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Climate of the Past Discussions

Interactive comment on "The global ocean circulation on a retrograde rotating earth" *by* V. Kamphuis et al.

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The manuscript presents model results of the ocean's overturning circulation in the case of a reversed rotation direction. Results from the CCSM3 fully coupled ocean-atmosphere climate model as well as an ocean only model used for stability analysis. I like the paper. It contributes to a better understanding of the processes that lead to the global MOC and I think that it is potentially publishable given that the following concerns are addressed.

Major: I think the conclusion reached by the authors that view (i) is not correct is not supported by the results. In fact CCSM3 shows something like an inverse conveyor. Certainly the statements in the abstract and on page 2465 line 13 that "there is strong . . . deep water formation in the North Atlantic" is inconsistent with the results shown

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in Figure 4c, which shows very weak deep water formation and only 2 Sv of deep water export to the Southern hemisphere, in contrast to about 12+ Sv estimated for the modern. I think these statements need to be changed.

Also the statement in the abstract that "These results demonstrate that the present-day asymmetry in surface freshwater flux is not a crucial factor for the Atlantic-Pacific asymmetry in the global MOC" is not supported by the results and should be rephrased. Figure 4 shows that it indeed is important in determining the Atlantic-Pacific asymmetry.

I recommend reading Schmittner et al. (Effects of Mountains and Ice Sheets on Global Ocean Circulation, in press J. Climate, http://journals.ametsoc.org/doi/abs/10.1175/2010JCLI3982.1) who come to a different conclusion concerning the development of an "Inverse Conveyor" state if they remove mountains and ice sheets.

I think the analysis of the CCSM3 results (Section 3) in general and the analysis of the freshwater budget of the Atlantic and Pacific in particular should be more quantitative. E.g. it would be useful to compute the basinwide budgets. Also river runoff should be included in Fig 7 and elsewhere.

Also, I think it would be useful to look at the changes in the Southern Ocean. The reversal of the mid-latitude winds will lead to changes in the "Agulhas leakage", the salt advection from the Indian ocean (Sijp & England 2009. J. Clim 22, 1277). Is the upwelling changed?

The results of the implicit ocean model prompt some questions. First, I think it is a matter of concern that the model does not reproduce the stronger deep water formation and MOC in the North Pacific found in CCSM3. This discrepancy needs to be discussed. Second, the model seems to be highly diffusive as suggested by the strong upwelling at low latitudes mentioned already. These problems make the results of the stability analysis highly questionable to me. I think this should be made clear to the reader.

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Minor: Page 2457 lines 17-22 add references

Lines 22-29: see Schmittner et al. (in press) for a discussion of this. In their model the E-P difference is set up initially by precipitation differences but at steady state E becomes more important as the circulation differences develop

Fig 2-3 note if long term averages are plotted

Page 2463 Lines 17-18: the North Pacific is NOT saltier in RETRO than in PRO in Fig. 3c. Please correct this sentence.

Fig 4. Include bathymetry and cut plot at 40 S.

Page 2464 The winter sea ice extending further southward in the eastern North Atlantic is also a concequence of the land sea contrast (Cold air in winter blowing from the Eurasian continent over the Atlantic) and not only due to the weaker MOC.

Lines 14-24: I'm not sure the separation into stable and convective precipitation makes much sense. I don't agree with the statement that "Stable precipitation is . . . independent of local sea surface temperatures and thereby the MOC itself". The hydrological cycle is strongly coupled. If evaporation depends on the MOC than there will be more water vapor in the atmosphere and more of it has to rain out again. The ocean feels the total precipitation not just the stable part of it, which BTW is a model construct.

I suggest to remove Fig. 5, which does not add much to the discussion.

Interactive comment on Clim. Past Discuss., 6, 2455, 2010.