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Climate of the Past Discussions

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

R. Marsh et al.

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We thank reviewer 1 for a thorough review, and we look forward to improving the paper accordingly. Below we outline our response to each group of comments. We agree with most of the comments, and our most extensive response concerns the issue of model setup. In order to thoroughly revise the manuscript, we will need to largely re-run model experiments. Although regrettable in terms of required effort, no other course of action is appropriate.

1. Model Setup We acknowledge that our treatment of oxygen isotopes is crude. To address the first-order effect on oceanic delta O-18 of sea level change, we will repeat the experiments with delta O-18 forcing that is consistent with reconstructed sea level

change. This may prove challenging, as the reconstructed sea level records are not smooth, so some further analysis of these records will be necessary. To account for changes in surface delta O-18, we will consider additional relaxation of surface delta towards values consistent with local surface salinity, according to the established delta O-18 - Salinity relationship, ensuring that the global-mean effect of this relaxation (on delta O-18 in surface waters) is zero. Whether this approach proves successful remains to be determined. We will note the Roche et al. (2004) sea level rise estimate for H4 of 1-3 m, and we may reduce our hypothesized HE amplitudes accordingly.

We do not use the benthic delta O-18 from the Iberian margin as a "constraint on the magnitude and duration of Heinrich Events", but rather as a target for our simulation. As well as variations in the isotopic signature of ambient waters at this core location (hence HE-type signatures), we also simulate the effects of deep ocean temperature variability, through changes in the relative influence of northern and southern water masses, changes in the source properties of those water masses, and changes in advective/diffusive balance. We acknowledge that GENIE-1 does not represent AABW, but this is hardly surprising at the low resolution necessary for such long transient runs. We will discuss this issue in the revised manuscript, and show the extent to which the model glacial ocean is consistent with paleoceanographic data. Note that bottom temperature in the glacial Atlantic is in fact 1.5-4.5°C lower than for the present day (see Fig. 11a).

We will examine the cited papers (Manabe and Stouffer 1997; Fluckiger et al. 2006) for a clearer appreciation of when and by how much the ocean cooled during THC shut-downs, and also carry out an additional experiment with a less stable THC (by reducing the additional Atlantic-Pacific moisture flux close to a threshold value for maintaining the Atlantic Conveyor). Such an experiment may simulate longer THC shutdown, extending the collapsed state shown in Fig. 9 to a multi-millenial timescale akin to that of D-O cycles. However, the true depiction of D-O cycles is almost certainly impossible with the present model setup (or any other we no of that does not invoke the stochastic

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resonance hypothesis).

2. Antarctic Scenario We concede that rather unrealistic Antarctic melting (and implied subsequent accumulation) is hypothesized. This will be downsized in new simulations, taking fully into account the quantitative argument of the reviewer. We tentatively expect to specify 25% of the original Antarctic MWPs. We will also bring this interpretation of our Antarctic mass balance into the revised manuscript.

3. Analysis We will bring more objectivity to the revised manuscript, pending the new results. Any summarising/concluding statements will be moderated accordingly.

4. Bibliography & Specific Comments We thank the reviewer for the diligent attention to detail. We admit to several confusing or erroneous statements and we will omit or correct each one.

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