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Climate of the Past Discussions

Interactive comment on "Glacial climate sensitivity to different states of the Atlantic Meridional Overturning Circulation: results from the IPSL model" *by* M. Kageyama et al.

Anonymous Referee #2

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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 fresh-water 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.

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

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

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events are. Of course, most readers know the difference and the background, but if this is assumed, the above references are in principle unnecessary.

- 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.

- 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?

- 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.

- 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?

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

- 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.

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

-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?

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

- 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.

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?

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

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

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

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

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

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.

- 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

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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.

- 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).

- 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?

- 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.

- 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?

-1083.9-11: "The freshwater changes over this region are likely to be more important

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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?

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".

- 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"

Interactive comment on Clim. Past Discuss., 5, 1055, 2009.