Detailed response to the reviewer of the manuscript

"Mechanismfor potential strengthening of Atlantic overturning prior to collapse"

by D.Ehlert and A. Levermann

Referee #2

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.)
Response: We would like to thank the reviewer for taking the time to review this manuscript and the positive review. The insightful comments have improved the quality of this paper, especially in regard to bringing a more applied background to this theoretical approach. We are positive about being able to address all of the stated comments.

Main comments:

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.

Response: We agree with the reviewer and performed simulations with freshwater forcing using the University of Victoria Earth System Climate Model, version 2.9 (Uvic ESCM 2.9). A brief description of Uvic ESCM, the simulations and their results have been added to the manuscript (see section 5).

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.

Response: We understand the reviewer's desire to bring our results closer to the real ocean. We have tried to motivate the choice of the parameters as good as we can in the paper. More importantly, we have provided an explicit condition on the parameters for which the main feature of the manuscript holds. Since the model presented here is however only conceptual in nature, we feel that we cannot push it any further than this. That is especially true because (like in the Stommel model) the meridional density differences require the definition of boxes in the ocean which are not very well constraint. We would be really grateful if the reviewer would allow us the mere qualitative statements that we are making in the text which we hope can make some contribution due to the analytic conditions that we provide.

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 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.

Response: We agree with the reviewer and have added to the manuscript that the qualitative result does depend on the assumption that M_N increases with D2 and Delta rho and that thereby only solutions where D increases with the freshwater flux are allowed. We have further added the physical interpretation that the meridional density differences are the main driver of the changes both in M_N and M_E while the vertical density changes (as represented by changes in the pycnocline depth D) play an important role in stabilizing the circulation (as mentioned for the model without Southern Ocean density difference in Fürst & Levermann, Clim. Dyn.).

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.

Response: The original parameterization of Gent and McWilliams is very well motivated the baroclinic instability and provides a very clear and qualitative large-scale representation of the eddy flux across a sloping density front. The wealth of adjustments that have been suggested to this formula might be very valid in specific situation and it is not our desire to dismiss any of this work. However, there must be about 1000 studies published on different variations of the GM parameterizations. For the current paper it is the representation of the slope by both a horizontal (meridional) and vertical measure for the density structure, we find that a serious discussion of the various variations of the GM-parameterization would require a lot of additions to the manuscript and any reduced discussion would be unfair to those left out. We would again be grateful if we could omit this or would be grateful if the reviewer would suggest specific issues that he or she considers particularly important. We are happy to include them. It is just that we feel that a comprehensive coverage of the matter would expand the paper a lot.

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.

Response: This is an important shortcoming of our description and we would like to thank the reviewer for pointing this out. There have been a number of studies and speculations about the circulation in the absence of northern sinking. Some models show an inverse circulation, which is sometimes associated with the Antarctic Bottom Water filling up the Atlantic (Rahmstorf et al. GRL, 2005) and some show a seemingly stagnant ocean (Stouffer et al. J Clim. 2005). Neither of these patterns would be properly captured by the physics that is incorporated in the conceptual model that we present. It is thus true that we cannot really describe a bistable situation but rather a threshold behaviour that describes that beyond a certain freshwater flux the circulation in the Atlantic cannot be captured by the conceptual model and is thereby not a classic overturning circulation as presently observed. We have now added a brief explanation to this end stating that the conceptual model does not capture the “off-state”. Furthermore we have exchanged the term bistability with threshold behaviour and cleaned the text in this respect.

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?

Response: The application of freshwater to the North Atlantic and infact a number of different places in the global ocean has been under intense investigation since the groundbreaking papers by Manabe and Stouffer starting in 1988. The behaviour of our conceptual model is rather straight forward when it comes to the “normal” addition of freshwater into the north Atlantic which is then compensated in the Southern Ocean. The really new issue in our model is the fact that the overturning **can** get stronger prior to collapse if the net-freshwater transport occurs between the high and low latitudes in the Atlantic. While we would be reluctant to discuss these “normal” freshwater forcings in the paper because we believe it would dilute the content of the study, we have studied the low-to-high-latitude case in detail: We have carried out experiments with freshwater forcing from the lower latitudes into the southern ocean and freshwater forcing from lower latitudes towards both higher southern and northern latitudes. All experiments lead to the same behavior in the overturning: there is an increased overturning prior to m_E becoming negative. In all cases, this can be explained with a decline in the meridional southern density difference, which in turn leads to a decline in m_E and due to continuity, m_N, the measure of the overturning strength, increases. We included a note about these results in the manuscript. However, including other freshwater fluxes into the model would make it necessary to recalculate the governing equations, i.e. a different version of this box-model. We feel this would expand the paper a lot and we would therefore prefer not to include this in the manuscript.

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.

Response: We agree with the reviewer that this point might not have been clearly enough explained in the manuscript. This is also a matter of the validity of our model assumptions. The eddy return flow becomes negative because the southern meridional density difference becomes negative. Our parametrization for the eddy is only valid for a positive eddy return flow. That means our model describing the current state of the overturning circulation is not valid anymore which is interpreted as a point if instability of the circulation pattern the model describes. Physically this means that there is no outcropping of iso-pycnals in the Southern Ocean anymore in this case and thus the eddy return flow does not follow the physics that is described by the baroclinic instability and thereby the Gent-and-McWilliams equation. This also establishes a qualitatively different circulation pattern. We included a more detailed description in the manuscript.

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.

Response: We agree with the reviewer that this is a very important point and included it in the conclusion section.

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 be- tween the Southern Ocean and the North Atlantic (Saenko and weaver, GRL 2003). How important is this choice in the context of this paper?

Response: While the introduction of the meridional density difference between low latitudes and the southern ocean in the parametrization of the eddy return flow is the main point of the paper and was motivated with the AMOC crossing the Atlantic in the south and in the north, the overall strength of the overturning as driven by the northern and low latitude box is not central. Picking the Southern box would make the analysis considerably more complicated. The main motivation for this choise is as follows: The circulation of the surface waters need to be in geostrophic balance, thus these circulations have to follow a density difference. These strong changes in density have been observed in the real ocean and numerical models and it is a key point in our model to include both a northern and southern meridional density difference in our model. However, the exact location of this density change can vary for the real ocean.

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.

Response: We agree with the reviewer. Thank you for the detailed reading. We rephrased this sentence accordingly.

p32. l21.”The threshold behaviour found here is consistent with the salt-advection feed- back 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?

Response: This sentence refers to the salt advection feedback and the transport of salt rich water by the AMOC into the northern Atlantic. We rephrased this sentence in the manuscript in order make this point more clear.

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.

Response: We included citations from Weaver 2012 (GeophysicalResearch Letters) and Bryden 2011 (Journal of Marine Research) into the manuscript here.