

***Interactive comment on* “Modelling ocean circulation, climate and oxygen isotopes in the ocean over the last 120 000 years” by R. Marsh et al.**

Anonymous Referee #2

Received and published: 12 October 2006

General Comments

R. Marsh and co-workers are simulating a full glacial - interglacial cycle with an intermediate complexity model (GENIE-1) including some representation of the oxygen-18 isotope. Their major outcome is meant to be that "*the best agreement between simulated oxygen isotopes record in the North Atlantic and core measurements is found in the experiment that includes MWPs around Antarctica*" challenging "*previous assumption about the dominant role of northern ice sheets in glacial sea-level variability*". (Abstract, lines 16-19, see also Conclusions, lines 10-14).

However, Marsh and co-workers' paper is not convincing in proving this statement, nor are the authors correct in the path used to simulate their goal, which would otherwise be an important contribution to the Past Climate Sciences. In the following, I list several major concerns, all of which need to be thoroughly assessed in order for the paper to be acceptable for publication. I also list a few minor points that would need the authors' attention.

Major Concerns

1. **Model Setup.** The setup for the glacial to interglacial simulations is somewhat awkward, as it is a mixture of both present-day fixed conditions and glacial to interglacial varying boundary conditions. Land-sea mask, ocean topography and climatological winds are prescribed to present-day state, whereas insolation, CO₂ and the ice-sheet cover (that is albedo and temperature related changes to orography as the winds are fixed) varying at glacial-interglacial pace. The vegetation is prescribed to present-day (therefore leave out big changes in vegetation cover influencing the albedo) although the vegetation is "*interpolated as the ice sheet grows and shrinks, dependent upon the fraction of ice present in the grid cell*" (page 667, lines 11-12). It is utterly not clear what is therefore done to the vegetation around ice-sheet when, say, the ice-sheet expands to a tree covered area. Is the ice-sheet replacing the fraction of tree with respect to the fraction of occupied cell? (meaning there are some trees sharing the cell with an ice-sheet? An unlikely climatic occurrence). This overall set-up is mentioned, but the paper completely lacks any discussion upon what are the implications of the choices made for the basic model set-up. The discussion is rendered even more complex by the fact that the authors are using two different version of the same

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model in the paper, but with different parameters values. The huge differences in the parameters used (e.g. isopycnal diffusion varying by a factor of two, diapycnal diffusion by a factor of 5, sea-ice diffusion by a factor of three) are not justified with respect to what is measured in (or is inferred from) the data. In principle, I would guess that the version tuned to present-day state by the "Ensemble Kalman Filter" would fit the climate in a more appropriate way. But in the rest of the paper the authors are using the "manually tuned" version of the model, better fitting the observation on the particular timescale. Therefore, why introduce the two versions? That only complicate the discussion, and as parameters choices are not justified, it doesn't bring more constraints (on dynamic for example).

- Control Climate.** Another very difficult matter is the quality of the climatic states simulated with respect to available data. As for the present-day state, it is mentioned to be comprehensively tuned to the present-day climatology. We can therefore assume that the model has some skill in representing it. But is this the case of the glacial? Given the, to say the least, questionable choice of model setup, it should be at least made clear if the climate obtained under these choice of parameters is consistent with data for some reference periods. In the time frame the authors have chosen, I see to main periods suitable for such a validation: the last interglacial climate and the last glacial maximum climate for which data model comparison have already been done. In particular, the availability of data for the Last Glacial Maximum (see Kucera et al., 2005a and companion papers, MARGO database) should be a natural target to assess if the model is doing a correct job in simulating past climates. LGM climate state has never been shown for this model, therefore casting doubts on the reliability of the glacial climate simulated - on which the conclusion of the present paper heavily relies. An example can be taken in the only information provided, that is the present-day (end of simulation) meridional overturning circulation plots. The two version of the model used after different tuning procedures are showing very different overturning patterns. The

"traceable" version of the model presents a maximum overturning streamfunction in the Atlantic at around 25 degrees north whereas the "subjective" version has it 50 degrees north. These are enormous differences, and I don't expect the model to produce the same present-day climate. What are then the differences, relevant to the study realized here? Furthermore, no version of the model has any Antarctic Bottom Water (AABW) entering the Atlantic for the present-day case. Without more information, there is no way to tell how this changes in the LGM climate. But the feeling one gets from the overturning strength figures (8a-c), is that there is no much changes in this situation. How can then the authors expect to represent correctly the interplay between AABW and North Atlantic Deep Water (NADW) which seen in the core at the Iberian margin (Shackleton et al., 2000, Skinner et al., 2003)? The two model versions are producing 10 degrees difference in Antarctica ... Why? Through which processes?

- d18O forcing.** In the model used in this study, there is no explicit simulation of the d18O in the water cycle. In fact, what the authors are labelling "d18O" is a conservative tracer in the ocean, tagged with meltwater at -30 per mil and modified by global mean evaporation. If this limitation (namely the absence of d18O in the atmospheric water cycle) is mentioned, implications of this choice are not discussed until the conclusions are reached. Only there can we find "*Also necessary is the explicit representation of the 16O/18O fractionation caused by both physical (evaporation/condensation)*" (page 686, lines 20-22). If it is necessary, why not take it into account, even in a simple way? Or at least discuss the implication of the missing process on the result by evaluation of the changes it might have produced (using Juillet-Leclerc et al., 1997 for example, cited by the authors). Indeed, in a climate very different from our own, the changes in water cycle might have huge importance.
- Sea-level & d18O forcing.** In the view of assessing the role of northern vs. southern meltwaterpulses (the goal set by the authors), the reconstruction used

for sea-level and the scenario for the meltwaterpulse is of primary importance to assess this goal in a consistent manner. Here, the authors are imposing varying ice-sheets using the sea-level curve of Siddall et al., 2003 (Siddall03 hereafter) to extend the reconstruction. Thus, they choose Siddall03 as sea-level for their study. However, careful reading of the paragraph on d18O forcing shows that they don't force the d18O cycle in the model with the same reconstruction. Indeed, they choose to apply (I take the MWP3 scenario as example) a -0.041 Sv constant evaporative flux from 120 kyrs BP to 20.5 kyrs BP, a $+0.08$ Sv deglaciation flux between 20 and 10 kyrs BP and a series of Meltwaterpulse of different magnitudes. In order to be coherent, these assumptions taken together should be quite consistent with the Siddall03 reconstruction in sea-level. It seems however not to be the case. A 0.041 Sv flux during 99500 years as the authors suggest is a sea-level drawdown of -314 meters equivalent sea-level (considering no evaporative flux during meltwater pulses). The deglaciation background flux account for 72 meters of rising sea-level. The meltwaterpulses introduced (as far as I can tell from the figures) are giving a total amount of 172 meters (106 meters from the south (!) and 66 meters from the north). That is a total of: $-314+172+72 = -70$ meters equivalent sea-level (e.s.l.). Therefore, this seems not to really match the hypothesis that the present-day level is about the same as the last interglacial, as the authors stated it (the end of the simulation being -70 meters e.s.l. below present). Moreover, given the strong pulses introduced in this MWP3 scenario in the Antarctic pulses, the maximum of sea-level lowstand is attained shortly before 60 kyrs B.P. instead of the 20-23 kyrs B.P. observed in Siddall03. This major discrepancy maybe only arises from not correctly explained fluxes in the text, but in any case, the same forcing should apply to the ice-sheet extend and to the sea-level, used in the d18O forcing. Even more, each strong Antarctic flux is a total of about 25 meters of e.s.l. rise in 2,000 years. However, the total LGM to present-day storage of ice in Antarctica is simulated to be 14-18 meters e.s.l. by state of the art ice-sheet models (Huybrecht, 2002). Following the scenario from

the authors, the Antarctic ice-sheet is oscillating very rapidly between full glacial and less than present-day ice volume in very short time, under glacial climate. I consider this hypothesis as very unlikely.

5. **Statistical relationship between oceanic cores and model.** After integrating the full length of the interglacial-glacial-interglacial cycle in the model, the authors present a comparison between simulated calcite and measured in different geographical locations. The model seems to represent quite well the overall trend in calcite, with respect to the data, although the millennial scale variability doesn't correspond well. However, this is only a direct linear effect of the forcing imposed to the $d18O$ of the water, as can be seen from the $d18O_w$ figure 10. The effect of the different scenarios used (although they are extremely different) is small. In fact, given the scale and format used in the figure, they are barely distinguishable. I can not agree with the authors when they state that "*However, some multi-millennial variability is captured through MWP forcing, and on visual inspection of the curves in Fig. 12 there is a degree of agreement between model and observations. In particular, secondary maxima in $d18O_{calcite}$ at 60-70 ky BP are simulated in MWP-3. These features, at all three sites, are a global signal associated with extensive glaciation prior to around 65 ky BP, which is only specified when we include extra MWPs in the southern hemisphere.*" I do not agree. In my reading of the figure, all MWPs are equivalent in terms of agreement (on a visual basis, for statistics see hereafter). Therefore, if any conclusion can be drawn from such a figure, it is that freshwater forcing in the GENIE-1 model does not matter much at glacial / interglacial time scales, but that global evaporative and deglaciation fluxes suffice. Even so, there are big differences with respect to the data in the first 20 thousands years of experiments where the imposed scenario is not a fast enough glaciation / the model doesn't cool enough. What is the model doing at the surface at the Iberian margin? Surface is shown for two sites and deep for one other. But they all have very similar (in pattern) time evolution. What would

be with a different time series, as the surface at the Iberian margin?

This is of major concern, as it is the very base of the conclusions drawn from the paper. Performing a correlation between simulated and observed $d_{18}O_{\text{calcite}}$, the authors find high correlation (from 0.65 to 0.90). However, this is not of strong support to their results, as they argue. Indeed, if the correlation is high, it is only because both the simulated (with any scenario) and the observed calcite (at any site) show a linear increase in $d_{18}O_{\text{calcite}}$ towards the LGM and a subsequent linear decrease during the deglaciation. To really assess the role of short-lived events, the correlation should be done between the two series after removing the general glaciation-deglaciation trend. Such a comparison would have some merit to disentangle the role of meltwater pulses vs. global sea-level induced variations.

To conclude this part, I would say that the strong and even slightly controversial conclusion brought by the authors: "and we propose an extension of that hypothesis to invoke the important contribution of substantial changes in Antarctic ice volume, particularly during Marine Isotope Stage 3 for which we identify five MWP events of southern origin." (page 686 lines 12-14), is not supported by their present results. Although it is of great importance to assess the role of the Antarctic ice-sheet in the Glacial-Interglacial cycles, this would require much more careful examination and coherent experiments set-up to reach a sound conclusion.

I therefore recommend major revision before the paper can be accepted in *Climate of the Past*.

Minor Concerns

- Figure 2 is redundant: insolation forcing over the last deglaciation is a well-known feature, and it is not used in the discussion

- On Figure 3 panel b), the sea-level forcing used by the authors for d18O should be superimposed to the reconstruction.
- Figure 6: Why is the comparison done with the GISP2 record? A more appropriate comparison would be achieved with the NorthGRIP record which spans the whole time period covered by the study.
- Figure 7: Provide LGM streamfunctions as well
- Figure 8: Use a proper scale, the figure is impossible to read in the present form

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Interactive comment on Clim. Past Discuss., 2, 657, 2006.

CPD

2, S453–S461, 2006

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