

## ***Interactive comment on “East Asian Monsoon and paleoclimatic data analysis: a vegetation point of view” by J. Guiot et al.***

**J. Guiot et al.**

Received and published: 23 May 2008

Response to reviewers #1 and #2

First we thank both reviewers for helpful comments. The main changes are a better introduction giving clearly the objectives of the paper and a new discussion section trying to go into more details in the interpretation of the results.

On behalf of the authors

J. Guiot

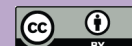
Reviewer #1: I have two concerns about this paper. First, this paper clearly covers three different topics which are (i) temporal variability, (ii) methodology, and (iii) special variability. Though the valuation is ultimately up to the editorial policy of the journal, I got an impression that the scientific target/focus of this paper is a bit unclear. It is

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perhaps for this reason that, after reading through this very dense manuscript, I was left with a feeling that I was a little uncertain as to what the message of the authors is. I am not suggesting to divide the paper into three papers. However, I would encourage authors to explain in the introduction chapter more about the rationale to put those three topics together in one paper.

Reply: We have clearly indicated in the introduction the objectives of the paper: a new paragraph has been added and the two last paragraphs have been rewritten in order to precise the objectives.

Reviewer #1: Secondly, I wanted to read more about implications of the results in larger pictures of palaeoclimatology and palaeoecology. In other words, it was inevitable to get impression that the paper is somewhat descriptive. Does the optimum hydrological condition to the tree growth contribute to reconcile any ongoing debates of paleo-biogeography? Does the trend of climate changes reconstructed using more robust method tell anything about forcing mechanisms of the Holocene climate changes? Does the spatial distribution of climate give any insight into mechanisms of Holocene climate changes? I strongly believe that addressing discussions to these questions would make the range of the beneficiaries of this paper much wider.

Reply: We have added a discussion section to try to explain the timing of the EAM as well in terms of proxy interpretation than climate mechanisms.

Reviewer #1: Location map of Bayanchagan site as well as other sites other sites mentioned in this paper (such as Dongge and Sambao caves) is necessary.

Reply: A map has been added (fig 1) with the location of the sites cited

Reviewer #1: It seems that the author considered possibility that a sort-lived cooling event at c. 8.5- 8.3 ka in Inner Mongolia was equivalent of the famous 8.2 ka events, and rejected (or at least suspended) the possibility because several such peaks are reconstructed during the Holocene. I am personally interested in knowing if those sev-

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eral peaks have chance of being equivalents of the Bond events and/or the monsoon declines reported from Dongge cave by Wang et al. 2005 Science.

Reply: We cannot reply to that question. Our data do not show any significantly enough cooling. I personally think that it is not because other authors have documented an event that this event must be privileged among the numerous wiggles of our curve.

Reviewer #2: I. I highly recommend a location map that delineates the dizzying array of regional geographic descriptions used in the manuscript. Those not intimately familiar with East Asian geography will certainly have trouble visualizing the differences discussed. For example, the abstract (15-21) and p215 (15-25) discuss southern China, northwestern China, north China, northeastern China, north-central China, and northern east-central China as well as Mongolia, northern Mongolia, and inner Mongolia. To the extent that this manuscript discusses the heterogeneity of the monsoon response both spatially and through the Holocene, geographic clarity is important.

Reply: Map has been added at figure 1.

Reviewer #2: II. Rationale for this quantitative work is set out in the introduction p215 (10-23) as an attempt to reconcile the heterogeneity (complexity) of the EAM, possibly due to the different responses of environmental proxies to climate change. The manuscript proceeds on to do an excellent job of reconstructing quantitative estimates of temperature and precipitation from Lake Bayanchangan but then does not close the circle by interpreting their results within the context of the heterogeneity in the timing of monsoon maxima across China as discussed in the introduction. I am left wondering if the authors are suggesting that their quantitative reconstruction of temperature and precipitation from Lake Bayanchangan can be interpreted as the timing of monsoon response across the whole of East Asia or if the differences in timing of their data set relative to other proxy data is further evidence of the regional heterogeneity widely discussed in the literature. In other words, if all other proxy data sets from East Asia could be similarly cast in terms of underlying

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temperature and precipitation drivers, would the East Asian monsoon look far more homogeneous in terms of the timing of changes in monsoon strength? Such issues are important with regard to the ultimate goal, which is to understand the physics behind changes in monsoon strength. Homogeneous and heterogeneous regional responses imply different underlying physics. This might be addressed in a Discussion section added between current sections 4 and 5.

Reply: We have added a new section (discussion) where we have tried to propose some ideas about the timing and the atmospheric circulation implications.

Reviewer #2: III. Both calendar years and 14C years are used making comparisons awkward. For example, the introduction uses 14C years in discussing heterogeneity of monsoon timing from the literature whereas results from Lake Bayanchangan (Figure 1) are plotted in calendar years. Calendar years would be preferable, to the extent possible.

Reply: The problem is that in the literature, some dates are given in C14. We took the liberty to change all the dates in calendar ones (roughly)

Reviewer #2: IV. Figure 1 comments. A supplemental figure plotting all three components used in PC1 would be useful to the reader in assessing the PC1 time series. Figure 1 would also benefit from plotting the underlying age control points, presumably 14C ages. This would better allow the reader to assess the age ranges of the transitions in the time series plots. Plotting precession ( $x-1$ ) as a dashed line in figure 1d might be useful. I find it striking that MTCO, MTWA, and MAP all have maxima extremely consistent with the timing of precession minima (June 21 perihelion) whereas Tree scores, PC1, and alpha all reach maxima later. A short discussion of this relationship might be useful.

Reply: We have added the three components (d18O, pollen and Pediastrum concentration) on an additional graphic in figure 2 (before fig. 1). We do not think that it is a good idea to add insolation as atmospheric circulation cannot be restricted to that

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parameter. Moreover only maximum of insolation at 9 ka BP is of interest for the discussion. The age control is discussed in Jiang et al (2006). Only the cal time scale is of interest for the discussion.

Reviewer #2: V. I recommend that the discussion comparing MAP and Dongge/Sanbao caves be accompanied by a figure as I do not follow the similarities presented in the text p220 (7-17). It is certainly the case that both increase strongly at 11.5 ka. However, Lake Bayanchangan MAP decreases sharply at 5 Ka (down to values found at 12 Ka) whereas cave d18O decrease only minimally at 5 Ka (a very small fraction of the amount necessary to reach 12 Ka values). This is a fundamental difference that should not be overlooked. One might interpret this difference in the context of latitude, postulating a rapid northward advance of the northern limit of the summer monsoon at 11.5 ka (beyond 41°N) followed by a slow retreat, falling back south of Lake Bayanchangan by 5 ka, while the caves, being further south, remain under the influence.

Reply: This interpretation is interesting. we have used it in the new discussion section.

Reviewer #2: VI. Figure 3 comments. The figure 3 caption could benefit from additional text including the statement that the plots are anomaly plots for 6 Ka relative to modern. The color scale bars would benefit from definition of the units (°C..).

Reply: It is added in the caption.

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Interactive comment on Clim. Past Discuss., 4, 213, 2008.

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