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Climate of the Past
Discussions

Interactive comment on "Impact of Southern Ocean surface conditions on deep ocean circulation at the LGM: a model analysis" *by* Fanny Lhardy et al.

Anonymous Referee #1

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This paper considers a series of simulations using the iLOVECLIM model to analyze the effect of different choices of LGM boundary conditions, parameterizations, and ad-hoc modifications (to wind stress and freshwater forcing) on the LGM solution, focussing on the surface climate as well as the deep ocean overturning. Although the results are generally interesting and worthy of discussion, I think the comparison between these experiments and conclusions about which "choices" are better or more important then others need to be drawn a bit more carefully, as these various modifications aren't really comparable and some are certainly more physical then others. The paper specifically highlights the use of a parameterization to represent "sinking brines" as key to obtaining a realistic simulation of the LGM overturning circulation. However,

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Discussion paper

I believe that the discussion of these results needs improvement, as elaborated below. Most importantly, I think that a PI simulations with this parameterization needs to be presented for comparison. Since the physics of the ocean have not changed between the present and LGM, the model needs to be able to reproduce the PI ocean and LGM ocean circulation with the same parameterization.

Specific comments:

- l. 15/16 in abstract. It would be good to clarify that you are referring to "different choices for LGM boundary conditions", or better yet "different choices for the LGM ice sheet topography" (see also comment below). After all, differences in boundary conditions between the PI and LGM are ultimately what has to explain the different circulation in the two climates.

- In various places (e.g. l. 36-38) the issue of open ocean convection versus sinking along the AA slope seems to be used almost synonymously to the role of brine rejection in deep water formation, but these are rather different processes. Notably, the CCSM3 LGM simulation shows very salty AABW, clearly as a result of strong brine rejection, yet I assume AABW is still formed by open ocean convection (e.g. Shin et al. 2003, https://doi.org/10.1029/2002GL015513 - indeed this paper should be discussed).

- Relatedly, the parameterization of sinking brines needs to be described a little more– both in terms of the formulation and its physical interpretation. I understand that this method has been published previously, but since it is key to the presented conclusion I think the reader needs to be able to interpret the results from these simulations without first reading Bouttes et al. (2010). In l. 125 it is argued that "its objective is to account for the sinking of dense water along the Antarctic continental slope", and similarly in l. 301 it is refereed to as "the parameterization of the sinking of dense water along the continental slope". This gives the impression that it may be a parameterization of down-slopes gravity currents, which, however, seems quite misleading. Indeed, if I understand correctly, the parameterization simply transfers salt from brine rejection directly and locally to the bottom of the ocean (without any mixing along the way), and it is not limited to the Antarctic slope. Personally, I'll have to admit that this parameterization seems rather unphysical to me (even gravity currents are associated with lots of entrainment and detrainment as they proceed down the slope, and of course they only exist on the slope). The readers can form their own opinion, but to do so, the parameterization needs to be discussed clearly.

- As discussed in the general comments above, the "P4-I brines" simulation needs to be compared to a corresponding PI simulation with the same parameterization. Ultimately the changes in the ocean circulation between the PI and LGM climates have to be attributable to differences in the boundary conditions, not different physics. It needs to be verified that the model is able to reproduce a reasonable solution for both the LGM and PI ocean with the same parameterzation. (One aspect that should be paid attention to here are the T and S properties of NADW and AABW. Importantly, AABW is fresher than NADW in the modern climate, which needs to be reproduced by any model that adequately represents watermass transformation processes in the SO.)

- in l. 131/132 it is argued that a quasi-equilibrium state is ensured. How is this evaluated?

- l.161: does "surface extent" here refer to sea ice extent or still the surface of the continent? I assume the former, but please clarify.

- The error bars of 10% and 20% for winter and summer sea ice extent seem to be mostly accounting for the uncertainty in the continental margins, which is probably relatively small compared to the large uncertainty in the sea ice line. As a result, these error estimates seem very optimistic to me. Indeed this seems to be confirmed by the fact that the previous estimates of Gersonde at al. (2005) and Roche et al. (2012) fall significantly outside of this error bar. I'm not arguing that either estimate is better or worse, but simply that a larger uncertainty has to be acknowledged. I think the uncertainty range should at least encapsulate the best estimates of these major previous studies.

- l. 220: It is argued that "the transfer of brines leads to a cooling of the Southern Ocean". Notably, however, the cooling does not occur in the regions of AABW formation but further north. There also is pronounced warming (relative to P4-I) in the North Atlantic. Do you have an explanation for these results? And does the warmer North Atlantic play a role in explaining the relatively weak and shallow AMOC in this simulation? The focus here seems to be almost entirely on the Southern Ocean, but what's the effect of the brine parameterization in the North Atlantic?

- l. 214: what is the statement that "'Cold P2' is not the simulation with the best overall agreement" based on? From what is shown in the paper, it seems to at least show among the best agreement in terms of SSTs. (And believing the Tierney et al. (2020) estimate it would also be the best in terms of global mean temperature.)

- Fig. 7: What exactly is plotted here? Is it only the resolved Eulerian mean overturning or does this include the parameterized eddy transport associated with the GM parameterization? What matters for the transport of physical and geochemical tracers is really the isopycnal overturning (which probably does not have two counter-clockwise cells in SO). Computing the latter is admittedly more challenging and not commonly done in studies like this, but at least the GM contribution should be included.

- From Fig. 7, it also seems that the AMOC in the PI simulation is too weak and too shallow, which should be discussed.

- For the evaluation of deep ocean water mass properties it would be very useful to show T and S (as a function of latitude and depth).

- l. 303-305: I don't follow the argument here about why 'P4-I wind' has a stronger Southern Ocean cell. My guess would be that the stronger wind stress over ice leads to enhanced ice export, which in turn leads to more new sea ice formation and thus brine rejection (c.f. Shin et al 2003).

- In Fig. 8 and throughout much of the manuscript "convection" seems to be used synonymously to the large-scale overturning circulation. However, convection can occur without a large-scale overturning and vice versa. I suggest to replace all references to convection cells with overturning cels.

- In section 4.1 the various simulations are separated into those that amount to different choices for "boundary conditions" and "experimental setting", a separation that makes its way also into the abstract and conclusions. This separation, and the term "experimental setting" seems very vague. E.g. the assumed glacial temperature profile affects the simulation results via heat flux in or out of the glacier surface, and thus effectively also amounts to a difference in boundary conditions. In general it seems that "boundary conditions" is only used for cases with different choices for ice sheet topography, so I suggest to simply be explicit about that. As for the various other experiments, I don't see how they can be lumped into one category.

- Section 4.3.: Given the high uncertainty, particularly in the reconstructions of summer sea ice cover, I think it would be useful to provide some estimate of uncertainty for the sea ice seasonality from proxy data.

- I find the last paragraph of section 4.3 and specifically the attempt to reconcile the conflicting results between this study and Heuze et al. (2013) hard to follow and it seems very speculative. I don't think this discussion is necessary either, so I suggest removing this paragraph.

- Based on the issues pointed out above, I'm not sure the last sentence of the conclusion can be justified. At the least, "boundary conditions, such as the ice sheet reconstruction" should be reduced to just "the ice sheet reconstruction".

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