

We would like to thank the two anonymous reviewers for their detailed comments and the very useful suggestions provided. These have certainly improved the quality of our manuscript.

Below, we have answered the reviewers' points one by one. All the resulting changes can be found in the attached revised version of the manuscript in track-changes format. The new corresponding line numbers are also indicated in the answers.

In the Supplementary Material, we have now included additional figures (to address the reviewers' suggestions) and a discussion about the calendar-effect correction. Figures 1, 2, 7 and 13 have been slightly modified, according to the reviewers' requests. In addition, units in the colourbar of Figures 13c,d contained an error and have, therefore, been modified; erroneous labels in Figures 10d and 12 have also been corrected. Other minor corrections have been included in the manuscript (see track-changes).

Referee #1

As explained in the "Initial reply to reviewer's comments", we do not think it is appropriate to divide this manuscript into two separate studies for the reasons given therein. Below we address all the other specific comments.

- *Page 2182 Line 5 – make clear that this is one model (and state the model - HadCM3L) and is a boundary condition ensemble.*

Done.

[lines 4-8]

- *Line 10 – make clear that these are model results 'The modeled summer monsoon is also...'. Check throughout the paper that these sorts of statements all reflect that these are conclusions based primarily on climate model data.*

Done. Both for this sentence and throughout the paper (e.g. lines: 252, 279, 335, 373, 412, 472, 863).

- *Page 2183-4 In this bit of the introduction I think the problem could be better set up. The authors state that model-data mismatch is for generally cooler (annual?) surface air temperatures in model than proxy data. Then the text goes on to discuss changing connectivity and catchments around the Med, including the response of sediments to orbital forcing, but the authors do not explicitly link this to the climate proxy reconstructions and the model-data mismatch. It seems therefore to be missing a link/step in setting up the questions that the paper deals with. I suggest here there is a need for more detail, perhaps at a regional level, of the model-data mismatch and/or how the Med records reflect this in terms of climate (in addition to details about surface hydrological flows). Again, I think if the paper was separated into two then there would be more room to introduce these fully.*

This part of the introduction has now been rephrased and rearranged to better clarify the motivations behind this study and the choice of this specific time period (also following similar suggestions by Referee #2).

[lines 64-99]

- *Page 2186 Line 12 - In what mode is TRIFFID being run – equilibrium or dynamic? This could make a difference to how close regional systems are to equilibrium.*

TRIFFID is being run in equilibrium mode to ensure that the vegetation is as close to equilibrium as possible. This is now also specified in the text.

[lines 142-144]

- *Page 2188 The intermediate and deep ocean not being in equilibrium could influence what the authors are investigating if it has an impact on water mass circulation and therefore temperature distributions in the Atlantic – and trajectories towards equilibrium may be non-linear over timescales of hundreds of years. Best to at least test this by plotting the time series of quantities in question for these particular*

simulations e.g. North African summer precip, in addition to comparing to what other papers and other models have done. This is included to some extent in the supplementary information in terms of the global mean temperature time series but I would suggest going beyond global mean quantities to the particularly regions the authors are investigating.

We agree with the reviewer's remark. We have, therefore, added summer precipitation timeseries for the North African monsoon area (averaged over the Southern region) in the Supplementary Material (Figure S4).

- *Page 2190 Line 1 change 'wa' to 'was'*

Done.

- *Figure 2 – can't see (a) (b) on the actual figure – only in the caption. Please add these to the figure itself.*

Done.

- *Page 2190 Line 21 - Global SATs are not plotted, only hemispheric, so it is difficult to relate to the text.*

Global SAT is now also plotted in Figure 2, panel a (grey line).

- *Page 2191 Line 17-20 I find the way leads and lags are discussed with phase and antiphase a bit awkward. The authors might rephrase, e.g. 'In winter, SAT in the Northern Hemisphere is roughly in phase with insolation, with SATs leading insolation by 2kyr. Winter northern hemisphere SATs are roughly in anti-phase with precession, with SAT leading precession by ~9kyr.' Also further down at line 22 this anti-phase with lead of 1kyr is used again.*

To avoid confusion, this part of the description has been modified and all leads and lags are now discussed with respect to insolation only (also following suggestions from Referee #2), which should make this clearer and easier to follow.

[lines 273-294]

- *Page 2192 Lines 11-21 This statement about the model complexity ends by suggesting that understanding the leads and lags is challenging and gives the impression that it might be too challenging and they're not going to address what the mechanisms might be. Perhaps the authors could allude to later sections where they discuss this further, and/or if they were to separate the paper into two there would be more room for examining the mechanisms.*

We now mention more clearly in the text where some of these mechanisms are discussed in other sections, as suggested by the reviewer.

[lines 309-312]

- *Lines 23-25. Simplify this sentence. E.g. 'The DJF SAT anomalies between precession minimum and maximum (pMIN-pMAX) are generally negative (i.e. cooler; Fig. 3a), especially in north-...'*

Done.

[lines 316-317]

- *Page 2193 Line 9-13. The authors suggest that the location of their warmer anomaly near the Arctic is different from previous studies because of the different palaeogeography used and different sea ice distribution. Since sea-ice is not plotted can the authors be more specific about the details of the 'different' sea ice distribution or could they also plot the sea-ice distribution in the model. How exactly is this region palaeogeography different and therefore how might this result in altered sea-ice, and why might there be a difference in regional sensitivity of the sea-ice to orbital insolation?*

We have now plotted differences in sea ice distribution between pMIN and pMAX (Supplementary Material, Figure S2). These show that the biggest differences between the late Miocene and preindustrial control experiments are found in the subpolar North Atlantic, with more sea ice in the late Miocene simulations. The figure also shows the differences in palaeogeography between the late

Miocene and the present day, of which the main one in these region is the presence of the Barents/Kara Sea landmass in the late Miocene simulations. We can only speculate that these difference in the late Miocene are causing the shift in the location of the anomaly near the Arctic compared to the mentioned previous studies based on more recent time periods (Yin and Berger, 2012; Lunt et al., 2013; Otto-Bliesner et al., 2013). This appears to be a plausible explanation, but further analysis and additional sensitivity experiments would be necessary to find a definitive answer. This is, however, beyond the scope of this work. Nonetheless, we have now added a further comment in the main text about the specific differences in palaeogeography and sea ice distribution between the late Miocene and preindustrial.

[lines 331-334]

- *The full precessional cycle is not really discussed with respect to precipitation, only SAT, apart from much later with regard to North Africa only. As the paper stands I can understand not wanting to make it too long, but seems like a missed opportunity.*

We prefer to discuss local precipitation as this is the focus of this paper. Global precipitation responses could be the focus of future work, so the opportunity will not be missed.

- *Page 2198 Line 12 ‘off-phasing’ - is this a word?*

Now modified as ‘moderately out-of-phase temperatures’. [line 469]

- *Page 2201 Line 26. The following sentence seems misplaced as it is surrounded by discussion of obliquity: ‘In addition, there are other higher-amplitude precession cycles in the Messinian.’*

This sentence has now been moved. [lines 567-568]

- *Page 2203 Lines 17-19 ‘In addition, where good agreement is obtained between model and data, it would also be possible to estimate during which part of the precessional cycle the proxy reconstruction has been generated’. This is quite a strong statement given the uncertainty in climate model dynamic responses. It would be incredibly useful to explore this further with an example case study from one of the data records. If the authors were to split up the paper they could demonstrate the potential advances that could be made here.*

Assuming that the model realistically simulates orbital and seasonal variability, the proposed methodology can be applied locally, where high resolution and more precisely-dated data is available for this specific Messinian time period and is the focus of an ongoing regional study for the Mediterranean Sea. However, this could not easily be extended globally, as the data may come from a different late Miocene precession cycle. We have therefore modified this sentence to clarify the limits in its application.

[lines 615-621]

- *Figure 7 – It may be my problem but to me the schematic is not clear in what the difference between the orange and black lines are. In Figure 7e the model and orbital range have the same included properties in the lists, but actually is black without orbital max-min and orange includes it?*

We agree that the schematic was not fully clear. There was also a mistake in the definitions in panel (e). This has now been corrected and the definitions further clarified.

- *In the discussion around Figure 7 (and Figure 8) there is no mention of model structural uncertainty as far as I can see. Can the authors add this to the results and discussion, including what understanding can be gained about the level of variation between models from PMIP. PMIP3 has pre-Quaternary experiments, and while not Miocene, there will be useful insights about regions and climate fields that are subject to more/less inter-model variation.*

We do not think that it is appropriate to include structural or parametric uncertainties to Figure 7 and 8, as here we are considering one single instance of one single model. However, we have now included

a further comment in the text, clarifying that any remaining error must be due to structural or parametric uncertainties which could be addressed through multi-model inter-comparison initiatives such as PMIP.

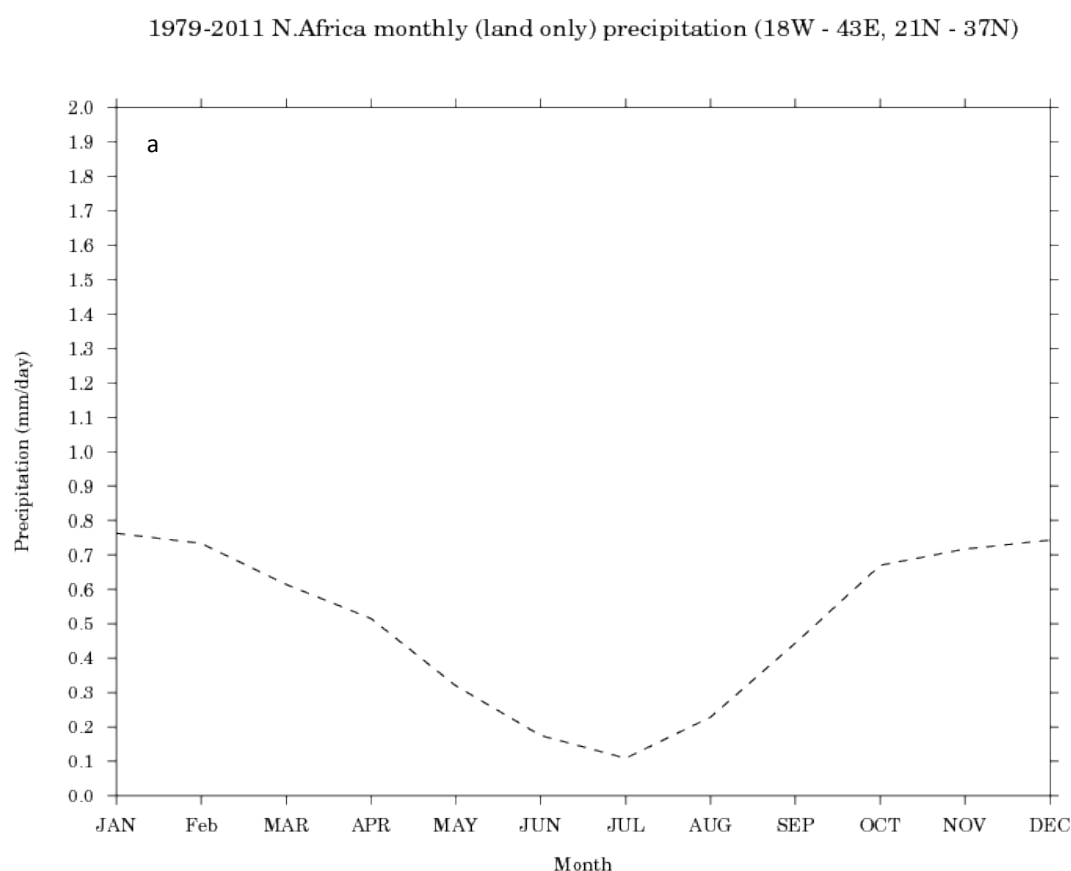
- *Figure 8 – In addition to the model-data points, it would also aid discussion to somewhere add in a figure from the purely proxy-data derived late Miocene minus present/pre-industrial temperature and precipitation.*

This comparison is already discussed in detail in Bradshaw et al. (2012) for mean annual temperature and precipitation with respect to their modern climate estimates (their Figures 7 and 11) and it is therefore not repeated in this study. We have however added a point of discussion on this matter in Section 4 and referenced Bradshaw et al. (2012).

[lines 850-854]

- *Page 2205 Lines 6-18 and Figure 10c. Can the authors say more about the double peak in precip in the northern region. What is the cause of this? As this bi-modal seasonal distribution is seen in the pre-industrial as well to some extent, can the authors briefly compare to observational/reanalysis data to get a sense of the robustness of the pattern and the sources of moisture for each seasonal peak?*

We have compared our results to present-day precipitation observations from the CMAP dataset (see Figure 1 below). The modelled and observed seasonal precipitation distribution is consistent for the North African monsoon region (Southern “box”), which gives us additional confidence in the representation of monsoon dynamics in the model. However, the seasonal distribution in the Northern drier region (Northern “box”) appears rather different in the model and is likely due to a model bias. Given that in such a dry region precipitation values are below 1 mm/day and that the double-peak feature is not consistent with present-day observations, we believe that further analysis would be beyond the scope and relevance of this study.



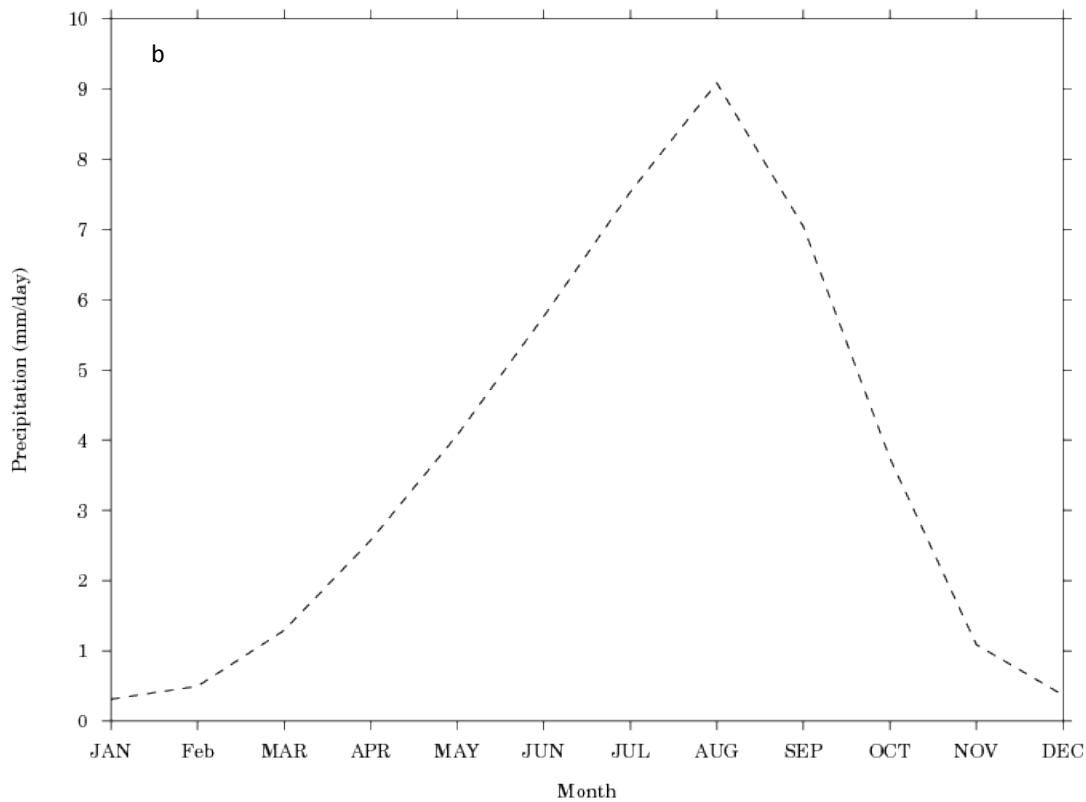


Figure 1. Precipitation distribution in the (a) Northern and (b) Southern regions of North Africa (as defined in the model) from the CMAP observational dataset.

- *Line 27 ‘as a result of stronger insolation and the negligible influence of monsoon cloud cover’. Since the only difference in the simulations is palaeogeography here, perhaps this should be rephrased. Do you mean lower levels of cloud cover produce stronger incoming insolation at the surface?*

The sentence refers to the absence of cloud cover due to the weak monsoon, which cools temperatures down in the Southern region even at times of maximum insolation when the monsoon is strong (e.g. during pMIN). To avoid confusion, we have removed the second part of the sentence, which was not necessary.

[lines 682]

- *Page 2206 section 3.4.1 Vegetation dynamics and interactions are only discussed with reference to North Africa. There may be more significant differences in sensitivity to orbital forcing with CO₂ in other regions where vegetation productivity is higher. Have the authors looked at the implications of this outside of N Africa?*

We have now plotted these differences globally and added them to the Supplementary Material (Figure S8), showing the differences in sensitivity to orbital forcing with CO₂ for all vegetation types in the model. The absolute difference plots (pMIN-pMAX) at both 280 and 400 ppm have also been included in the Supplementary Material (Figure S9 and S10). A comment has also been added in the text [lines 741-744].

Different feedbacks in other regions are, however, harder to disentangle than in the North African monsoon area, where vegetation changes can more directly be linked to shifts in the position of the ITCZ. Analysing these processes in detail in other regions globally is beyond the scope of this work, but this could be addressed in future studies. In fact, our results show that, for instance, it would be interesting to investigate vegetation dynamics with respect to changes in CO₂ and orbital forcing in the Amazon area, Indian monsoon region and more generally across the Asian continent, as well as North America and Greenland (see Figures S8, S9, S10).

- *Page 2208 Line 13-19 The authors suggest that perhaps another mechanism (lack of telenconnections) might be producing the underestimation of northward ITCZ movement. The authors should also discuss the possibility that the vegetation model itself and its coupling to the atmosphere might be the problem.*

Yes, we agree. This is now briefly discussed in the text and an additional reference has been included.
[lines 763-764]

- *Page 2209 Line 11 ‘...smaller than in the northern region’ change northern to southern*

Done.

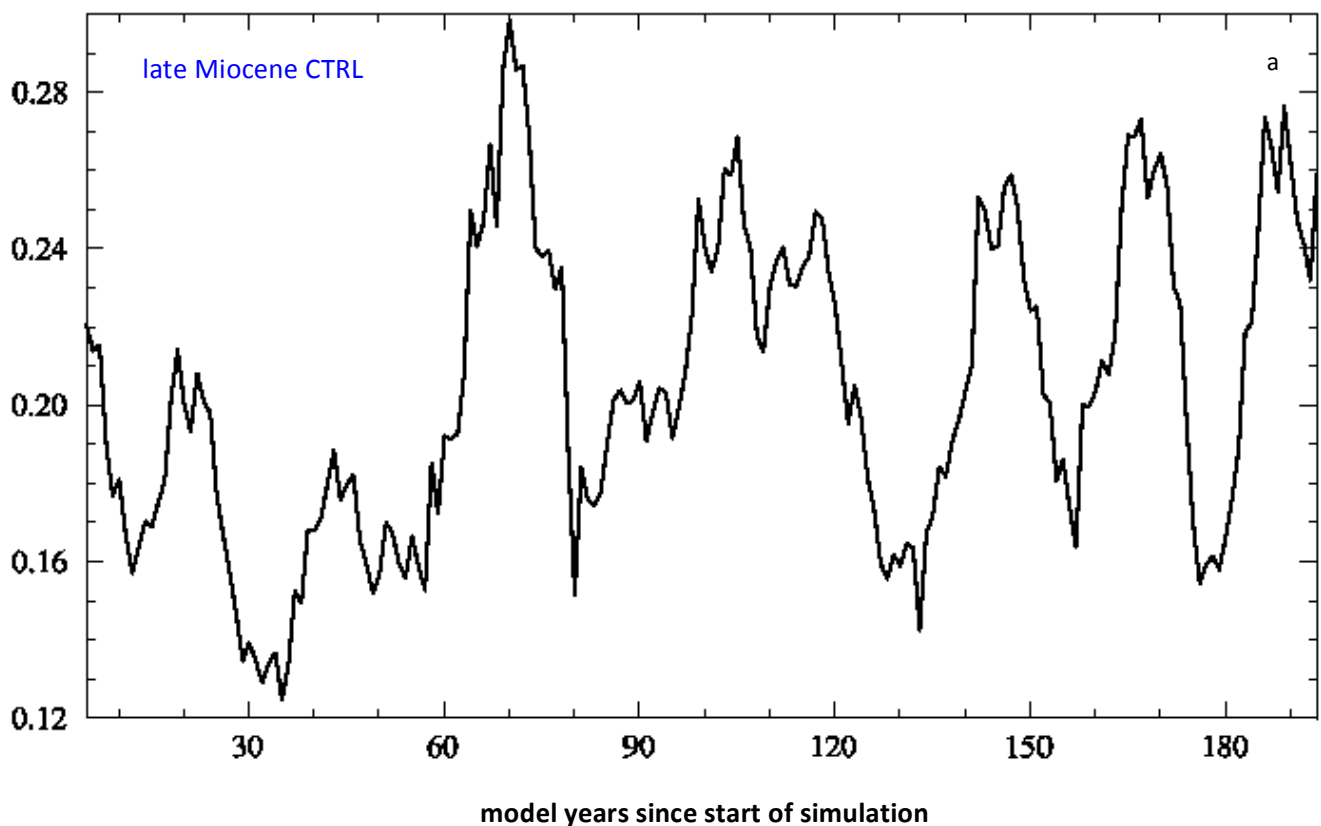
- *Page 2210 Line 3 change ‘tis’ to ‘this’*

Done.

- *Lines 8-13. Could some of this variation also be ‘noise’ due to interannual (or decadal) variability in the model, which might be influencing the 50-yr averages to a degree, particularly in the northern region where precip is low generally?*

Variations could be linked to centennial/interdecadal variability. Interannual variability is largely unresolved in the 50-year climate means. We have now plotted JJAS precipitation in the Northern “box” for both the late Miocene control and precession minimum experiments (see Figure 2 below). As seen in the timeseries plots, there is a strong decadal component which is likely going to influence the signal in the northern region of North Africa. A comment has now been added in the main text to point out the possible impact of interdecadal variability on precipitation in the Northern region.

[lines 784-786]



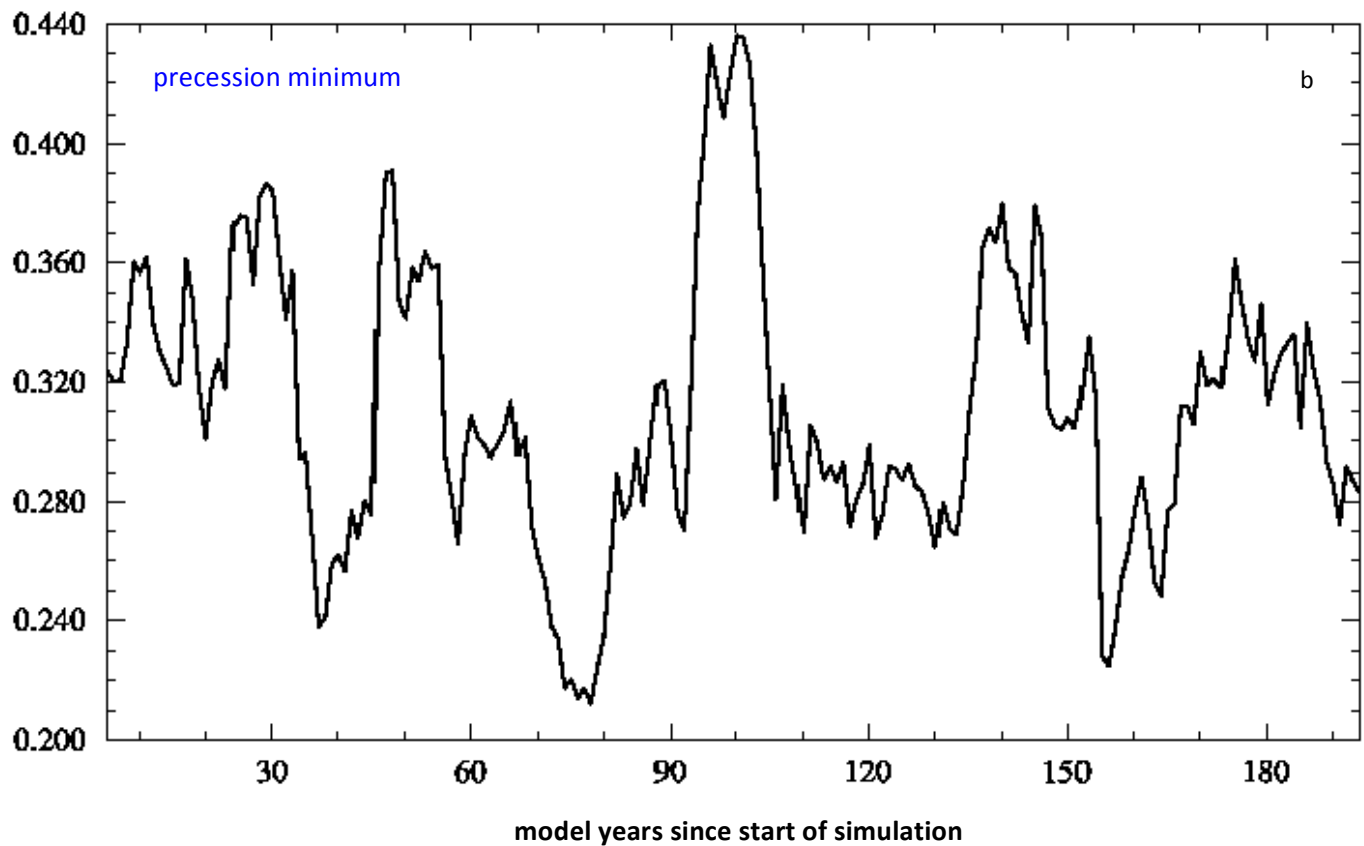


Figure 2. Timeseries of JJAS precipitation (mm/day) averaged over the Northern “box” (land only) for the (a) late Miocene control and (b) precession minimum experiments. Note the different scales in panels a and b.

- *Line 17 ‘The evolution of global mean annual SATs is not influenced by changes in insolation’. The start of the conclusion section here needs more detail and introduction. The ‘evolution’ - over a precessional cycle? – in the HadCM3L model...?*

This paragraph has now been expanded (initial and final part) and slightly rephrased.
[lines 816-820]

- *Line 21 ‘This response is part’ - change to ‘This response is in part’*
Done.
- *Page 2212 Line 24 ‘palaeoenvironmental syntheses Prescott et al. (2014).’ Put Prescott et al within brackets.*
Done.

Referee #2

Specific comments:

- 1) *Not enough discussion is given to the possible effects of obliquity. Over the course of the obliquity experiments, obliquity decreases from a max of ~23.9 degrees to a minimum of ~22.8 degrees, a change which should have effects on the climate system. While it is not clear how to explicitly separate the effects of precession and obliquity in these simulations, additional discussion should be made regarding the possible effects of obliquity. At the moment, obliquity is essentially ignored throughout much of the paper, and (except for brief moments) the entire variability in the orbitally-forced experiments is implicitly attributed to precession. Among other places, obliquity is potentially relevant for the leads and lags of temperature and precipitation response discussed in connection to Fig. 13. The paper does mention on p.2202 that this will be covered in a future study. However, since obliquity is almost never mentioned in this paper, the reader gets the implication that precession is the only orbital forcing that matters, which is an over-simplification.*

We agree with the reviewer and note that partially isolating the effect of obliquity was one of the considerations influencing our experimental design (see Figure 1, where simulations 1 and 22 have very similar precession and eccentricity values and maximum and minimum obliquity, respectively). The possible importance of the role of obliquity is now discussed in more detail throughout the paper, both globally and locally for the North African monsoon. An additional figure has also been added to the Supplementary Material (Figure S5).

[lines 6, 105-106, 295-299, 438-447, 464-469, 563-567, 628-630, 692-700, 825-826]

- 2) *Much of Section 3.1.2 “Global climate response to orbital forcing: precession extremes” offers too many details without enough synthesis. This results in a listing of observations (which the reader can see in the figures alone) that doesn’t offer much insight. The authors are encouraged to decide what details are most interesting/relevant to their argument, and leave the rest for readers to see in the figures for themselves. Much of the paper does not suffer from this, but it does occur in places.*

A few sentences have been removed. However, we generally prefer to keep this more detailed description of the figures as we think it draws the reader through the argument we are making. Mechanisms are also discussed throughout the section (e.g. lines 322-334, 337-341).

- 3) *The writing in the paper is occasionally sloppy, with references to the wrong figures and a few confusing sentences. Some examples are given in the “technical corrections” section below.*

This is addressed in the “technical comments” below.

- 4) *The abstract introduces the orbitally-forced simulations, but then discusses climate sensitivity to CO₂ without mentioning the additional CO₂ sensitivity simulations. This was confusing. It would be better to briefly mention those CO₂ simulations in the abstract, rather than waiting until later in the paper.*

The CO₂ sensitivity experiments are now briefly introduced in the abstract.

[line 10]

- 5) *The introduction should mention why the authors are studying the Miocene, rather than a different time period.*

Part of the introduction has now been slightly rephrased and rearranged (also following suggestions from Referee #1), in order to better clarify the reasons for studying the late Miocene.

[lines 94-99]

- 6) *The paper includes much comparison with results from Bradshaw et al. (2012). Do the authors account the corrected data from the corrigendum of that paper?*

We are using the corrected version of the database in this study. We have now added a note in the text about the corrigendum to that paper in order to avoid any confusion.

[lines 241-243]

- 7) *p.2185, line 23: “global circulation model” should be “general circulation model”.*

What we meant was “global general circulation model”. This has now been corrected.

- 8) *p.2187, line 20-21: “relatively high amplitude of the precessional cycle itself” means the same thing as “high eccentricity values”. The sentence is repetitive.*

The repetitive sentence has been removed.

- 9) *p.2189, line 11: The paper says “we only consider maximum and minimum values”. This is not true. The paper often considers seasonal averages or monthly differences (e.g. Fig. 13), which are influenced by the calendar effect.*

As stated in the text, that sentence only refers to the model-data comparison (Figure 8) and the analysis of the phase relationship between precession and surface air temperatures (Figure 6) where we do consider only maximum and minimum values. To clarify this further, we have included the reference to the two specific figures in the text.

[lines 212-213]

In addition, we have now tested our analysis applying a calendar-effect correction to our results (now discussed in the Supplementary Material, Figures S3 and S4). Differences are largely negligible for this study (also for Figure 13, as shown in Figure 3 at the end of this reply) and these are therefore not discussed in the main text.

- 10) *p.2191: It may be useful to state that changes in precession alone (ignoring eccentricity) have no effect on global, annual-mean insolation.*

An additional comment to point out this aspect has now been added in the text.

[lines 257-259]

- 11) *p.2191-2192: The paper discusses correlations with both insolation and with precession (here and in other places), making the paragraphs here overly complex. Additionally, discussion of correlations and anti-correlations with precession (as opposed to insolation) isn’t very useful. The timing of “maximum” and “minimum” precession is somewhat arbitrary, so positive vs. negative correlations are not insightful. Limiting the discussion to correlations with insolation would be more straightforward and satisfying. (This may be considered a personal opinion. If you have reason to believe that such discussion is useful, you can keep it. However, because of the large amount of numbers in these paragraphs, this data may be better summarized in a table.)*

Now all leads and lags are discussed with respect to precession and the descriptions have been shortened [lines 273-294]. So now we think that adding a table is no longer necessary.

- 12) *p.2203, lines 17-19: The sentence which starts “In addition, where good agreement is...” is arguable. Models and proxies may agree for the wrong reasons.*

We agree. We have changed the sentence to clarify that this could only be valid locally (e.g. in the Mediterranean Sea) where high-resolution data is available for this specific time period.

[lines 615-621]

13) p.2210, line 17 says “The evolution of global mean annual SATs is not influenced by changes in insolation”. You show in Fig. 3e that this is not true.

This has now been slightly rephrased and the introduction to this section has also been briefly expanded (following suggestions from Referee #1).

[lines 818-820]

14) Fig. 1: Why is the obliquity scale given in radians instead of degrees. I think that most readers would find degrees easier to conceptualize.

Values in degrees have been added to the figure and also mentioned in the text [lines 296-297].

15) Fig. 2: The differences in insolation scales for panel (a) versus the other panels is so large that it should be explicitly pointed out in the caption. Also, the fact that panels show the same seasons for NH and SH (e.g. DJF for NH and JJA for SH) rather than the same months (e.g. DJF for both) is a little confusing.

We have now added a note in the caption about the different scales used in the different panels and especially the small range in panel (a). The panels show different months in order to have the same seasons grouped together, which are opposite in the two hemispheres. This is now further clarified also in the caption.

16) Fig. 4: Labels on panels a and b say “JJA” but the caption says “JJAS”. Which is it?

It is JJA. This has now been corrected in the caption.

17) Fig. 8: Some of the colors chosen for this figure may be difficult for red/green colorblind people to distinguish. You don’t need to change it, but I thought I would point it out.

We appreciate the comment and we have taken extra care in testing other figures for colorblind readers. However, in this case we would like to keep it consistent with the already published ones from Bradshaw et al. (2012) so we have not changed it. We have, therefore, added an additional figure using different colours in the Supplementary Material (Figure S6).

18) Fig. 13: The numbers on your color bars do not correspond with the boundaries between colors. This makes it difficult to determine exact values from your figures. Please fix this.

Done.

19) Fig. S1: Optionally, you could overlay a few words on this figure pointing out the major geographic changes from modern (i.e. the differences you point out in the text).

These are now indicated in the figure.

20) Fig. S3: Why is the contour interval different between positive and negative?

The downward motion is stronger than the upward motion over the region of interest. Therefore, we use different contour intervals in order to better represent the anomalies for both the positive and negative values, as specified in the caption. A further explanation has, however, been added to the caption.

Technical corrections:

1) Some figures or table references in the text specify the wrong number.

This has been checked and corrected throughout the manuscript.

2) *The use of parentheses around citations is inconsistent and sometimes distracting.*

This has been checked and corrected throughout the manuscript.

3) *Some sentences have errors or are confusingly written.*

(a) p.2192, line 14: *Is “result in” the right phrase here?*

“result in” is correct, but a comma was misplaced. The whole sentence has now been rephrased for clarity [line 302]

(b) p.2196, line 15: *“Patterns are less pronounced...:” in some regions, but not in others.* This sentence has now been rephrased [lines 410-411]

(c) p.2200, line 12: *“the the”*

Corrected.

(d) p.2200, line 19: *“...where 9 are 8 the gridcells...” is confusing.*

There was a typo, now corrected to “...where 9 are the 8 gridcells surrounding the data...”.

(e) p.2204, lines 19-26: *The sentence starting “In the northern region...” is badly written.*

Rephrased [lines 648-554]

(f) p.2209, line 11: *“northern region” should be “southern region”.*

Corrected.

(g) p.2209, lines 19-22: *This sentence is confusingly written.*

Rephrased. [lines 790-793]

(h) p.2210, lines 12-13: *the phrase “...during precession minimum, throughout their entire simulated time slice” seems self-contradictory.*

What we mean is: for all the precession minima throughout that time slice. We have rephrased this sentence for clarity [lines 809-812].

(i) p.2210, line 18: *“The” is capitalized.*

Corrected.

(j) p.2212, line 14: *“...a full the precession cycle” has an extra “the”.*

Corrected.

(k) Fig. 2 caption: *“througout” should be “throughout”.*

Corrected.

(l) Fig. 6 caption: *“...maximum/minimum SAT...” should be ...maximum/minimum precession parameter...” (if I understand things correctly).*

Maximum/minimum SAT is correct. As discussed in the text and in the caption, the figure is showing in which one of the 22 simulations the maximum/minimum SAT values are reached for each model grid cell.

(m) Fig. 10 caption: *“Southern “box”” should be “Northern and Southern “boxes””.*

Corrected.

(n) Fig. 12 caption: *Be consistent about whether you put figure letters before or after the relevant descriptive text.*

These are now consistent.

(o) Fig. 13 caption, line 2: *“annual” should be capitalized.*

Corrected.

(p) Fig. 13 caption, lines 6-7: *The sentence which starts “Note that panel (c) is...” is confusingly written.*

The units had to be changed in this figure because of a mistake. The caption has been corrected accordingly and also slightly rephrased for clarity.

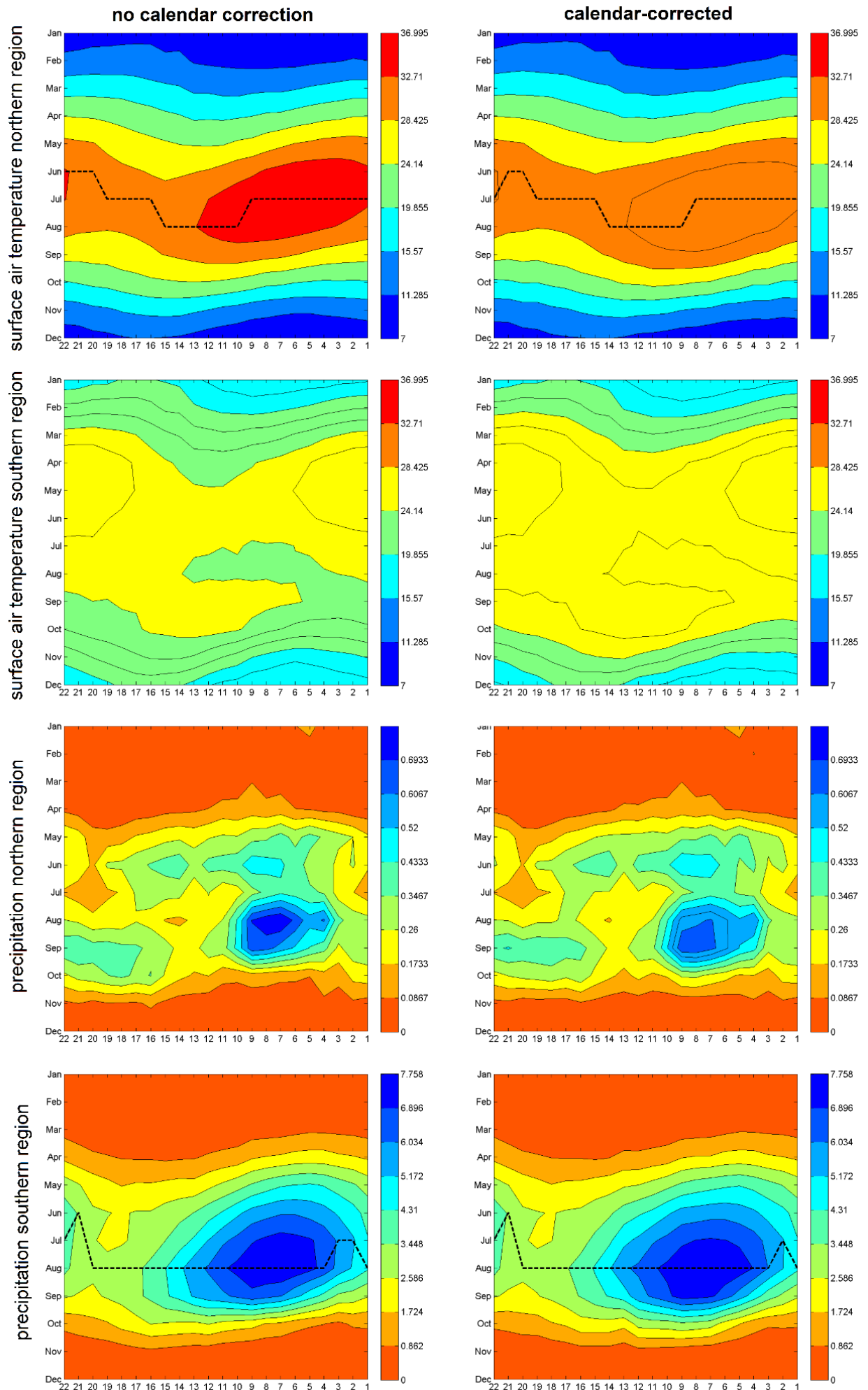


Figure 3. Panels from Figure 13 (main manuscript) before (left) and after (right) the calendar correction was applied.