

Interactive comment on “Low-frequency oscillations of the Atlantic Ocean meridional overturning circulation in a coupled climate model” by M. Schulz et al.

Anonymous Referee #2

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Review of Schulz et al., Low frequency oscillations of the Atlantic Ocean meridional overturning circulation in a coupled model

The authors present an interesting new mechanism to explain centennial to millennial scale climate shifts in the North Atlantic region during the Holocene using an intermediate complexity coupled model. The mechanism is based two different states in the Labrador Sea, one with ocean convection and one without, between which the climate can switch, triggered only by internal variability.

I very much enjoyed reading this manuscript. The authors have done a very thorough job in analyzing the oscillations in the coupled model, in finding the process by how

the signals propagate, and they nicely describe the timescales and patterns. They even describe a conceptual model that shows many of the characteristics of the coupled system. The experiments are selected in an intelligent way to answer the relevant questions, and the steps are clearly described. The results are novel, and interesting with regard to the question of whether the oscillations seen in some paleoclimate archives can be interpreted in a climatic sense. There is little to criticise about this manuscript, and I highly recommend acceptance with minor revisions.

Details: The authors mention that the oscillation they describe is different from those found in simpler models, e.g. Ganopolski et al. Nature 2001, or the deep decoupling oscillations. Yet they don't explicitly state that what they see in the model is unlikely to be an alternative mechanism to explain the large abrupt events during the glacial period. I think it would help to make it clearer that the amplitudes are much smaller here, despite the fact that the shifts could be triggered in a very similar way to what was described in the Climber2 model. It seems like a shift in convection is not sufficient to explain the large amplitude and extent of the events seen during glacial times.

The oscillation in that model is only seen in a very narrow range of constant boundary conditions. A few milli-Sverdrups of freshwater are sufficient to go from the bistable to a monostable regime. This again is similar to what was found by Ganopolski et al. Nature 2001, and Knutti and Stocker J. Climate 2002, and is not surprising. Instabilities tend to be located only in a small area of the parameter space. This raises some questions as to whether such a mechanism can account for climate shifts found in proxies over many thousand years. The boundary conditions have changed over the Holocene, e.g. insolation, greenhouse gases, ice sheets, vegetation, etc., and those changes were likely much larger than what seems to be needed to push the model out of the bistable regime, which would make such a process unlikely as an explanation for climate events over such a long time. I wonder whether the authors would like to comment on this. One way to test this would for example be to redo the experiment with mid Holocene insolation and greenhouse gas concentration instead of preindustrial, to see whether

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the oscillation is still possible, and what freshwater perturbation is needed.

Related to that, the authors speculate at the end of the manuscript that the AMOC might get into a bistable regime through Greenland melting in the future. I think this is stretching it a bit far. First, the location of such oscillatory regimes tend to be strongly model dependent. Of the about 20 AOGCMs that were used for upcoming IPCC report (data archive at PCMDI), none showed anything that looks like an oscillation. This should at least be mentioned. Second, related to the comment above, with future warming there will be strong changes in the boundary conditions in addition to Greenland melting, i.e. a strong warming, increased high latitude precipitation, sea ice retreat, changes in runoff, etc. The melting from Greenland would also not be constant over time. In my view, even if the model could briefly come into an oscillatory regime, it would likely leave that regime soon again because many other things in the climate system are changing.

There are at least two papers that I'm aware of that might be mentioned with regard to future changes in convection sites, one by Schaeffer et al. (GRL 2002), and one by Wood et al. (Nature 1999). The latter shows that Labrador convection might stop due to future warming (consistent with what is hypothesized here), but the short simulations do not allow to test whether an oscillatory regime is possible.

Figures: Labels are too small in most figures Fig. 3: The color scale is not very good in distinguishing the different blues in particular.

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