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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 #1

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This manuscript explores the Holocene trends in climate and precipitation isotopic composition using 8 Holocene time slice simulations conducted with an isotopic coupled ocean-atmosphere model. The focus of the paper is the interpretation of proxy records of precipitation isotopic composition. The authors highlight the importance of large scale moisture advection in the isotopic signal. This manuscript is a new and original contribution fully relevant for Climate of the Past. Some aspects in the structure of the paper should be improved before it can be accepted for publication, and I suggest major revisions.

In the description of the model boundary conditions and in the results, I find that there is a complete blackout on obliquity (e.g. p 1135, Table 1, discussion of SST trends and global temperature trends, p 1138, 1147. . .). The authors must explain if they also

included obliquity forcing. If they did, they must be aware that Holocene annual mean insolation has opposite trends at low and high latitudes due to obliquity trends, and that this cannot be ignored as a driver at the global scale. Several recent papers have also been dedicated to the differential influence of precession and obliquity on land and ocean seasonal cycles and monsoons.

The manuscript structure is weak regarding the relative importance of section 3 (results) and section 4 (isotope record comparisons). Section 4 should be removed. The skills of the climate and isotopic model for present day should be discussed in section 2. The Holocene isotopic and climate trends must be discussed throughout section 3.

The manuscript would clearly be more convincing by including a more detailed comparison with the data throughout an expended and better structured "results" section. The authors can improve the model data comparison by representing trends in ice core data from both Greenland and Antarctica (and possibly including deuterium excess as an important control for the moisture origin features) together with model results. The model data comparison is a weak point of this manuscript. Regarding climate outputs, there is no assessment of the realism of the model response, for instance regarding ITCZ shifts. Could the authors go futher and discuss if the spatial shifts are compatible with the available data? They have been numerous efforts within PMIP to reconstruct for instance African monsoon precipitation changes at 6k. This model should at least be discussed in this respect (p 1140). This model-data aspect also deserves a clear discussion of changes in seasonality (which can be represented as the fraction of JJA precipitation to annual precipitation).

Most of the paper description is focused on the difference between the early Holocene (with maximum precession and obliquity forcing) and pre-industrial, and the authors do not fully take advantage of the whole set of simulations. They may want for instance to test how much of the model response (regarding climate and isotopes) is linearly responding to the instantaneous forcing and if they are threshold effects. Another key question is the importance of a coupled ocean-atmosphere approach for the simulation of the full isotopic water cycle. Could the authors discuss the added value of coupled simulations compared to earlier studies conducted with atmosphere only models? How much does the isotopic coupling modify the atmospheric response (compared to atmospheric simulations which would use the same SST and sea ice but without changes in ocean surface isotopic composition)?

Section 3.3 is very interesting and an original contribution of this manuscript. It would be valuable to separate the seasons (DJF, JJA and annual mean) and analyse their impacts on the large scale advection of depleted tropical water vapour. I encourage the authors to go further in their analyses and to characterize the importance of changes in convection and lateral transport in the simulated structure of water vapour 18O and finally precipitation 18O. The caption of Figure 8 seems inappropriate with respect to the titles of the individual figures. This discussion of large scale atmospheric water cycle dynamics would deserve to be placed together with the discussion of the links between precipitation 18O, local rainfall amount, local temperature, and moisture advection (p 1142). The authors chose to discuss only two locations (India, China) and the readers are interested by a larger perspective: it would be valuable to show a map of temporal correlation between local 18Op and local rainfall amount, and the correlation between 18Op and temperature (annual and seasonal aspects).

The discussion of the consequences of changes in Bering Strait flux (p 1145) is too short and deserves to be fully developed when considering the termination of the Younger Dryas.

The discussion is not clear. I cannot understand how any feature given here demonstrates the potential to determine model sensitivity from paleoclimate modelling. One of the main problem when considering interglacial climates, is the fact that the global radiative forcing due to orbital features is null. While the authors point that precipitation 18O is strongly influenced by large scale advection features in specific areas (here, downwind of the Laurentide and in China), the manuscript does not fully demonstrates how much of the advection features affect more strongly 18Op compared to climate

variables in other areas. They do not take advantage of available deuterium excess data which can be used to assess the realism of the simulations, or, alternatively, propose improved interpretations of this parameter.

There should also be a discussion of the artefacts involved in proxy records of 18Op. While ice cores provide direct archives of past precipitation, this is clearly not the case for speleothems. Figure 6 therefore compares changes in calcite 18O with changes in precipitation 18O. The manuscript should at least mention this and discuss the processes at play linking precipitation 18O and calcite 18O.

The conclusions of the paper are a frustrating for the reader. The fact that the model captures the first order of major Holocene changes is important but is not enough to assess quantitatively its skills. The paper does not present any quantitative assessment of the added value of the stable isotopes regarding the magnitude of simulated versus observed changes (the two main biases discussed in the conclusion regarding Sahel rainfall and sea ice changes are based on other types of datasets).

Detailed comments. 1. As the manuscript is mostly based on climate modelling without proxy or archive modelling, I would suggest to change the title to: “Holocene trends in precipitation isotopic composition: a modelling study”. 2. Introduction on stable isotopes. I suggest to use the word “distillation” to describe the processes influencing air mass isotopic composition. Please update the references describing the links between isotopic composition of precipitation and climate (e.g. IAEA review papers at least, more data are available now than in Dansgaard 1964). Explain clearly the time scales discussed here (ex : line 24, “short time periods”, does it refer to events, seasonal cycle, interannual variability. . .). The writing “thought to correlate to” is misleading : it does show a correlation, but the authors mean that the processes at play are complex and involve other features than just local precipitation (such as convection etc). It must be clearly stated. Do tree ring cellulose data provide “long term” records? (define “long term” here). The choice of references cited here is curious (not the pioneer papers nor the most recent or longest records). I would recommend to cite review papers (e.g. Mc

Carroll and Loader QSR 2005 for tree ring cellulose, Masson-Delmotte et al Clim Past 2006 for isotopes in ice cores, Laschnier et al QSR 2009 for speleothem 18O…). 3. Isotope-climate relationships. Why use the word “gradient” here rather than “slopes”, as commonly used? 4. Greenland isotope-climate relationships (page 1136). There is a misleading presentation here of changing glacial interglacial slopes when discussing Holocene variability. The current state of the art takes advantage of different paleothermometry methods at the glacial interglacial scale (borehole temperature), abrupt events scale (gas thermal fractionation), changes in moisture origin (deuterium excess) and climate simulations (pointing to the importance of intermittency/seasonality of precipitation). I recommend that the authors make a clear discussion of the state of the art using the right key papers. Basically, there has been until now very few studies on the isotope-climate slopes on the time scale of the Holocene. 5. Model results. Why do the authors chose to discuss only 18O and make no use of their modelled deuterium excess? There are records available in Greenland, Antarctica, and a few tropical glaciers which have deuterium excess data (for reviews, see for instance Vimeux et al, Clim Dyn, 2001 for Antarctic deuterium excess, or Masson-Delmotte et al JGR 2005 for Greenland deuterium excess). 6. Ice sheets. Can the authors make clear that the topography of Greenland and Antarctica is constant throughout the simulations? 7. Averages. Do the authors take into account the problem of duration of seasons in their calculation of annual means (lengths of months within another orbit)? This can be a problem and has to be clearly explained (see for instance Timm et al Paleoceanography 2008). 8. When discussing processes at play that propagate the summer precession forcing into winter climate, the authors may want to mention that vegetation can add to sea ice action, but is not represented in their simulations. 9. Regarding ice sheet freshwater, the authors clearly explain the impact of the Laurentide ice sheet imbalance to the freshening of the Labrador Sea. However, they do not mention anything about Greenland. How is Greenland mass balance taken into account here? 10. Moisture origin. It seems that the authors have diagnosed within their simulations the relative contributions of various moisture origins to Greenland 18O. I recommend that they rep-

resent the simulated change in moisture origins through time, the modelled deuterium excess, and the comparison with the ice core data (Holocene data are available from GRIP and NorthGRIP, Masson-Delmotte et al, JGR, 2005). 11. I do not think that Hoffmann (2003) showed that the tropical water cycle is closed (page 1143, line 18). Please quantify the impact of methane change in stratospheric water vapour isotopic composition (p 1143) if it is significant. 12. Figure 5 is misleading. The slope of 0.3 per mille per °C (Cuffey et al, 1995 but also Masson-Delmotte et al, Science, 2005) is not “standard” and is expected to hold true at the glacial interglacial scale (due to changes in seasonality and moisture origin) but there has never been any argument to use this slope over the course of the current interglacial. 13. The discussion of the skills of the isotopic model regarding present day Antarctic has been published for locations other than just Vostok (Masson-Delmotte et al, J Clim, 2008) and could be cited. 14. Change the reference to Andersen et al (2008) to NorthGRIP community members (2008). The authors discuss their results at Summit while referring to NorthGRIP ice core (page 1146).

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