

Interactive comment on “Orbital control on late Miocene climate and the North African monsoon: insight from an ensemble of sub-precessional simulations” by A. Marzocchi et al.

Anonymous Referee #1

Received and published: 14 July 2015

The authors present results from an ensemble of snap-shot HadCM3L model experiments covering a precessional cycle in orbital configuration under late Miocene palaeogeography. In addition they have performed max/min precession forcing under two different CO₂ levels to account for the uncertainty in knowledge of late Miocene atmospheric GHGs. They examine the impact and phase response of surface air temperatures and precipitation to precession. This is done at a global scale and then with a focus on North Africa. The authors demonstrate the important results that (1) climatic responses are not necessarily in phase with precession, and the complex spatiotemporal patterns need to be considered in terms of correlations between records and with orbital configuration that are often done for pre-Quaternary data, and (2) given

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the uncertainty in chronologies and synthesis of Miocene records for model-data comparisons, it is necessary to incorporate orbital variability into model experiments correspondingly. The paper is clearly within the scope of *Climate of the Past*. It presents novel and detailed work worthy of publication following revisions. The experimental design is appropriate (given computational constraints) and the results are sufficient to support the conclusions made. The manuscript is suitable for publication as it is, however, I do think the authors should consider separating the global analysis and data-model comparisons from the North Africa sections to form two papers. In doing so they could include more detail for each e.g. more detail about the influence of the seasonal cycle of sea-ice and vegetation feedbacks for global patterns, or more on model-data comparison improvements and how the model could potentially be used to inform data interpretation. More detailed comments are below:

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

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.

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.

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.

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.

Page 2190 Line 1 change ‘wa’ to ‘was’

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

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

Page 2191 Line 17-20 I find the way leads and lags are discussed with phase and anti-phase 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.

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

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further, and/or if they were to separate the paper into two there would be more room for examining the mechanisms.

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-...’

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?

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.

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

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.’

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.

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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?

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.

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.

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?

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?

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?

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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.

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

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

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?

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...?

Line 21 ‘This response is part’ - change to ‘This response is in part’

Page 2212 Line 24 ‘palaeoenvironmental syntheses Prescott et al. (2014).’ Put Prescott et al within brackets.

Interactive comment on Clim. Past Discuss., 11, 2181, 2015.

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