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Climate of the Past Discussions

Interactive comment on "Sources of holocene variability of oxygen isotopes in paleoclimate archives" *by* A. N. LeGrande and G. A. Schmidt

Anonymous Referee #3

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Formal review of manuscript entitlted "Sources of holocene variability of oxygen isotopes in paleoclimate archives"

The authors present a series of model simulations of the Holocene with a coupled atmosphere – ocean GCM model for times slices in the Holocene, including the full description of water isotopes within the water cycle. The authors aim at understanding the regional variability of the water isotopes and its link to the recorded climate changes in paleoclimate archives through the course of the Holocene.

As it stands, the manuscript clearly lacks several crucial points to meet the goals stated by the authors: a thorough comparison to previous modelling studies, a better

model to data comparison and a detailed discussion of the model setup and strength. The setup of the paper is a bit careless, the authors discussion a few points but lacking exhaustive discussion of their otherwise very interesting simulations. All points need to be improved to merit publication in Climate of the Past.

1 Model to model comparison

The authors present their suite of simulations of the Holocene using a snapshot approach with varying boundary conditions. While the discussion of the climate obtained is interesting (part 3.1) it completely lacks a comparison of previous simulations with similar boundary conditions of the same period. In particular, it should include a throurough comparison to the simulations performed within the PMIP-2 framework, where several coupled GCMs where used for the same purpose. Also, the authors should consider a comparison of their results to the extended work done by H. Renssen (cf. Renssen et al., 2005 Renssen et al., 2009, ref. below) in simulating transient climate change throughout the whole period they consider here. Including such a comparison would enable them to discuss not only the 9kyr B.P. simulation as they do now but also the other simulations they have performed and offer a broader view of climate change through the Holocene. That would be a big plus, as not many groups have performed millenial snapshots of that period.

References Cited:

PMIP-2: *http://pmip2.lsce.ipsl.fr*
Renssen, H., Goosse, H., Fichefet, T., Brovkin, V., Driesschaert, E., and Wolk, F. (2005) Simulating the Holocene climate evolution at northern high latitudes using a

coupled atmosphere-sea ice-ocean-vegetation model. Climate Dynamics 24, 23-43.
Renssen, H., Seppä, H., Heiri, O., Roche, D.M., Goosse, H., Fichefet, T. (2009) The temporal and spatial complexity of the Holocene Thermal Maximum. Nature Geoscience, *doi: 10.1038/ NGEO513*

2 Model to data comparison

While the manuscript attempt a fairly interesting model to data comparison for the region of the Asian monsoon systems, the manuscript generally lacks model to data comparison. Indeed, there is enormous amounts of isotopic data from merely all regions of the world covering the Holocene period. Therefore, the authors should consider including data – model comparison for different key regions and not only the Asian monsoon area. This is important as the authors need to convince the reader that the changes they observe in their model is reproducing the observed changes well and not only in one specific region. I would like nonetheless to point out that I appreciated very much the discussion of the evolution of the T°C to d18O slope in Greenland, a discussion that would need to be extended to more regions as well.
The discussion of Water Vaport transport changes (their part 3.3) is also lacking a thorough data – model comparison. While the authors state a few good points about the changes of source region of precipitation as a important driver (as it should be indeed), they completely missed discussion of the relevant deuterium excess measurements for Greenland that provide data constraints on the relative changes ÂńÂăsourceÂăÂż versus ÂńÂăsiteÂăÂż of the isotopes in precipitations. Such a comparison is required in order to support or invalidate the changes observed by the authors in their model.

3 Discussion of the model itself

Probably the most difficult point to discuss, but one that cannot be avoided. The authors are uing a model and as any model it is not perfect. Therefore, the authors need to discuss the weaknesses / strong points of their model with respect to other studies. They also need to discuss the mechanisms lacking in their setup. A few examples: the authors do not account for the changes in vegetation for their simulations: how do they expect that to impact their climatic results? Also, the d18O changes in the water cycle in the vegetation/runoff: how is it dealt with? What are the expected impacts/shortcomings?

4 Other comments

pp. 1138, lines 4-7 : are changes in the land-sea mask prescribed or not? Implications for the results?

pp. 1139, lines 21-27: how do you prescribe the additional meltwaterflux? What is the impact of the chosen the d18O value for that flux with respect to different values? (you may expect the melting of old ice to have a d18O different to the one falling on the ice-sheet at one time)

pp. 1140, lines 17-21: changes in the interocean water transport, Panama: this has already been analysed in previous studies ... how does your results compare with them?

pp. 1143, line 25-27: I do not understand what you mean with your last sentence? Is it in the model or in reality? Impacts?

pp. 1144, lines 6-8: what do you mean?

pp. 1144, lines 20-21: why is the transport diminished? Is it an effect of sea-level changes? Oceanic conditions overall? Then how?

pp. 1145, lines 6-9: intermediate water formation in the Pacific: data evidences? And inn other models?

pp. 1146, lines 2-4: this is not a very informative statement for the reader ...

pp. 1149; lines 8-11: Changes in Sahelian rainfall are too small. How are the changes in the d18O in precip. comparing with data? (underground lakes d18O ?)

Figure 8: specify the axes ...

Interactive comment on Clim. Past Discuss., 5, 1133, 2009.