Response to Referee #3

[Anwers are provided in Italic, all section numbers relate to the new manuscript, unless otherwise stated]

GENERAL COMMENTS

The study by Kageyama et al. describes the response of the IPSL climate model to freshwater perturbations imposed on a glacial background state. The response of the Atlantic Meridional Overturning Circulation (AMOC) appears to be very sensitive to small differences in the applied freshwater forcing, and the spatial and temporal characteristics of the response of the model are described in great detail. It is crucial to understand the sensitivity of the climate and how it might be different depending on the initial background state of the system. Such a study should also help elucidate the mechanisms responsible for past abrupt climate changes such as the Dansgaar-Oescher events of the last glacial cycle.

One major weakness of the manuscript is its excessive length and lack of focus. In particular, the main scientific contribution of the work should be stated clearly in the text and repeated in the abstract. At the same time the significance of the new findings should be emphasized. Although the introduction includes an extensive literature review, the summary section does not discuss the relevance of the new findings in the context of existing proxy data and previous model studies, nor their impact on future work in the field.

It could be in the interest of the authors to focus their results in more than one manuscript. In this case it would be better to focus on one of the main topics presented in the paper such as (this is not an exhaustive list):
1) the response of the ITCZ and monsoon system to AMOC changes;
2) relevance of changes in ITCZ and transport of moisture across latin America;
3) transient nature of AMOC response;
4) possible link between AMOC changes and signature of H-events as seen in proxies.
If the authors chose to continue with the present format it would be best to skip experiment LGMb as well as several of the figures (e.g. 9,10, 11, 12, 14).

We thank the reviewer for his helpful comments.
We have removed some topics from the manuscript (e.g. the discussion on the hydrological cycle at the end of the manuscript) but have chosen to keep most of the results initially presented to give a broad panorama of the results from our simulations. However, we agree that the manuscript lacked focus in the sense that its organisation was not very straightforward. We have therefore reorganised it so that it is now more fluid and (hopefully) easier to read. These corrections are presented in the response to reviewer 2. We have also added a short comparison to paleo-data in the conclusions. The abstract has been rewritten and shortened to better represent this new organisation of the manuscript.

SPECIFIC COMMENTS

A few suggestion for improvements are as follows:

1) Abstract needs to clearly summarize the main findings and their significance.

We have modified the abstract accordingly.

2) It is stated that the observed rapid millennial scale climate changes observed during the glacial period are a response to changes in the AMOC. This is not clear from the proxy data and is rather a hypothesis to be tested and should be clearly stated as such.

We agree and have modified our introduction in this sense (see also the responses to the other referees).

3) Introduction is nearly 8 pages long and should be shortened substantially. Part of the discussion of changes observed in the proxy records should be moved to the end of the manuscript and discussed in the context of the model results.

We agree that our introduction is rather long. However, Reviewer 1 pointed out that s/he liked the review of the data and model results. We also had to add a few words on the rationale of the study, namely that we were testing the idea that abrupt climatic changes during the last glacial could be related to AMOC changes, and devoted a section to the objectives of the manuscript, which increases its focus. The resulting introduction is not shorter than the initial one, but it hopefully helps to focus the manuscript. Model-data comparisons have been added in the discussion of the results and in the conclusions.

4) In the introduction it is stated that one has tried to tie observed changes in the ocean to the abrupt changes in temperature on Greenland (D-O events) and that marine proxies suggest that the most stadials show little evidence for ocean circulation changes. At this point it should be made clear to the reader if the simulated AMOC changes are used as an example in the context of H-events (and not D-O events). In addition, a discussion of the difference between these events is required as most of the records described in the introduction have been related to D-O cycles and not to H-events. Note that in most records H-events do not have a strong climate signal, although the events are thought to have triggered AMOC changes.

These events are now precisely defined from the start, which we hope also helps clarifying the hypotheses tested in the manuscript and focussing the model-data comparison.

5) The title of section 1.1 makes a bold statement that changes observed in the paleorecord are due to AMOC changes - this is not clear from the proxy data.

Agreed and modified (along proposition of reviewer 1)

6) The description of the freshwater forcing is incomplete. A figure is necessary here to show where the freshwater is imposed in the case when the calving flux is increased. This will be crucial, as some of the far field responses observed could be due to imposed local changes in calving rate. E.g. does the calving change around Antarctica?

As requested by all reviewers, we have given more details, and hopefully clarified, the description of the experiments. The calving figures for Antarctica have been inserted in the text to show that this is a second order effect. We believe that the description of the experiments is now clearer and more complete.

7) Is the model stable? It is stated that runs LGMa and LGMb are initialized from a previous LGM run (1065.25). Is there a reference for this, and was this run stable? 250 years is not sufficient to equilibrate the intermediate and deep ocean.

Both simulations start from a previous multi-centennial LGM run in which, unfortunately, the fresh water balance was not completely achieved, but which helped correcting it. This is now explained in the model description. Also, LGMa has been lengthened to 300 years, while LGMb has been lengthened to 1000 years. The results are shown on Fig. 1.

8) The control simulation of the IPSL model is not discussed in the paper. To be able to analyze the response of the model to a freshwater perturbation in the LGM case, it should be compared to the equivalent experiment for the modern climate. Further, the IPSL model gives a complete shutdown of the AMOC when a very small freshwater pulse is applied (0.08 + 0.1Sv), this should be compared with the results of other models, e.g. Stouffer et al. (2005) who use the same flux and observe a ~30% weakening for the modern state.

A presentation of the model results for pre-industrial, present-day and future climate simulations, with appropriate citations of existing references, has been added to the manuscript. This gives the information required by the reviewer without adding too much text to the manuscript. A companion paper (Swingedouw et al, accepted for publication in Journal of Climate) shows that the response of the LGM atmosphere-ocean system to the 0.1 Sv fresh water flux is indeed very sensitive compared to the pre-industrial background state and the results presented in Stouffer et al (2006). We have added a short discussion on this topic in the manuscript.

There are also several previous studies perturbing an LGM state with freshwater which should be discussed (e.g. Ganopolski & Rahmstorf, 2001; Bitz et al., 2007; Hu et al, 2008).

This has been added, also at the request of reviewer 1 (and lengthens the introduction).

9) In the section 4.2 the surface response to the AMOC changes are assessed. This should be done in a more rigorous manner using appropriate statistical tools (regression, EOF analysis e.g.).

We are aware that powerful statistical tools are in general required to assess the surface response to the AMOC changes (e.g. Msadek and Frankignoul 2008, Clim. Dyn., published online). Here though, as in the rest of the paper, the purpose is to assess the surface changes associated with the AMOC changes. We thus believe that in the framework of this study, it is enough to interpret map of seasonal differences, together with the statistical significance tests for these differences.

10) New data is presented in the summary (section 6) with figure 17. This is not appropriate in a summary and should if necessary be presented at an earlier stage.

This figure and the discussion of this effect was removed and left for a forthcoming study.

TECHNICAL COMMENTS

1057.15: more appropriate here would be “suggest” rather than “demonstrate”.

Done

1057.22: “logically” used in this context is subjective and should be rephrased.

This term has been removed.

1059.3: Denton et al. (2005) is stated as “showing that the response to a weak AMOC is.....”, there is no clear proxy for AMOC strength that is correlated with the data presented in this paper. This is rather a hypothesis, and there is little, if no, data from periods with D-O cycles.

We have removed the reference to the AMOC in this sentence.

1058.7: “margin” is out of place. Correct sentence: “..show that simultaneous to...”

Done

1062.2: the original references should be added for the bipolar-sesaw: Crowley, T. J., North Atlantic deep water cools the Southern Hemisphere, Paleoceanography, 1992, 7, 489-497.

Done (sorry for forgetting about this reference!)

1066.25: confusing model time references. Is the LGMc output at the end of the run, i.e. model year 570?

We have put all simulations on the same timescale, i.e. that of LGM and b. Hence LGMc runs between years 151 and 570.

1069.13: there is no clear difference in temperature over northeastern Europe in LGMb vs LGMc (figure 4).

Northwestern Europe was meant in this sentence. This sentence has been removed during the process of re-writing this part anyway.

1071.15: analyzing the mechanism for the possible propagation of anomalies in the model should be done in a rigorous manner (see e.g. Knutti et al., 2004).

We agree that our study could be completed by more detailed analysis concerning each of the mechanisms proposed for the teleconnections. However, these would need additional experiments to test the sensitivity of the response to the speed of AMOC collapse, or even partially coupled experiments to isolate the precise role of the ocean, atmosphere, and interactions between those. Since we are not in the position of performing these in the time allowed for revision and since all reviewers suggest the manuscript is already too long, we have chosen to mention this shortcoming and propose it as one of the perspectives of the present work.

1073.16: there is no clear connection between “loss of freshwater” and increase in sea ice cover in the area of the Labrador Sea.

We do believe that there is a connection. Indeed, the figure shows the anomalous ice to ocean net freshwater flux, showing that the oceanic freshwater loss is linked to ice formation. The text was clarified in order to insist on this point.

Figures 4, 5, 7, 11, 12, 13, 14 and 16 show the modern land-sea mask, which, according to the manuscript, is not used in this experiment.

This is right but cannot be changed in our plotting programme. We mention that we use LGM coastlines despite the impression given by the figure in the first figure including maps.

Figure 8: this is too small and makes it impossible to see sea ice limit. Either one needs to include a separate plot of sea ice, or increase figure substantially. Would include figure here with mixed layer depth anomaly (and make figure 9 simpler to read (or skip).

Figure 8 has been merged with fig2 in the attempt to shorten and focus the manuscript. We hope that it is now clearer.