Response to Referee #2

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

General comments

The manuscript describes three different experiments with the fully coupled atmosphere-ocean IPSL general circulation model. All experiments are carried out with Last Glacial Maximum boundary conditions but different treatments of the freshwater budget yield different climate states. In particular, the strength of the Atlantic meridional overturning circulation differs between the experiments, and analogues to a possible shut-down or weakening during glacial stadials and Henrich events are drawn. The manuscript goes into great detail in describing the physical mechanisms and climatological consequences both regionally and seasonally. Particularly interesting is the description of the order of events when entering the different states.

We thank the reviewer for his/her helpful and detailed review.

Two important sections/paragraphs are missing, however: i) A discussion of this model's performance under present day conditions. How well is surface climate simulated and how do the deep water formation sites compare with observations? How will deficiencies influence the conclusions of the article?

We have now included the proper references about the performance of our model under present-day conditions and briefly summarised them in section 2, trying not to lengthen the manuscript too much, though.

How does the LGM climate of this model compare with observations and that of other models?

Comparing the results from our simulations to LGM climate reconstructions would be the topic for an entire new paper if we were to perform this task correctly. Rather, we give references for the performance of a previous LGM simulation, in comparison to reconstructions and to other models of the PMIP2 database, and state that a full comparison to LGM conditions will be dealt with in a specific paper. Given that the other 2 reviewers found the manuscript lacking focus, we have decided to remain concise in all items added to the manuscript, taking as much profit from other works as possible.

ii) Although the introduction goes into some detail about available proxy records for regional and seasonal changes during stadials and Heinrich events, the article lacks a part where the conclusions based on the experiments are held up against these records. Perhaps a table listing the observations and the model's consistency/inconsistency. Additionally, I would like to see a recapitulation, perhaps in the form of a table, of the timing of events when going between the states. This information is worth highlighting since the cross-dating of proxies is becoming much better in these years and really begins to provide a basis for understanding the transitions between states and not just comparisons between the states themselves.

At the request of reviewers 1 and 2, we have clarified the fact that AMOC changes were a hypothesis to explain paleo-records of glacial climate millennial variability and that our experiments could shed light on this hypothesis. We have added a qualitative model-data *comparison at the end of Section 3.3 and in the last section, we discuss about possible improvements to the experiments presented here. Finally the timing of climatic changes during the AMOC collapse is now summarised in section 6 (Summary and discussion).*

The results are interesting and novel since only few "water hosing" experiments have been made on top of a glacial background state and the subject should be of interest to the majority of the readership of Climate of the Past. The manuscript is well-written (albeit somewhat lengthy) and the figures and the discussions of the findings go well together. I recommend to accept the manuscript for publication pending the previous suggested changes and some other minor revisions.

Specific comments

Presentation

- 1057.21 In this line, a difference is referred to between "Heinrich events" and "stadials", with no previous description of what these are. In 1057.12, references to Heinrich and Dansgaard et al. are given but it is not clear (from the text alone) what the different events are. Of course, most readers know the difference and the background, but if this is assumed, the above references are in principle unnecessary.

We have added a precise definition of these events at the beginning of the introduction. This was certainly lacking in the first version of the manuscript.

- 1062.10-12: The sentence "When the anomalous winds pass ... ITCZ shift." is unclear. What anomalous winds? How does their direction change? If this description of the mechanism should be included, it should be clarified.

We have removed this confusing sentence. This description was not absolutely necessary and expanding its description would have been too long for the introduction.

- 1063.13-23: This paragraph discusses results from the CLIMBER model, but it is not until the last line that the reader is told that it is the CLIMBER that is discussed. This might be interesting to put in the beginning of the paragraph. Also, which version of CLIMBER?

Done.

- 1066.1-14: I had to read the description of the freshwater corrections and increased fluxes several times before (I think) I understood it. A better explanation should be considered. Add also a comment on how the different correction schemes compare with a simple water hosing experiment.

At the request of all reviewers, we have largely rewritten the description of the simulations. In particular, we have added details on the starting point of each simulation and on the closure of the freshwater budget through the ice sheet parametrisation. The description of the experiments is now hopefully clearer.

- 1067.25: What is the atmospheric heat transport and how is it calculated? Is it the total energy transport (both dry static and latent heat)? Is it calculated as an implied transport inferred from the top-of-atmosphere and surface budget or is it calculated directly from the Vq, VT and VZ terms?

It is directly calculated, at each time step during the model integration.

- 1069.14: "cooling over northeastern Europe". Is it not rather a cooling of the North Sea region (between Norway and the British Isles)?

This sentence has actually disappeared from the manuscript (but the reviewer was right!).

- 1073.26-1074.8: The conclusions drawn from Fig 10 are perhaps a bit shaky due to the very noisy nature of the series plotted in the figure. I have to admit that I am unsure how to do it better. But consider improving the readability of the figure somehow.

We reformulated this part in order to lead the reader a little more when discussing this figure (section 3.1)

- 1074.21: "the Norwegian Sea mixed layer depth sometimes reaches 1000 m". So does the Labrador Sea!

The reviewer is right, the argument holds for the Labrador Sea as well as for the Norwegian Sea. The lines about these results have been reworded in order to clarify our point: We now insist more clearly on the stability of the time series itself than the depth of the mixed layer. We have also added a reference to a paper that is now accepted which investigated the role of convection sites on the AMOC variability in a modern simulation of the same model.

-1076.5: "Central and eastern Europe, explaining the local maximum cooling there." In which figure can a local cooling in Central and Eastern Europe be seen?

Fig. 6 and 10 (new manuscript numbers). We have actually replaced "central and eastern Europe" by "eastern Europe, North of the Black Sea", which is probably more appropriate. References to the figures have been added.

- 1077.7-8: "southward shift of...". Where can this southward shift be seen?

It wasn't shown for these seasons (and this precision has been added)

- 1080.12-14: The mechanism for the Norwegian Sea warm anomaly is mentioned here but not described very thoroughly in 4.1 section. Conversely, the effect in the Labrador Sea is described in great detail in section 4.1 but is not mentioned here. This might make the reader wonder what is most important.

The reviewer is right that the mechanism for the Labrador Sea is more deeply described in the text (in the sense that at least it has a figure) than the one for the Norwegian Sea. Indeed, in our view, the mechanism for the Norwegian Sea is more straightforward than the one for the Labrador Sea, which we thus chose to describe in greater details. Moreover, for the Norwegian Sea, it corresponds to the surprising anomalous warming in the atmosphere. This was the reason to insist on this region in the final section. The manuscript was modified in order to insist on this point.

Fig 1+Fig 2: In the caption of Fig 2 it is detailed what a Sverdrup is. Should this not be in the caption of Fig 1 instead, if it is necessary?

The definition has been moved to fig. 1

Fig 3: Tell in the caption that it is the northward transport. Meridional could, in principle, also be southward.

Done.

Fig 4: Tell in the caption that it is surface air temperature.

Done.

Fig 6: Tell in caption that it is northward. Also in the figure titles (2 places) "tranport"->"transport".

Done.

Fig 9: Is the unit for the ice cover change really kg/m2/s?

Obviously, this is a mistake. Sea ice coverage has no units and ranges between 0 and 1. The legend of the figure was changed. We apologize for this error.

Fig 10: Top right panel (Irminger): "deepening"->"shallowing"

The reviewer is right, this was changed. Sorry for the confusion. (This is now fig. 4)

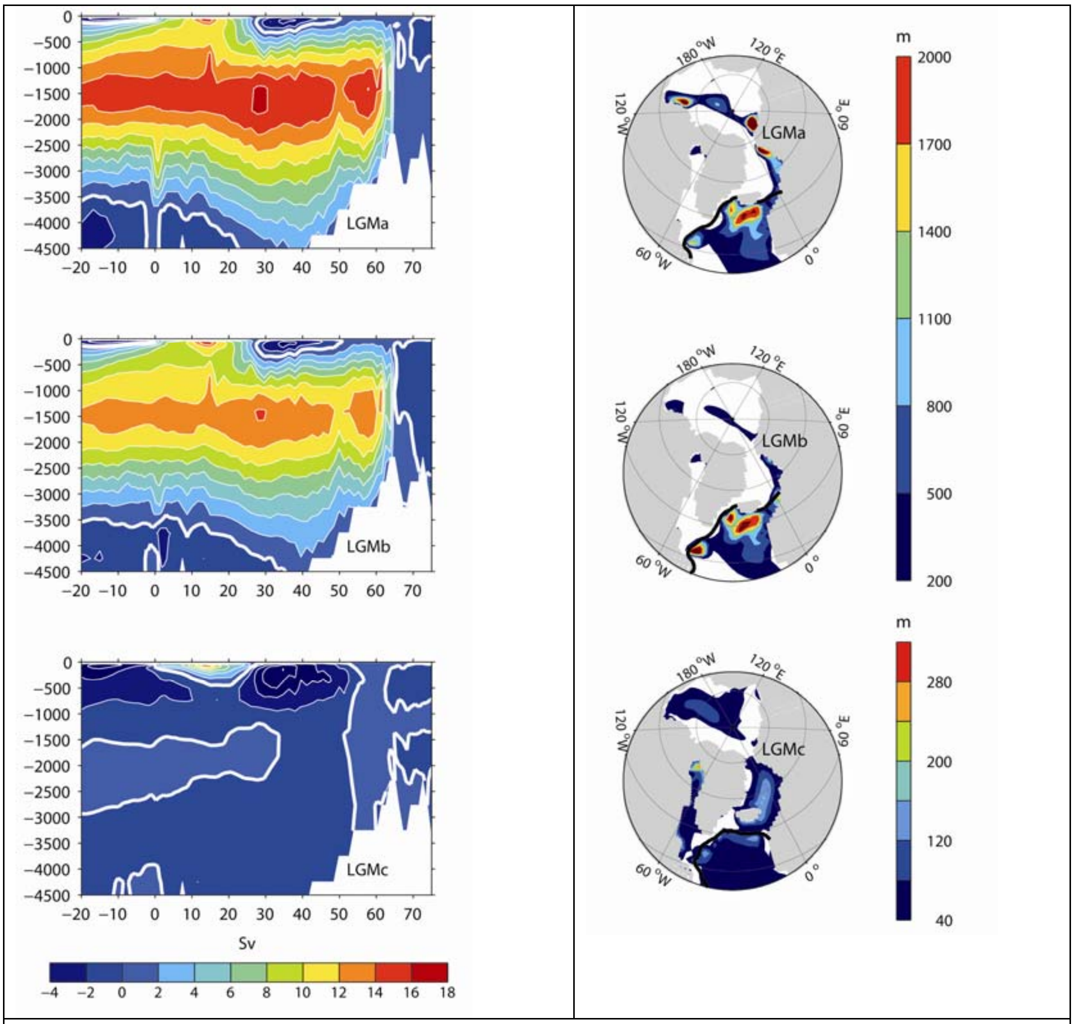
Science

- 1066.22-23+Fig 1: Judging from Fig 1, it could seem that at least the black curve (LGMa) and perhaps the red curve (LGMb) are not entirely equilibrated. There seems to be a downward drift (most clearly in LGMa). I know that it is always easy for a reviewer to ask for more statistics but in this case it would really be nice. Or give at least a discussion of why this apparent drift is unimportant.

Since the writing of the manuscript, we have lengthened LGMb considerably to a total duration of 1000 years. However, we had not chosen to lengthen LGMa. We have lengthened it to year 300 for this reponse. It does seem that the simulations do not converge, and it is certain that LGMb is equilibrated. The longer time-series are now shown on Fig. 1.

- 1066.24-25+Fig1: Many of the following figures are based on years 201-250 in exps LGMa and LGMb and here I see two issues: i) If there is still a drift (at least in LGMa) then this period might not be representative. ii) Given that equilibrium really has been reached, there is still a lot of variability, especially on a timescale of something like 50-100 yrs, and in that respect a 50 yr averaging is maybe on the low side. At the beginning of the averaging period (around year 200) the two curves are on top of each other and at the end they are far from each other. Consider choosing longer and/or different averaging periods or give a better argument why the chosen periods are representative of the two climate states.

We now show longer time series on Fig 1 to prove that LGMb is stable and that LGMa does not merge with LGMb (although for LGMa we have not been able to run the model for much longer due to a lack of computing time). We have repeated some (but, to be honest, not all) of the analyses over years 200-299 of runs LGMa and b. One example is given below for the new figure 2. It did not appear to modify the results. For consistency of all the analyses presented in the manuscript, we have therefore chosen to keep the climate analyses over the 50 year long periods initially chosen.



Same as (new) Fig. 2 but for years 200-299 of simulations LGMa and LGMb

- 1067.5-10: The point about LGMb being close to a threshold is a good one. And it may be made even stronger (or weaker) depending on further statistics. If the black curve in Fig 1 comes even closer to the red one after more years of integration, the point is strengthened by the 0.1 Sv a->b forcing giving an even smaller response while the 0.08 Sv b->c forcing gives a catastrophic response. On the other hand, if the red curve (or both) should somehow collapse after longer integration, the threshold might be in a different range (or not be there if both curves collapse, which I do not expect, though).

Now having a longer time series for LGMb provides part of the answer. It would be too long to lengthen LGMa to the same extent, though, but what is presently available does not show a collapse for this simulation either.

- 1070.9-14: "This timing pleads for a very rapid atmospheric adjustment..." This conclusion comes right after a sentence talking about an Atlantic heat transport decrease over 70 years. How is the former concluded from the latter?

There was indeed a mistake in the ordering of the sentences which has been corrected. The related figure (Fig. 6) has also been clarified (it is now fig. 10).

- 1071.13-14+1076.29-1077.1+1081.6-8: The Northeastern Pacific/Northwestern American LGMb->LGMc warming happens very late (after of the order of 300 years) and is attributed to an atmospheric cyclonic anomaly but I find it hard to follow the reasoning. How can an atmospheric effect take 300 years to become active? Is the atmospheric anomaly a stationary wave response to a Pacific SST anomaly? If so, how is this forced? Is it a far-end effect of changes in the oceanic overturning circulation? I am just puzzled by the time scale of 300 years which seems to be slow for an atmospherically mediated phenomenon and fast for a global ocean phenomenon.

Yes, we believe that it is related to a change in atmospheric circulation but that changes in the surface oceans are needed first to trigger the atmospheric circulation changes. This is now more clearly stated.

- 1073.16-18. How does an increased sea ice cover in the northwestern part of the area lead to a net oceanic freshwater loss downstream? By advection of rejected brine? And exactly in which direction is downstream?

Since the net oceanic freshwater loss appears in the ice to ocean anomalous freshwater budget (fig 3), we argue that it is due simply to brine rejection. This was added to the text. The term "downstream" was misleading here and it was replaced by "further South" in the manuscript (that is in the adjacent ice free area).

-1083.9-11: "The freshwater changes over this region are likely to be more important than those over the tropical Atlantic region south of 4N". Why is that? The freshwater perturbation in the tropical Atlantic is huge. Is this freshwater really that unlikely to be advected to the convection sites?

We have decided to remove the related paragraph from the text. This is indeed a complex question which merits more than a paragraph. The study of the hydrological cycle and the possible feedback associated with it is now announced as a perspective of this work.

Technical corrections

- The term "associated to" is used multiple times throughout the manuscript (1056.4, 1056.12, 1068.8, 1074.11, 1075.5, 1080.10, 1081.29). Being a non-native English speaker myself, I am not entirely certain, but I believe that a more correct use is the term "associated with".

Thank you for the correction.

We thank the reviewer for the following corrections. They have all been implemented.

- 1056.19: "this"->"these"
- 1057.27: "close"->"close to"
- 1060.4: "correspondance"->"correspondence"
- 1062.11: "tends"->"tend"
- 1062.15: "concommittant"->"concomitant"
- 1068.28: "western"->"eastern"
- 1069.1: "northeastern"->"northwestern"
- 1074.20: "variables"->"variable"
- 1076.9: "limitates"->"limits"
- 1081.27: "tropospheric"->"troposphere"
- 1082.4: "response at"->"response is at"