Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2018-134-RC1, 2019

Ocean Science Discussions
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Interactive comment on "Cold vs. warm water route – sources for the upper limb of the AMOC revisited in a high-resolution ocean model" *by* Siren Rühs et al.

Anonymous Referee #1

Received and published: 17 January 2019

I enjoyed reading this manuscript, which has a very clear narrative, is technically sound, and clearly answered the questions it set out to answer. The authors use well established Lagrangian methods and an eddy-rich model to update previous Lagrangian estimates of the South Atlantic AMOC return flow circulation patterns. Contrary to previous Lagrangian analyses based on coarser-resolution models, they find that the "cold-water" path through Drake Passage is a significant contribution to the AMOC upper-cell return flow (and how this might have changed over time). They also use the Lagrangian approach to clarify the water-mass transformation histories of the "warm/salty-water" and "cold/fresh-water" pathways as they interact with the mixed-layer. I have general comments about the framing of the paper and the lack of some important references. I

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Discussion paper

have only minor comments about the content of the methods and results.

General Comments:

The major weakness of the manuscript is its disregard for theories of the Atlantic Meridional Overturning Circulation and inter-basin exchange. The remainder of the general comments section is devoted to these theories.

In particular, I recommend an additional sentence before p.2, l.8-12 describing research about the salt advection feedback which maintains the AMOC and also the related (bi-) stability of the AMOC. The authors should modify the following lines (currently 8-12) to contextualize those references in this boarder theoretical context. It would be good to link back how the prevalence of the cold/fresh-water route fits into this context (perhaps not much, since as you have shown these waters are subsequently transformed as they traverse the South Atlantic).

Examples of conceptual theoretical models of the AMOC and the salt-advection feedback: Stommel's classic two-box model (https://onlinelibrary.wiley.com/doi/abs/10.1111/j.2153-3490.1961.tb00079.x) Rahmstorf's recent model (https://link.springer.com/article/10.1007/s003820050144) Wolfe and Cessi's more recent attempt (https://journals.ametsoc.org/doi/10.1175/JPO-D-13-0154.1)

Similarly, in p.2, l.26-33, the authors cite many observational inverse models and model-based Lagrangian analyses on both sides of the debate but there is very little theoretical consideration of why either the cold-water or warm-water pathways should be favored, despite the existence of such studies. Notably, Cessi and Jones (https://journals.ametsoc.org/doi/10.1175/JPO-D-16-0249.1) derive an elegant theoretical explanation for the (supposed) dominance of the warm/salty-water route relative to the cold/fresh-water route. Summarizing their theoretical argument, they say:

“Here, we present arguments indicating that all of the supergyre enables the warmroute exchange of the MOC. Specifically, we show that the position of the short continent relative to the latitude of the zero Ekman pumping determines whether the cold route or the warm route is the preferred mode of interbasin exchange. We find that as long as there is a subtropical supergyre connecting the South Atlantic to the Indo-Pacific, the exchange is via the warm route. Conversely, if the subtropical gyres in the Atlantic and Indo-Pacific sectors are separated, then the interbasin flow is via the cold route."

Given that this theoretical result directly contradicts the findings of this paper, I would like the authors to acknowledge this work and perhaps speculate on what might be the cause of the contradiction.

Specific Comments:

p.5, l.8-9: Why free-slip in the base model and no-slip in the nest? Is this a standard difference between model formulations for eddy-permitting and eddy-rich models? I suspect it does not make much of a difference to the simulation but I am curious and other readers will likely be as well.

p.9, l.16-31: Perhaps worth mentioning that in this case (and more generally) it is difficult to directly compare Lagrangian studies because making slightly different choices in release / conditional sections results in differences in transport estimates for which it is impossible to disentangle model differences from methodological differences. The lack of Lagrangian analysis inter-comparisons makes it even more difficult to determine how important these differences can be. Some progress has been made by recent model inter-comparisons across OGCM and Lagrangian Model code by Tamsitt et al. (https://www.nature.com/articles/s41467-017-00197-0) and across model resolutions within a single OGCM and Lagrangian Model formulation by Drake et al. (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL076045), both of which consider the complimentary pathways of upwelling circumpolar deep water in the Southern Ocean.

p.9, l.27-30: Please explain what a "OGCM used in diagnostic mode" means. Do you mean in a data assimilation mode, as in the ECCO models?

p.10, l.6-9: It may be worth clarifying that the Lagrangian method used here does not account for the effects of sub-grid scale stirring, which would act to further disperse trajectories and could modify transit time distributions.

p.12, l.7: “alreday” is a typo for “already” but I would just leave the word out all together as the sentence does much make much sense with it in there.

p.12, l.10-11: Could you clarify whether these distributions (and all following distributions) are particle weighted and whether this is equivalent to area-weighting, transport-weighting, or neither? It seems that for T,S diagrams along a section that area-weighting might be most appropriate?

p.13, l.24-28: I am admittedly a bit confused about how the Ariane model considers flow in the mixed layer. Since transformations within the mixed layer are a key part of this section, I feel it would be important to discuss what it means to advect particles with the resolved velocities in a mixed layer for which (presumably) convection is not resolved (Is this true? I have little experience with such a high-resolution model). In most mixed layer schemes that I am familiar with, the tracer fields are rearranged or homogenized by artificially increasing the vertical diffusivity. What does this mean for a particle in the mixed layer? See for example the discussion in van Sebille et al. (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/grl.50483).

p.17, l.11-13: See however Tamsitt et al (https://www.nature.com/articles/s41467-017-00197-0) and van Sebille et al (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/grl.50483) who use particle probability maps to show that even in a Lagrangian framework in eddying models, the Global MOC still has some spatial coherence that corresponds to coherent tracer distributions.

Figure 7: It took me perhaps 10 minutes to understand what was happening in this entire figure. I would recommend adding the blue lines to (a) and the red lines to (b)

and perhaps adding annotations of “AC” in red and “DP” in the top left of (a). The use of parentheses to signify the converse in the last sentence of the caption did not help.

Technical Corrections:

p.5, l.13: Typo? Should the maximum bi-harmonic eddy viscosities Ahm0 be -6x10ˆ9 (-1.5x10ˆ11)?

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