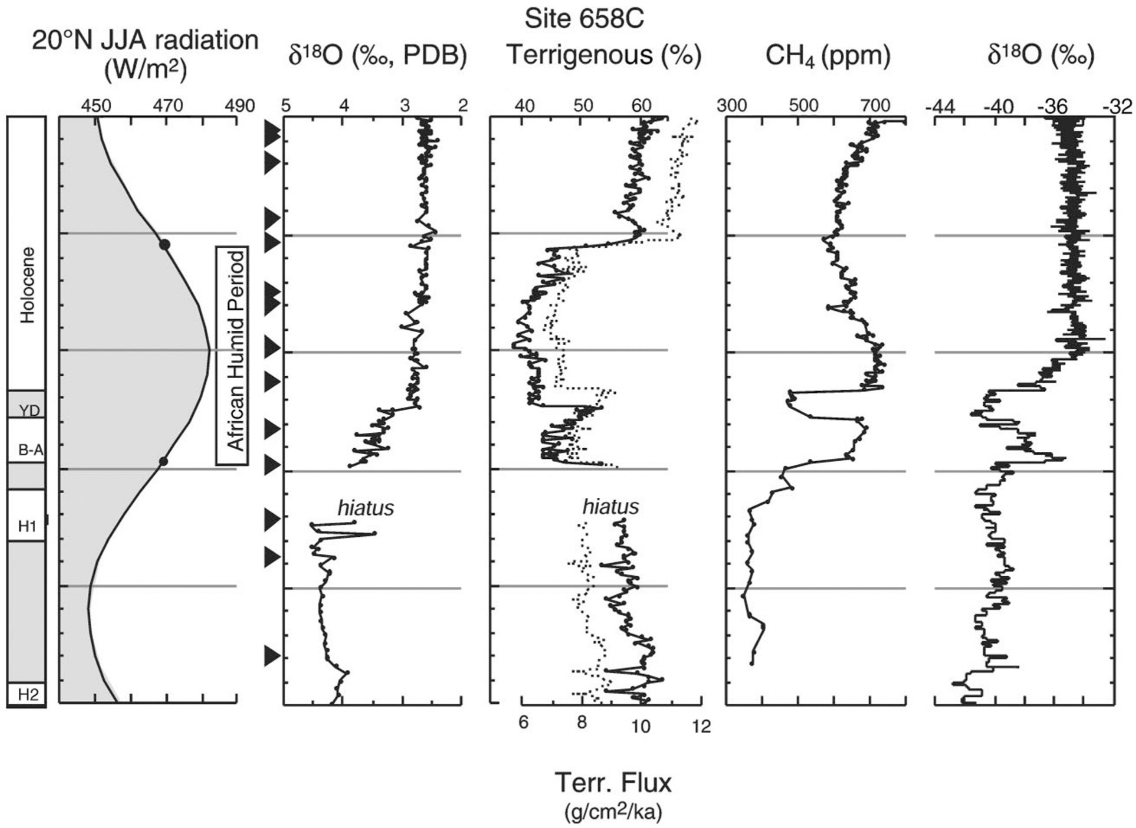
The manuscript is overly long with potentially redundant figures.

In the revision of this manuscript, the length will be significantly reduced by making available many of the figures and associated discussion as supporting material.

Comment 5.
I am sorry but I do not recommend publication.

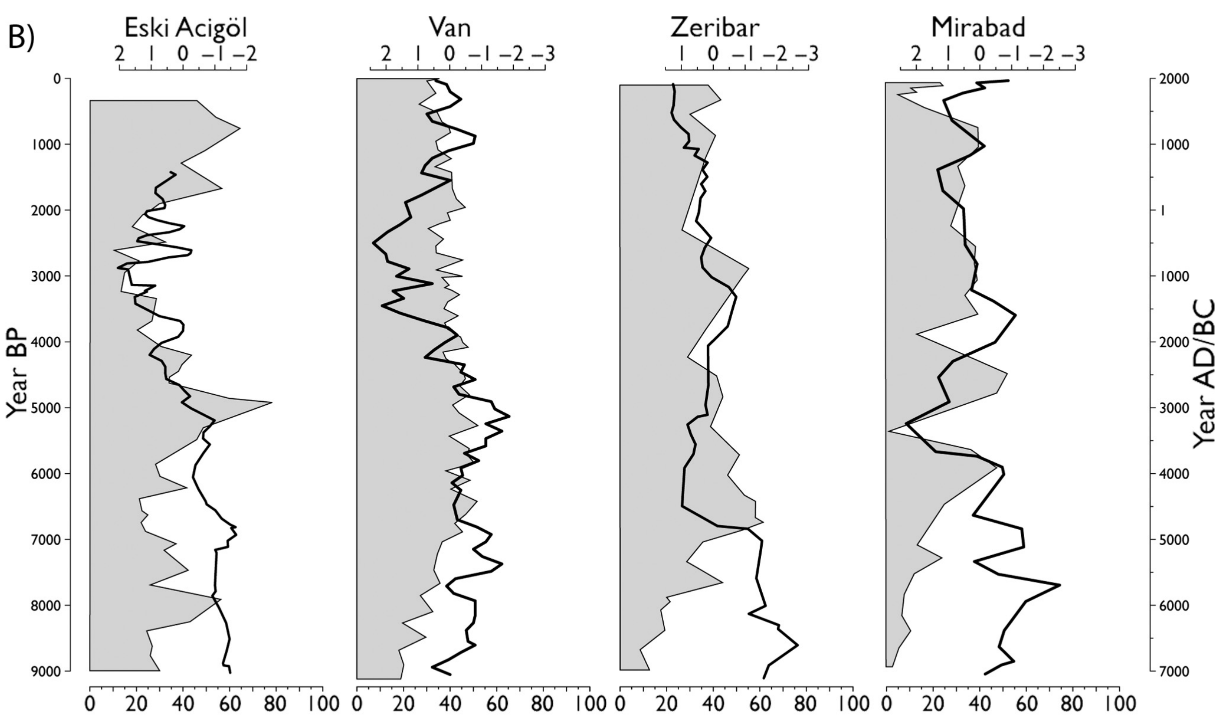
Following the reviewer's comment above, we have provided a very clear and concise description of the purpose of our manuscript, which will be included in the revision to be submitted. Since the purpose of this paper addresses a highly significant issue concerning the Climate of the Past, it is our hope that the reviewer's view of our contribution will change.

A)



20°N JJA radiation (W/m2)
450
470
490
Site 658C
δ18O (‰, PDB)
5
4
3
2
Terrigenous (%)
40
50
60
CH4 (ppm)
300
500
700
δ18O (‰)
-44
-40
-36
-32
Calendar age (ka BP)
0
5
10
15
20
25
Holocene
YD
B-A
H1
H2
African Humid Period
hiatus
hiatus
6
8
10
12
Terr. Flux (g/cm2/ka)

B)



Eski Acigöl
2 1 0 −1 −2
Van
2 1 0 −1 −2 −3
Zeribar
1 0 −1 −2 −3
Mirabad
2 1 0 −1 −2 −3
Year BP
0
1000
2000
3000
4000
5000
6000
7000
8000
9000
0 20 40 60 80 100
Year AD/BC
2000
1000
1
1000
2000
3000
4000
5000
6000
7000

Figure 1: Part A of this figure shows a comparison between boreal summer (JJA) average insolation at 20° N Berger and Loutre (1991) with benthic isotope record and terrigenous percentage and flux records from a marine sediment record excavated off the coast of West Africa (20°45'N 18°35'W, deMenocal et al. (2000)), and with atmospheric methane concentration preserved in ice bubbles and oxygen isotope composition of glacial ice in the GISP2 Greenland ice core (Blunier et al., 1995, Dansgaard et al., 1993). Time range of this comparison covers 25k calender year BP. These records show the onset of the most recent African Humid Period occurred approximately around 14.5ka BP, during which increase of summer season insolation over northern hemisphere coincides with increase in terrigenous proxy percentage, increase in atmospheric methane concentration, and decrease in O18 isotope concentration. These changes in climate proxies suggest an increase of terrestrial vegetation production over northern Africa, an expansion of boreal wetlands, and an increase in global temperature at this time. The terrigenous percentage and flux records has a significant decrease near 5.5ka BP, marking the termination time of the most recent Africa Humid Period, which also coincides with a decrease in northern hemisphere summer season insolation. Part A of this figure is a redraw of Figure 4 in deMenocal et al. (2000). Part B of this figure shows delta O18 and Arboreal pollen percentile from four lake sediments records around the Middle East region, covering a time period of 9k calender year BP. These records shows a transition of climate trend near 6ka BP, separating a period of increase in precipitation and tree cover around these lakes before it, and decline in precipitation and tree cover after it. The exact time of this transition differs between each lake record due to their geographical position. Part B of this figure is a redraw of Figure 6 in Roberts et al. (2011).

Main comments:

Comment 6.
It remains unclear by the end of section 1 what the purpose of this study? It seems that some simulations are already in press by the authors but they propose to try some other land surface schemes. However it is not clear what the aim of this is or what will be learnt from this.

As we mentioned in the answer to the general comment, the purpose of this study is to address the question as to whether the end of the most recent GS and the landscape change over the Levant that happened during the same time period should be seen as a chance correlation or whether it involves a causal connection. We would highlight this purpose in the revised section 1.

Comment 7.
Reading section 2 the main advance proposed here seems to be an incremental modification of albedo and FPAR (fraction of photosynthetically active radiation) in which regional increments are added depending on the vegetation type already prescribed. This therefore seems to add even more 'greenness' to a model with green Sahara already prescribed. It is unclear a) what is the justification for this, b) whether the resultant fields are physically plausible and c) what we can learn from this sensitivity test.

The incremental modification of albedo and FPAR is a sensitivity experiment, which addresses the question on how sensitive the influence of GS is with respect to the greenness of the prescribed land surface conditions. The most recent GS surface reconstruction, used in Chandan and Peltier (2020), has spatial detail that must be considered modest at most. This sensitivity test treats the GS land surface reconstruction used in Chandan and Peltier (2020) as inaccurate, and investigates how model results would respond to slightly different Green Sahara surface conditions. Our primary results in this regard are contained in the series of analysis we have performed by varying the physical parameterizations in the WRF component of the downscaling pipeline, which explicitly determines the variability of precipitation responses in the ancient Near East that a modern climate model is able to predict. By performing a sensitivity experiment we are able to verify the robustness of our conclusions by exploring its dependency on the influences from the prescribed GS surface.

Comment 8.
Given that the land-surface is prescribed it is unclear why using a different land surface scheme should meaningfully change the results. This is hinted at on line 260-262: "This confirms that the "control Mid-Holocene" setup is properly implemented in the L ensemble members, and suggests that choosing a different land surface scheme does not lead to a significant difference in MH climate anomaly in the MH-ref scenario without the GS surface." but seems to be a major weakness of the experimental setup.

The reviewer needs to note that the description on line 260-262 addresses results from the “control Mid-Holocene” setup, in which land surface conditions over northern Africa are kept in their Pre-Industrial state. Therefore the land surface schemes are not expected to have a significant difference when there is no change in landsurface. It should also be noted here that the performance of different land surface schemes is not guaranteed to be similar when simulating changes in land scape, which is the case when the barren surface of northern Africa is replaced by a prescribed green GS surface. What we observe is that different land surface schemes react differently to the same prescribed land scape changes, which is expected based on the fact that the representation of surface greening within the two land surface schemes are intrinsically different. The exact differences between the two land surface schemes in terms of which the different aspect of the land surface schemes regarding changes in land surface conditions are explicitly analyzed in section 3.3 of the manuscript.

Comment 9.
Any dependency of the results on the land surface scheme used is entirely expected given existing model differences. The differences are analysed but there is no connection made to the structure or parameters of the land surface scheme. Therefore, it is unclear what we can learn from this comparison.

We have attempted to provide an analysis that narrows the difference between land surface schemes to the effective change in surface albedo they produce. This connects to the structure of the land surface scheme in such a way that the Noah-LSM scheme takes in prescribed surface albedo, while the CLM scheme computes surface albedo from surface vegetation types, therefore bypasses the prescribed albedo changes. This is mentioned in line 405-411 and line 548-558 of our manuscript. In the revised manuscript these points will be further highlighted.

Comment 10.
The principle finding from the albedo modification run A seems to be that it can also influence the climate over Arabia. This is not justifiable as currently written (see comment 2 above). But this finding is also already evident from the non -A sensitivity runs that seem to be in press i.e. from the panels on the left column of figure 14. This aspect needs to be clarified.

The paper which precedes this paper is already published as Xie et al., 2025. It is true that we found both the A experiments and the non-A experiments show that the influence of GS reaches Arabia. We also found that the influence from the A experiments, in which the prescribed GS surface is greener, is stronger than the non-A experiments. The main result from comparing A experiments with non-A experiments is to demonstrate that GS influence over Arabia is sensitive to the greenness of the GS surface conditions, as mentioned in reply to previous comments. This paper is not focused on climate over Arabia. This paper is concerned with the impact of the re-desertification of northern Africa on precipitation over the ancient Near East. We show that when GS conditions are eliminated from the model, that precipitation over the ancient near East is dramatically reduced, at a level that can directly explain the transition of landscape from trees to grasses. As explained in our responses to the General comment of reviewer 2, in the revised manuscript this purpose of our paper will be highlighted in the revised introduction section.

Minor Comments:

Comment 11.
Lines 6-7: “having a prescribed GS in the land surface in WRF, but allowing the land surface scheme in WRF to compute influence from GS” This does not make sense to me I’m afraid. Please clarify what this means and why it is important for regional modelling.

What we intend to say here is that instead of prescribing every field of GS surface conditions, some fields are assigned to be computed by the land surface scheme in WRF. By giving some freedom in the land surface scheme of the model, the accuracy and uncertainty of the land surface scheme when resolving the influence of Green Sahara can be assessed.

Comment 12.
Lines 8-10 “Further analysis determined that under the same GS prescription as in a previous study, the land surface scheme used in this study produces 10 higher evaporation and a smaller change in albedo, jointly producing a lower 2m surface temperature”

At this point it is unclear what the previous study is and how it can be referenced like this in the abstract? Is it another study with this model or a different model? It is also unclear what the importance of this finding really is beyond other users of this model?

We will rephrase this section to “comparing with previous results” to avoid ambiguity. The previous study is using the same model but a different land surface scheme. Comparing model results from different land surface schemes shows the sensitivity of Green Sahara influence with respect to the land surface conditions prescribed or computed within a model, which is of broader interest.

Comment 13.
Lines 123-136: The description of the sensitivity simulation is unclear. Why are you changing albedo in a run that already has a GS and why are you altering FPAR? The resultant albedo and FPAR fields are not shown. Are they physically plausible?

As mentioned in our response to the general comment, since the quality of the prescribed Green Sahara surface only have modest accuracy, we need to consider the scenario that the actual Green Sahara surface is slightly different from the prescribed fields. Changing surface albedo and FPAR represents a scenario of increased vegetation influences, in which the surface of northern Africa is slightly greener than the prescribed fields. Using the methods described in Section 2, the resulting fields does not exceed values from a land surface category of higher vegetation cover, and is therefore plausible.

Comment 14.
Lines 202: “produced an increase in annual precipitation and 2m surface temperature (T2) with respect to the PI climate” This is not clear from figure 3?

We will add a quantitative analysis of these changes in the revised manuscript, so statistical significance of these changes will be addressed.

Comment 15.
Lines 65-72: it is unclear what you are getting at with ‘only one land surface scheme” it would be helpful to elaborate (even if only with model/scheme names).

line 571: “By comparing the absolute annual precipitation over various sub-regions in the Middle East, this study found that the shift away from GS over northern Africa will lead to increased aridity over the Middle East, to an extent that causes deforestation in it.” This is unclear.

In many places the bracketing of references is incorrect. Here is one example:

Line 60: “... and coupled atmosphere-ocean experiments Huo et al. (2021).”

Figure 1: the colour scale should be labelled with classes not numbers.

These issues will be addressed in the revision process. Since the revision will include re-organizing and rewriting the results section, many of these issues will be covered by the rewriting of their respective sections. Therefore we would not provide specific responses to each of these issues.

References

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