1   Author's response to "Interactive comment on ***Climate and vegetation changes around the Atlantic***

2   ***Ocean resulting from changes in the meridional overturning circulation during deglaciation*** by

3   D. Handiani et al."

4

5   By Handiani et al.

6

7   We thank the anonymous referee #2 for his/her comments, suggestions, and support that greatly
8   improved the manuscript. We modified the manuscript according these suggestions.

9   In the following we address the referee #2 requirements.

10
11   **Response to Anonymous Referee #2**

12   *General comments*

13
14   *The results shown in this paper are valuable. In particular, the fact that no recovery of the AMOC*
15   *occurs under BA-like climatic conditions (large ice sheet, different CO_2 than under present day) is*
16   *important in the context of the Weaver et al. (2003) results, who propose a few attractive*
17   *mechanisms to explain the HE1-BA and YD sequence, using the same model as the one used in the*
18   *present study. Indeed the present results invalidate the Weaver et al. (2003) results by showing that*
19   *neglecting the fact that the climate conditions where different at the time of the BA for testing a few*
20   *hosing simulations can lead to important flaws. Moreover the attempt to use a vegetation model to*
21   *better compare with available proxy is clearly a valuable attempt.*

22   *Nevertheless, in its present form, the manuscript is not well presented and structured and the results*
23   *are not analysed in sufficient depth to provide a clear story concerning what is happening in the*
24   *few simulations presented here. I think the authors should consider continuing the analysis of their*
25   *simulations in order to try to better sort out what are the main achievements from their study.*
26   *Moreover, I would like also to insist on the numerous errors in the reference list (for instance*
27   *Stouffer et al. (2006) should be Stouffer et al. (2007)), among many other errors, cf. Below) and*
28   *advise the authors to be more careful when reading, explaining and citing existing literature.*
29   *Nevertheless, given the nice design of their experiments, I am sure that with a few more analysis*
30   *and reading, this can lead to a very interesting manuscript, which is sadly not the case in its actual*
31   *form. Maybe the authors could consider the possibility of dividing their manuscript into two parts*
32   *or separate papers: one dedicated to the oceanic and climatic analysis of their simulations, the*
33   *other to the comparison with the vegetation reconstruction and simulations.*

34   *If so, it should necessitate to really strengthen each part (cf. below).*

35   Answer**:** As mentioned in our answers to anonymous referee #1, we rewrote the manuscript. We
36   extended the analysis of vegetation changes under BA-like climate conditions and we also
37   improved our literature references. We tried to more clearly separate the oceanic and climatic
38   analysis concatenate from the vegetation simulation and data-model comparison.

39
40   *Specific comments*

41
42   *Title: what is presented is the impact of freshwater input during the last deglaciation. I therefore*
43   *suggest that a more accurate title could be: "Climate and vegetation changes around the Atlantic*
44   *Ocean resulting from freshwater forcing during the last deglaciation"*
45   *p. 2820 l. 19: "shifted northward": add "in the model during BA-like warming"*

p. 2820 l. 21-24: The sentence going from "An equal…" to "…data" is not very clear. I think the authors should clarify what they meant here. In particular, I am wondering what a "transient and robust" change in vegetation cover is.

p. 2820 l. 26: "this time period". Please specify which one? The BA I assume.

p. 2821, l. 19: Driesschaert et al. (2007) concerns projections with the inclusion of ice sheets models. I do not understand why this study is cited when dealing with "exact timing and melt water sources" during the last deglaciation?

p. 2821, l. 21: "The most successful". Can you be a little more specific concerning the criteria you use to assess the most successful attempts?

p. 2821, l. 26: Please specify that the Liu et al. (2009) transient model simulation is made with an AOGCM contrary to the previous studies that you cite.

p. 2822, l. 2-5: In my opinion, this study does not only focus on the vegetation model results and indeed, it is good given the very interesting results obtained from the hosing simulations, so please be more specific or add a "notably" before "focus".

p. 2822, l. 25-28: An important results of the present study is the fact that the Weaver et al. (2003) story based on results from the same model is not valid when you consider correct climatic conditions in the model. This should be clearly stated here I believe since it is an important result.

p. 2823, l. 4-6: The use of a vegetation model is also useful to help comparison with proxy records. I believe you should also stress this here.

p. 2823, l. 16-20: if you're not using the carbon cycle in this study, I do not think it is worth describing the fully coupled carbon model, which is not used here... This is too far from the present subject

p. 2823 l. 21: Please specify what data you use for the wind stress forcing.

p. 2823, l. 24: "based on carbon fluxes", since the carbon cycle is not fully activated, please be more specific concerning these carbon fluxes. What are you using as carbon fluxes to force your vegetation model?

p. 2824, l. 5: "present day". Your simulations are based on 1950 climatic conditions. The GHG concentration have really increased since that time. So it should specified more clearly in the text what "present day" refers to i.e. 1950.

p. 2824, l. 17: "(AAIW)". The importance of this water mass for the Weaver et al. (2003) mechanism notably should be introduced in the introduction. Here it is coming too late.

p. 2824: I think that for the sake of clarity, the authors should consider the idea of making a figure representing their experimental design, with the different amount of freshwater added at the different time period, in the same manner as their Fig. 2, but for the freshwater and insisting on the localization of the hosing.

Answer: We implemented and included the suggestions into the Introduction and Model description and experimental design sections of the revised manuscript (p. 2-7). We also added a new figure (Fig. 2a) representing the experimental design together with the different amount of freshwater forcing at different time periods.

p. 2826, l. 2: The authors here talk about AABW but then shifts towards AAIW and not comment about AABW changes. This should be clarified.

p. 2826, l. 6: "500 years" Do you mean that Fig. 3 shows differences only for one year? I believe this is a bit short. Usually differences between different states are made for at least one decade average.

Answer: We withdrew the statement and all the analysis and discussion now refers to model results time-averaged over the last 100 years of each simulation.

p. 2826, l. 10: you're dealing with AAIW but you have not defined it before and it has not been shown how it is represented in the model. Is there any AAIW production in this model? At which rate? Are their characteristics correct in terms of salinity and temperature?...As said before the interplay between NADW, AABW and AAIW should be clarified as their representation in the present model. For instance, the authors can show the mean density of the their different water masses after providing a definition of each of them (see for instance Fig. 3 from Weaver et al. (2003))

p. 2826, l. 14: It is not clear what is meant by "NADW disappeared", based on Fig. 3 results. Once more it could be useful to give the characteristics of the NADW water mass and evaluate the volume changes in the different experiments following for instance methodology of density binning proposed by Walin (1982), Tzipermann (1986) and improved by Marshall et al. (1999). This will help to better depict the actual changes in the different water masses.

p. 2826, l. 22-26: The authors claimed that the steric height and the AMOC are proportional in their model but do not show any demonstration of it. They should put at least a "(not shown)". Nevertheless I believe it will be even better if thy show this result through a steric height vs AMOC diagram as in Thorpe et al. (2001) for instance.

p. 2827, l. 12-13: "although in different ways". Could you please be more specific and explain what are these different ways. I assume that what is meant here is that the AMOC recovers in PI through freshwater in the South, while in H1_EXT, it recovers through salinification of the North Atlantic, which are indeed very different way. Nevertheless we are left here concerning the mechanisms explaining this similar behavior. Moreover, at the end of this paragraph we still do not understand why the AMOC re- covers in PI and not in GL. This is a pity because it is an important result of the present study. It is certainly the fact that PI and GL have different mean state, but what mechanism play the main role? Is there any AAIW in GL? We see in Fig. 3 that there is no decrease in salinity in the South (around 20°S) in GL while it is the case in PI. Explaining this difference will certainly help to better understand what is happening in the simulations. For this purpose, the author should understand the processes at play after hosing in the South. A few proposition are given in Stouffer et al. (2007) and Swingedouw et al. (2009), which could help. I believe showing maps of SSS changes (helping to understand how the freshwater spread in the different climates) similar to Fig. 5 but for SSS could be really helpful (and at differente time,period to see the spread). My impression is that the hosing in box B does not affect salinity in the South Atlantic, as if the SSS negative anomalies induced in the Southern Ocean do not spread in the Atlantic Ocean. Reasons for that should be clarified. Has the ACC and the main oceanic pathway been highly modified between PI and GL?

p. 2827, l. 14-15: Fig. 3 shows differences T1-T0 as it is the case for Fig. 5. Here the text states the opposite (T0-T1) please clarify. p.

2827, l. 18-19: It is really surprising that SST (please state "SST" in place of "temperature", which is more specific) decreases both in the North and South Atlantic. For instance, when comparing with Fig. 4 of Weaver et al. (2003), we find a very different pattern. This should be commented and an explanation or hypothesis should be given. Is it an effect of the average over only one year (please use longer time average for all your figures)? Is it because of the inertia of the system? Given that the AMOC recovers in PI, we were waiting for a warming in the North Atlantic...Nevertheless the latitudinal gradient is clearly modified given the large modifications observed in the ITCZ. Please explain why the North Atlantic is still cooler in the PI, while it is surprisingly not the case in H1... We can also wonder why the effect is so small in GL all over the world?

p. 2828, l. 6: Given the model is simplified, I think it is useful to discuss how it represents the ITCZ and its dynamics.

p. 2828, l. 14: "This precipitation pattern was stronger". How do you define your pattern? How do you estimate that the pattern becomes "stronger"? This should be rephrased or better quantified.

1   *p. 2830: This part should be developed and improved I believe. Fig. 8 shows the computed biomes*
2   *in the model, but (i) it is very hard to compare them with the reconstruction from Fig. 1.b. (maybe*
3   *the authors should superimposed the reconstructed biome on the Fig. 8) and (ii) it is hard to see*
4   *any differences in the different climatic conditions. Maybe there are no differences. If so it should*
5   *be stated clearly. In that case, it is as if the AMOC recovers or not have very little impact on the*
6   *biomes distribution. Maybe it is once more an effect of the time chosen (T1). What about T2? In*
7   *general, given the complex experimental design, a few more words should be given on the temporal*
8   *evolutions of the biome changes. This would be indeed very interesting to also evaluate the*
9   *transient behavior of the vegetation as well as their characteristics time response and inertia.*

10 Answer: These comments and suggestions above refer to the Results section of the old version of
11 the manuscript, which were rewritten. Many statements and figures were withdrawn or changed;
12 Fig. 3 and Fig. 4 were withdrawn and Fig. 5 was changed. Fig. 6 now shows the difference in the
13 ocean component (sea-surface temperature and salinity) and in precipitation between time T2 and
14 T0 (as suggested by both reviewers). We commented on the small differences in the biome
15 reconstructions of H1 and H1_EXT and adapted the previous Fig. 8, now Fig. 9.

16

17 *p. 2831, l. 3: "Swingedouw et al. 2009", as cited in the reference list, is dealing with the sensitivity*
18 *of the response of AMOC to freshwater input in the North Atlantic under different climatic*
19 *conditions. This is indeed an interesting reference to compare with your experimental design that*
20 *also deals with different climates (cf. p. 2832, l. 2-5). Nevertheless in the present sentence, I believe*
21 *the authors rather refer to Swingedouw et al. (2009b, cf. Bibliography) who deal with the effect of*
22 *freshwater input in the Southern Ocean.*

23 *p. 2831, l. 7: We need an explanation for this very interesting result.*

24 *p. 2831, l. 16. Maybe you can add: ", thus questioning the Weaver et al. (2003) mechanism to*
25 *explain the BA." after "slightly"*

26 *p. 2831, l. 24: "This probably..." It would be better to try to prove it in the manuscript with the help*
27 *of additional diagnostics in spite of only hypothesising it.*

28 *p. 2832, l. 5: You could also cite here Hu et al. (2008). Note sure that Kageyama et al. (2009) really*
29 *deals with the question of different AMOC sensitivity under different climate states, but rather*
30 *Swingedouw et al. 2009a.*

31 *p. 2832, l. 14-25: This discussion is interesting but in my opinion, you should introduce that in the*
32 *introduction and then test the mechanism in your simulations. You state that you are observing*
33 *similar mechanisms in your simulations, but this is not clearly shown. In particular no SSS maps*
34 *are provided to see any SSS negative anomalies spread (cf. former comments). Please re-organise.*

35 *p. 2833, l. 5-14: Here once more, we have an interesting discussion on existing literature and the*
36 *authors claim to have similar results, but they do not show them before! I have not seen any figures*
37 *of freshwater transport before. I believe that here the authors propose an interesting analysis,*
38 *which they should perform before to discuss it. Once more, the author should consider to give a*
39 *flavour of existing literature in their introduction and then discuss it in details on the basis of the*
40 *results shown, which is not the case here.*

41 Answer: We cited appropriate references and improved the literature review. We implemented the
42 suggestions, e.g., by showing the sea-surface salinity map in Fig. 6 and adding new references. We
43 also rewrote the Discussion section. The following points had been changed in the new version:

44 • The model shows that under glacial climate conditions the AMOC is less susceptible to recovery
45   by adding freshwater to the Southern Ocean than under interglacial conditions. Under these
46   conditions, NADW is less dense, AAIW is denser and the density difference between NADW
47   and AAIW is larger than under interglacial climate conditions (all densities refer to the surface
48   potential densities in the source regions).

- After recovery of the AMOC, the climate of the North Atlantic Ocean is warmer and wetter than that of the South Atlantic Ocean.
- The model shows that the regions of sub-tropical and equatorial North Africa are most sensitive to climate changes in the North Atlantic Ocean. The vegetation cover in those regions mostly increased due to increased precipitation over northern Africa.
- The results show that both locality and intensity of the vegetation response in northern Africa is different in the two recovery experiments.
- Biome reconstructions from our results indicate agreement with pollen records from western tropical Africa, southwest Europe and the Mediterranean.

p. 2837, l. 7-8: "reduction of iceberg calving". Earlier in the manuscript you rather state an increase in evaporation. It is the conclusion part, so you need to be coherent with what you state before. By the way, I believe it could be interesting to translate your freshwater forcing in the box C in terms of evaporation increase in mm/day, in order to see if it could be realistic as compared to present day estimates of evaporation. Please clarify what is your main hypothesis for a salinification of the box C: reduction of calving or increase of evaporation (or both of them, but try to estimate what is the most plausible, in the discussion section for instance).

Answer: We withdrew the statement and implemented the suggestions (p. 16, lines 26–33 and p.17, lines 1-12).

Technical comments

p. 2821 l. 16: no "-" in "sea level"

p. 2828, l. 4: replace "was generally higher" by "increased"

Answer: We withdrew the statements.