

# ***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 #1**

Received and published: 18 September 2013

Yamamoto et al. have written an interesting manuscript dealing with the reconstruction of the East Asian winter monsoon. In essence, they propose to make use of the difference in sea surface temperature (sst) response to winter monsoon cooling between a site in the South China Sea (SCS; strongly affected by winter monsoon) and a site in the Western Pacific Warm Pool (WPWP; hardly affected by winter monsoon cooling). The author combines several different paleo-sst proxies to make sure that the observed changes in sst are true (Uk37, TEX86, Mg/Ca). The idea to contrast the large changes in winter-sst in the SCS against the minimal changes in the WPWP to obtain a measure for the strength of the winter monsoon is clever and the topic is highly suitable for Climate of the Past. Below I will follow the review template provided by CP:

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1. Does the paper address relevant scientific questions within the scope of CP? Yes (see above)

2. Does the paper present novel concepts, ideas, tools, or data? Yes, although the concept of contrasting the climate signal of different regions is not new, the presented study is cleverly designed and I believe this is the first attempt at this kind of reconstruction made in at least this area. A somewhat similar study (for the last 26ky) was published by Huang et al. 2011 (cited in the manuscript). but this study goes back much longer.

3. Are substantial conclusions reached? No, the conclusions are minimal and too general, stating explanations such as "a physical mechanism of inter-hemispheric heat balance". This claim must be substantiated and the mechanism properly explained in the discussion.

4. Are the scientific methods and assumptions valid and clearly outlined? Yes. The methods are properly described. However, there is a need for a more thorough explanation why the measured sst is only affected by the winter monsoon and not by any other factor caused by the extremely large changes in basin geometry and geographical setting that characterize the SCS on glacial-interglacial time scales.

5. Are the results sufficient to support the interpretations and conclusions? The results support the interpretations, however, it is not clear why only winter monsoon strength would affect sst. The core (MD97-2151) is situated directly at what appears to be the boundary between the weak cyclonic gyre that develops in the northern SCS, and the anti-cyclonic gyre in South (Separated by eastward summer jet) during the summer. How is the circulation pattern influenced by the dramatic change in geography caused by sea level changes? And how are these changes reflected in the proxy records at this site? These questions (and possible complications) are overlooked and need to be addressed.

6. Is the description of experiments and calculations sufficiently complete and precise

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to allow their reproduction by fellow scientists (traceability of results)? The phase relationships between the different monsoon proxies are difficult to follow, and on several occasions the authors refer to the Ice Volume Maxima in a way that gives the impression that ice volume is controlled by precession. I believe obliquity and eccentricity play a considerably larger role in controlling ice volume.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

8. Does the title clearly reflect the contents of the paper? No. The expression "Inter-hemispheric heat balance" occurs in the title, the abstract, twice in the discussion, and once in the conclusion without any explanation of what this would mean in the physical world. If this is central to the study, as indicated by the title, then this concept must be explained properly, and its relevance to the winter monsoon must be explained in detail.

9. Does the abstract provide a concise and complete summary? Yes

10. Is the overall presentation well structured and clear? Yes, until 5.2 when it becomes difficult to follow the arguments regarding the phasing of different components of the climate system.

11. Is the language fluent and precise? Generally yes.

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes, as far as I can judge.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Under section 5.2, the part dealing with the phase relationships needs to be expanded and better explained.

14. Are the number and quality of references appropriate? The general oceanographic description appears to largely be based on Wyrki (1961). There is a large literature summarizing the recent understanding of the SCS that needs to be considered: Hu, J.,

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Kawamura, H., Hong, H. and Qi, Y., 2000. A review on the currents in the South China Sea: Seasonal circulation, South China Sea Warm Current. *Journal of Oceanography*, 56: 607-624. Wang, P. and Yu, Q. (Editors), 2009. *The South China Sea. Developments in Paleoenvironmental Research*, 13. Springer. And especially the effect of local upwelling near the studied site: Xie, S.-P., Ilmberger, J., Xie, Q., Wang, D. and Liu, W.T., 2003. Summer upwelling in the South China Sea and its role in regional climate variations. *Journal of Geophysical Research*, 108(C8).

Kutzbach is sometimes misspelled Kutsbach or Kutsbach

15. Is the amount and quality of supplementary material appropriate? I couldn't find any supplementary material, but that might be my own fault.

Additional, specific comments: Page 4230, line 24: inter-hemispheric phenomena is very wide and sweeping, specify or remove. Page 4233, line 12-13: The sentence is a bit awkward and I think it actually means that the site has been located offshore from Vietnam for 150,000 years. Obviously what is meant is that the records go back 150,000 y. Page 4235, line 14: Awkward English: He was the carrier gas at 30 cm/s. Page 4236, line 1: SSC? Should be SCS? Section 5.2: For the sites in the WPWP only coordinates are given. It may help to plot them on the map in figure 1, or to describe their position in the text. Simply giving coordinates forces the reader to look up those positions by themselves. The SST record of MD97-2140 was re-tuned to the age model of Martinson et al. (1987). Is this really the best age model available for the WPWP? Page 4242, line 10-14: Other possible factors influencing the SST of the SCS site must be addressed, such as geographic/bathymetric changes due to sea level, upwelling changes due to changed deep circulation (there are a couple of recent studies addressing deep water changes in the SCS on glacial time scales). Page 4242, line 19: Huguang is a maar, which is a kind of lake, thus no need to call it Huguang Maar Lake (although Wang et al. does it...) Page 4243, line 6: Here ice volume is stated to be at a maximum at the March perihelion, but ice volume doesn't change on the 23k precession cycle, does it?

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On page 4244, the authors list a number of records from East Asia that show other phase relationships. If the winter monsoon is offset at these locations, how can the authors then be certain that their record is generally valid?

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

**CPD**

9, C2050–C2054, 2013

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