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

**M. Yamamoto et al.**

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We thank reviewer#2 for his/her review, which has lead us to improve our manuscript. Below we provide our response to queries and comments raised.

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



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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. Reply. All right. We revised the manuscript according to the comments and suggestions.

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. Reply: Oxygen isotope and Mg/Ca studies for G. ruber in a core from the northern WPWP region indicate that the calcification depth of G. ruber is ~60 m in the surface mixed layer (Sagawa et al., 2012). The temperature in the surface mixed layer does not show little variation on seasonal and interannual timescales in the WPWP (NOAA, 1998). It is thus likely that the G. ruber Mg/Ca in ODP 806 and MD97-2140 cores reflected the mean annual SSTs. In core MD97-2151, TEX86H and UK37âĂš reflected subsurface and surface mean annual temperatures, respectively, as discussed in the previous section. Thus, the seasonal bias of paleotemperature proxy does not affect the variation of ΔSST. We added one paragraph to explain this in section 5.2. (Response of SCS to orbital forcing). We also added the following explanations. The stacked ΔSST is designed to minimize the spatial heterogeneity and the errors that arise from independent age-depth models and proxies.

We tuned the records from three different cores in the same way to the LR04 stacked record.

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. Reply: As we explained above, the seasonality is not a major factor of leads and lags of different proxy records in the study region. Our South China Sea core record showed millennial-scale variability, but the central WPWP cores do not have enough time resolution to show millennial-scale variability. The approach of $\Delta$SST is not useful to evaluate the effect of millennial-scale changes on the phase of variation at the precession band.

Another concern I have is the discussion on the timing of the winter/ summer indian/asian monsoons in section 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. Reply: According to this suggestion, section 5.2 was separated to three sections 5.2-5.4. The discussion on the phase was thoroughly reorganized.

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 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). Reply: Figure 3 of Laepple and Lohmann (2009) is interesting. The study site is located at the boundary between summer and winter sensitive regions. We, however, do not fully understand how the distribution of sensitivity affects monsoon activity. We thus gave up to include the concept in our manuscript.

Minor comments: Abstract: the last sentence sounds pretty complex, please reformulate Reply: The last half of abstract was revised.

Page 4231, line 1: please provide a key reference. Reply: All right. We added "Wang et al. (2003).

Page 4231, lines 15-20: can you provide more discussion and background on the inconsistencies between the different hypotheses listed here? Reply: The introduction on past proxy studies on the monsoon variability was expanded and reorganized in section 1. "Long-term changes in the Asian monsoon are an important topic of paleoclimatology. The Asian monsoon responds to insolation changes at low latitudes on theoretical ground, which is regulated by precession, and hence it has been assumed to respond to precessional forcing (Kutzbach, 1981). According to this hypothesis, the summer monsoon is maximized when the northern hemisphere summer insolation is maximal on precessional cycle. However, the periodicity and phase of the monsoon variability in proxy records were inconsistent. Chinese speleothem records have suggested that the variation in the East Asian summer monsoon responded to precessional forcing and was maximal at the July to August perihelion (Wang et al., 2001; 2008; Yuan et al., 2004). Clemens and Prell (2003) argued that the Indian summer monsoon variability has obliquity and precession cycles and was the strongest at the November perihelion on precession cycles. In contrast, based on marine records, Huang et al. (1997a; b) stressed that monsoon intensity is regulated by glacial conditions. In glacials, the summer monsoon was weaker, and the winter monsoon was stronger. A paleoproductivity record from the Sulu Sea suggested that the East Asian winter monsoon was stronger in glacial periods (de Garidel Thoron et al., 2001). Chinese loess records have suggested that the East Asian winter monsoon was stronger in glacials than in interglacials, and the variability has strong eccentricity cycles (Ding et al., 1995; Xiao et al., 1995). Different proxies and archives provided different conclusions on the periodicity and the phase of the Asian monsoon variability."

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. Reply: Larger carbon reservoir age has long been suggested in the South China Sea (e.g., Wang et al., 1999, Marine Geology, 156, 245–284). We also suggested that there were possibly large fluctuation in marine carbon reservoir age in the northern South China Sea in MIS-2 and MIS-3, based on the correlation of oxygen isotopes between Chinese stalagmites and the northern South China Sea cores (Lin et al., 2013, Journal of Asian Earth Science, 69, 93-101). It is, however, not easy to turn the knowledge to improve the age-depth model in a southern South China Sea core. We thus use the constant local reservoir effect of $\Delta R = 0$. The deviation of carbon reservoir age within several thousand years in MIS-2 and MIS-3, however, will not change significantly the entire variation of $\Delta$SST during the last 150 ky. This explanation was added in section 3.1.

Why not updating the use of MARINE04 (INTCAL13 was just launched that week)? Reply: All right. We updated the age depth model. According to the change of age-depth model, Figs 3-8 were revised.

Also, I think Toba is older than 71 ka as stated in the manuscript, probably dated between 74 ad 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. Reply: We realize that the estimated age of Toba tephra ranges from 71 to 74 ka. We omitted the age control based on Toba tuff in the age-depth model. We also added the following sentence in section 3.1. "The Young Toba Tuff appeared at 1556 cm in core MD97-2151, and its age was assigned 75.1 ka in this age-depth model, which is consistent with recent results of its age (75.0 ka; Mark et al., 2013)." The age-depth model of the original and revised manuscript was basically created by benthic oxygen isotopes in before MIS-3. The removal of Toba tuff as an age control did not affect the periodicity and phase of variation.

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 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 ears 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. Reply: The paragraph was shortened and the description on South China Sea SST was removed. "In the central WPWP region (ODP Site 806 in the Ontong Java Plateau [Lea et al., 2000] and MD97-2140 at the northern margin of New Guinea Island [de Garidel-Thoron et al., 2005]), Globigerinoides ruber Mg/Ca-derived temperatures showed a pattern that was broadly similar to that of the atmospheric CO2 concentration recorded in Antarctic ice cores (Fig. 6B; Kawamura et al., 2007). These correspondences suggest that the surface temperatures in the central WPWP regions and changes in atmospheric CO2 concentration responded principally to orbital forcing."

Page 4242, line 12: 'lower', did you mean 'higher'? Reply: Thank you. We corrected.

I am not a native English speaker but found numerous English mistakes. One native

English speaker should have a read over the manuscript. Reply: We will ask grammatical check by the CP editorial service.

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



**CPD**

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Interactive Comment