

Interactive comment on “

Climate response to freshwater perturbations in Northern or Southern Hemispheres at the last glacial inception, the last glacial maximum and the present-day” by G. Philippon-Berthier et al.

Anonymous Referee #1

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Major comments

1. I am sorry to say that the paper was very hard to read; sometimes sentences are incomplete; the structure goes back and forth and frequently missing a straight line. I will provide some of these shortcomings in my “minor comments” but the list is not complete. I am sorry to say that the writing was a major obstacle for me to focus on the

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content. This is a pity, because at least two of the co-authors are senior researchers and I think it is not proper to transfer the task of making the paper legible to the reviewers. I am willing to give more in depth comments on the results once the paper has been revised. I therefore expect another round of major revisions. 2. The authors present the theory of Dansgaard-Oeschger events based on shifts in the Atlantic meridional overturning circulation as fact. I have no objections to the theory, but feel that the language on this is too strong through-out the paper. It should be presented as a well-based theory, not a fact. Facts are for example the observed warming in Greenland not it potential cause. 3. The authors discuss one of two possible responses of a freshwater perturbation to the North Atlantic. There is a second possibility (Mignot et al., 2007, J Clim.) that needs to be discussed. Because the mechanism is fundamental to most of the results in the paper. The associated difference between the two options in the subsurface ocean temperature might be very important for the dynamics of ice sheets and marine glaciers. 4. Since the ocean is one of two major components that are crucial for this study it needs to be clearly stated that it is two-dimensional and does not resolve the zonal direction. This information is of course available in the cited papers, but needs to be stated here for its potential importance for the results. 5. 3 of the 6 figures show the rather simple and boring freshwater/salinity forcing. While these could be combined in one figure with two panels. The opposite might be better for figure 3 which I can hardly read even in the biggest display mode on my screen. Perhaps displaying the figures below each other or given them separate figures would be appropriate? This is particularly important because for the DO-events the details on the overshoot matter. Grey might not be a good choice for the second hysteresis curve in fig. 3. 6. I think that the notion “PD-THC is more stable than LGM-THC” is a bit problematic. The reason is two-fold. First, what is actually meant here is that for the LGM the THC is closer to the threshold than in PD. It is much clearer to say that than statement chose because in fact the THC is unstable in the same way in both situations, i.e. it shows a threshold behaviour. Second, we do not know at all how far we currently are from the threshold. The reason is that we can not estimate the freshwater

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flux distribution nor the contribution by the ocean to the North Atlantic salinity budget with a precision that would be required for that. There is thus no ground for the assumption that the models presented here can get these number quantitatively right. It is a fair approach to use them for the qualitative behaviour and it also fair to make statements about the distance from the threshold WITHIN the model, but there is really no ground for the statement “PD-THC is more stable than LGM-THC” which suggests that we know from this study anything quantitatively about the distance from the threshold, let alone a comparison of this distance between the PD and LGM. This can be easily changed and should be. The same holds for the comparison of the interglacials. 7. I think the paper would gain from more process-oriented analysis of the model simulations. The results section is very descriptive. With all these simulation results available it should be possible to further understand the results on a process basis rather than only describing the outcome of the (very well designed) experiments. 8. I think the title of the paper should rather refer to the role of ice-sheet-climate feedback in freshwater flux perturbation experiments. For me this was the most interesting outcome of the paper (and it is stated so in the abstract) compared to the fact that simulations under different boundary conditions where conducted.

Incomplete list of minor comments:

Page 1078 Line 25: This was already suggested by Flueckinger et al PAO, 2006 and Mignot et al JoC, 2007 before.

P 1079 L 1: Not a good conceptual idea. It is by now clear that deep water formation is a limiting process for the Atlantic meridional overturning circulation and thereby heat transport, not a driver in the sense of energy providing process. L 5: Stocker 1998. This is a News&Views not a paper. L 5 – 11: I do not understand the difference between these two mechanisms. L 15: cite the intercomparison by Rahmstorf et al. 2005 here? L 22: “.. is non-linear” As an example for my major comment #2, this should be formulated more carefully. It might be so, but perhaps it is not. L 26: “. . . is less stable” Again this is stated to strongly. It is not yet clear whether this is the case.

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P 1080: L 2: I do not think this is a sentence. L 4: In my opinion, this is not the correct term. Phase space has a clear definition in theoretical physics which is not the one used here. I am not aware of another definition that would allow the usage here. L 11: Please, specify the time scale. L 14: “. . .because. . .” Is this really a causal link? L 24: I think a schematic of all these possible feedbacks would be very helpful for the reader.

P 1081: L 21: I have not seen the acronym AOV before and it is not used in the Petoukhov paper. The later reference to CLIMBER-2 also confused me. Perhaps it is better to call it CLIMBER-2 from the start?

P 1082: L 4: I do not understand what the resolutions refer to. L 5: “coupling aspect” should become “coupling”. L 8: “coupling procedure” should become “coupling”.

P 1083: L 7: How long was the integration time? L 20: Does this region cover all convection sites? L 25: In my experience the AABW is not equilibrated in 1000years. These kind of strong statements make me suspicious about other statements that I can not check myself. There is no need for these kind of strong statements. L 27: “minima” should become “extrema”?

P 1084: L 9: This is not a sentence as far as I can see. L 9: Please put the 20m sea level equivalent in perspective of the reconstructed sea level changes. L 13: See comment on “phase space”.

P 1085: L 2: cross out “alone”. L 3: I found no reference to figures 1 and 2. See major comment on figure 3.

PLEASE CHECK THE REST OF THE TEXT FOR SIMILAR ISSUES PRIOR TO SUBMISSION!!!

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