

Interactive comment on “The East Asian winter monsoon variability in response to precession and inter-hemispheric heat balance” by M. Yamamoto et al.

Anonymous Referee #2

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The paper "The East Asian winter monsoon variability in response to precession and inter-hemispheric heat balance" by Yamamoto et al. uses multiple temperature proxies for extracting an index of East Asian winter monsoon, which is used to comment on leads/lags of winter monsoon WRT insolation changes. The paper deals with a lot of different proxies, sites and hypotheses launched in the literature to explain how the East Asian winter monsoon responds to precession. I recommend publication after moderate to major revisions, my two main concerns being (1) the lack of description of the uncertainties associated with the use of different SST time series (mainly the age model and the proxy sensitivity), and (2) a lack of clear synthesis and analysis of the literature that deal with different kinds of monsoon the authors deal with.

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One of my concern deals with the use of different proxies of SST. I find curious that the authors describe the likely season and water depth of their own temperature proxies (TEX86 and Uk'37), but fail to do so with the Mg/Ca records. The authors should better describe what season and water depth the temperature proxies are likely representing, in particular those ones used in the discussion (such as the Mg/Ca records of de Garidel-Thoron and Lea). How would the discussion change if, e.g., Mg/Ca records were also skewed toward one specific season? The authors should clearly state what is the likely meaning of their stack. Is it a pure 'winter SST anomaly' if, e.g., Mg/Ca turns out to be representative of the mean-annual SST? Or is it a seasonal SST contrast if Mg/Ca is skewed toward summer SST? I don't think the interpretation of the stacked delta SST would dramatically change, but a little extra discussion is warranted here in terms of the actual meaning of the stacked delta SST records and in terms of uncertainties of the age model. Also, leads and lags at the millennial to sub-millennial timescales might affect the timing. You might want to refer to Wang et al., in press in *Paleoceanography* ('Northern and southern hemisphere controls on seasonal sea surface temperatures in the Indian Ocean during the last deglaciation') to see how different proxies can be affected by seasonality to better comment on the significance of the leads and lags you record and interpret later.

Another concern I have is the discussion on the timing of the winter/ summer indian/asian monsoons in chapter 5.2. (and in particular page 4244, lines 2-13). The discussion here starts to be confusing to the reader as it deals with numerous monsoon systems, hypothesis, regions, proxies, etc. I suggest the authors to rewrite that part, and discuss the comparison of their records with other records from other regions (Indian Ocean, Japan, etc.) in separate subchapter. It is unclear for example, as the discussion stands, why is it important to consider other hypothesis from other regions which are a priori not supposed to be perfectly synchronized as long as they belong to contrasted climatic systems with different forcings. Here it will perhaps be useful to rapidly remind the reader the state of the art of the different hypothesis already listed in the introduction to clarify all that. It should also be useful to consider the paper by

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Laepple and Lohmann, 2009, *Paleoceanography*, to have a look at how the present-day seasonal cycle can shed light on the regional sensitivity to orbital forcing (see their figure 3 in particular that shows that the sites discussed in the manuscript have very different sensitivities to the annual cycle, and hence probably to the insolation forcing).

Minor comments:

Abstract: the last sentence sounds pretty complex, please reformulate

Page 4231, line 1: please provide a key reference.

Page 4231, lines 15-20: can you provide more discussion and background on the inconsistencies between the different hypotheses listed here?

Age model:

Please comment more on the age model, in particular the radiocarbon. Please rapidly justify the use of constant reservoir age, in particular in the SCS which is semi-enclosed during the LGM. Why not updating the use of MARINE04 (INTCAL13 was just launched that week)?

Also, I think Toba is older than 71 ka as stated in the manuscript, probably dated between 74 and 75 ka. As cross-spectral analysis are ultimately used to discuss leads/lags of temperature WRT insolation, I am wondering whether using a more accurate date for the Toba eruption impacts the discussion and conclusion on the phasing of the winter monsoon with the precessional cycle. I am also wondering how shifting the Toba tie point would impact the age model as a whole, as I feel other tie points might be affected by this revision. It seems that assigning the winter monsoon to mid-May insolation can change if the age model needs to be revised by more than 3ka, this should also be discussed in the text.

Page 4241 lines 7-15: I find this part of the discussion a little awkward. My overall feeling is that there is not much of a doubt that temperatures, ice volume, orbital forcing and CO₂ are coupled in paleoclimatic records that deal with climate changes over time

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periods longer than one glacial-interglacial cycle. It is unclear to me why the authors point these correspondences between regional temperatures and ice volume / CO₂, in particular if the following paragraph describes leads and lags between all those records. I suggest to reduce that paragraph as it doesn't help the reader to get to the main point: differences in the timing of regional temperatures and other reference records.

Page 4242, line 12: 'lower', did you mean 'higher'?

I am not a native English speaker but found numerous English mistakes. One native English speaker should have a read over the manuscript.

Interactive comment on *Clim. Past Discuss.*, 9, 4229, 2013.

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