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Earth System Dynamics Discussions

Interactive comment on "Mechanism for potential strengthening of Atlantic overturning prior to collapse" *by* D. Ehlert and A. Levermann

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Review of Mechanism for potential strengthening of Atlantic overturning prior to collapse by Dana Ehlert and Anders Levermann.

Based on the conceptual model by Fuerst and Levermann (2012), the authors develop a model with an additional degree of freedom by the inclusion of a parameterisation of SO eddies. This allows a new type of behaviour, where the steady state AMOC can increase with increasing FW flux into the North Atlantic, all the way to the bifurcation point. The main effect found is that fresh water-induced MOC strengthening in response to a fresh water flux from low latitudes to high northern latitudes could take place in the wind-driven case. By continuity, due to a resulting reduction in eddy return flow m_e arising from a denser low latitude box and subsequent reduction in density difference between the SO and the low latitude box, northern sinking must compensate in the conceptual model. (Unlike Gnanadesikan 1999, the authors parameterize m_e in terms of this density difference in Eq. 4.)

The following main comments are mostly limited to the wind-driven case.

I recommend that the authors look for this MOC enhancement effect in a numerical ocean or climate model as this is not very difficult to do (I realize that the paper points to future work to be done in this regard). The conceptual parameterization is based on GM, and many models use this parameterization: the effect should show up if it exists by programming the FW flux in the way envisioned in this study (I imagine using low vertical diffusivity). Based on many assumptions and parameter choices, the assertions made in the paper seem to be a stretch and in need of additional ways of illustration and validation.

There are (too?) many parameters in the conceptual model. How physical are the parameters chosen for the eddy return flow term? How sensitive is the solution to these values? Towards the end of the paper, the authors appear to state the purpose of the paper as showing only that this type of effect (strengthening MOC with FW flux in wind-driven case) is possible in reality and/ or a climate model, rather than showing that it actually exists. This might be an appropriate scope for the material covered so far in this paper, but that then also severely limits the weight carried by this study. More could be said about the likelihood of the main result to be real by conducting a parameter sensitivity study.

A continuity argument is invoked to explain the MOC increase (p41, l25). Although strictly true (from eq. 5 and steady state), it would be helpful to state that in addition to satisfying continuity, M_N $\sim$ Dˆ2 delta rho here (referring to Eq. 1) and that this must also be satisfied. As a result, the system must adjust in a very specific way to allow a steady solution. So only solutions where D increases (in the right way) with the FW flux are consistent with continuity and have a chance of being allowed. The paper could benefit from more physical explanations of the mathematics in general.

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The validity of the main result appears to depend on how valid the GM parameterisation is for the real ocean. There are of course more complex parameterisations, and there are also results from eddy resolving and permitting models. The paper should contain some discussion of how the results might depend on GM specifically, using existing literature.

The definition and significance of terms like “bistability” and “threshold” is not very clear in the paper. Bistability should refer to a situation where two steady states exist under the same forcing, whereas by “threshold” I think the authors mean a FW flux above which no northern sinking occurs. This should be spelled out more in the paper, and something should also be said about the circulation when northern sinking is absent. Any negative strength overturning states should also be interpreted.

The results depend on a very specific spatial pattern of moisture transport changes, namely an increase of FW transport from the low to the high northern latitude box. How do the authors expect the overturning to change under more general moisture flux changes? For instance, a freshening of the SO could negate this effect?

I don’t understand (immediately) why the threshold is reached when the eddy return flow becomes negative (Fig. 5), or whether this is physical. What does this change in sign mean? Why is the threshold reached there? Is this physical or an artifact? This should be explained clearly.

The work is only valid for steady states. This makes the conclusions less relevant to any global warming scenarios (they generally are not in steady state), even though the paper alludes to “monitoring activities” in the abstract. This should be stated.

More specific comments:

The density difference between low and high northern latitudes sets the overturning strength. However, studies have shown the importance of the density difference between the Southern Ocean and the North Atlantic (Saenko and weaver, GRL 2003).

How important is this choice in the context of this paper?

p32. l1. “”it was shown that this kind of model. . .” This sentence is not clear, it would be good to first explain that Gnadesikan 1999 does not appear to be concerned with the role of the density difference, that it is treated as a constant there.

p32. l21.”The threshold behaviour found here is consistent with the salt-advection feedback in. . .”. These sentences are not clear. What is meant by “net salinity transport by the overturning”? Does it mean the overturning exports fresh water from the Atlantic basin?

p32. l24. Reference to Huisman 2010. This paper only deals with a Rahmstorf type box model and a fully implicit global ocean model, not “a number of climate models” as stated in the text here. Also, unlike what is implied in the paper here, dynamics are not examined for observations in Huisman 2010.

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