

Ocean Sci. Discuss., referee comment RC2
<https://doi.org/10.5194/os-2020-124-RC2>, 2021
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Comment on os-2020-124

Anonymous Referee #2

Referee comment on "Nonlocal and local wind forcing dependence of the Atlantic meridional overturning circulation and its depth scale" by Tim Rohrschneider et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2020-124-RC2>, 2021

This paper looks at two different experiments to examine the degree to which depth scale can be represented by a pycnocline scaling or by the level of no motion, in a realistic eddy-permitting OGCM, in regard to its relationship to AMOC. The first experiment examines the impact of local vs. non-local winds on AMOC (on decadal timescale). The second looks at AMOC in a step-up 4xCO₂ coupled simulation across 100 years.

The paper gets off to a decent start by raising some interesting questions: one set of questions is related to the aforementioned local vs. nonlocal wind question (i.e. intro first paragraph) whereas a second set seems generally related to a lack of understanding in AMOC theory related to depth scaling (i.e. second paragraph). On the former, while there are cited studies which examine this question, certainly this study digs deeper to try and better understand the literature. On the latter, I certainly agree that while there has been much theoretical progress over the past several decades, interesting questions remain.

But my main difficulty here: which of these is the main focus of the paper, and what is the take-away message? About 3/4 of the paper focuses on the wind experiments, and if the title were changed to reflect this topic, the paper would very much read like this was the primary subject. But the paper also analyzes the global warming experiment, which (given the ms. title) suggests that this study is meant more as a contribution about AMOC depth scaling and theory (?) So which is it?

I could see a stand-alone paper on the wind experiment focus, although the challenge is to be sufficiently novel, given past studies (for the most part, indeed I think it makes a step forward). I could also see the paper being more about AMOC depth scaling and theory (given presentation of the two different experiments), but I think the paper needs to be revised throughout given this main focus in mind, provide more context and synthesis, and sharpen the take-away message. As is, the 4xCO₂ experiment feels a bit tacked on and undermotivated. My recommendation is major revision.

Other comments:

- As one example of what seemed underwhelming in regard to building on AMOC theory, while the Bryan 1987 scaling is cited and its general approach explained, the ms. does not explain how this has been used to predict scaling of the AMOC to different parameters, the most explored being diapycnal mixing, but also density gradients. Might this be relevant in the 4XCO₂ expt? And although Levang and Schmitt is cited, there is little discussion of how new findings here might relate to their study (note, not sure I understood the sentence l. 421). I would think the large spread of AMOC changes in CMIP-class models might be excellent motivation for the 4xCO₂ experiment, If the authors wished to explore this further.
- Motivation for model choices was lacking. If this work was intended as a more conceptually-oriented contribution, could this be accomplished using a coarse, idealized setup? Or maybe, both a (cheap) coarse, idealized setup could be contrasted with the realistic run? While there is some hint in the ms. that the eddying capability is important in particular for accurate wave-propagation (given the decadal adjustment timescale), this is not clear to me. Or, why not run a coarse model to equilibrium? Is the decadal adjustment an element of the story (i.e. as implied in title) ? I'm not saying this study needs to be redone with a different setup, just that there is scant justification for the model setup used.
- On figures, many lines labelled as "black" were more gray to me, and "opaque" vs. "transparent" were better described as red vs. pink, for example. Is dotted blue line missing in 8a?
- l. 40 discusses "diapycnal upwelling in the tropics" after mentioning the Gnanadesikan (1999) model. Although perhaps a bit beyond the scope of this paper, I might argue Gnanadesikan is a single basin model that explicitly assumes the adv-diff balance in its overturning hemisphere, whereas the model here is global and one might assume the important advective-diffusive balance (justifying an e-folding pycnocline scaling) might be occurring in the (larger) Pacific basin. Again, if the main focus is as a conceptual AMOC contribution, it is a bit disappointing to not even comment on other possible relevant issues such as this, nor advance the science with new or revised conceptual models, nor use new results to go back and comment more extensively on conceptual understanding in the literature.
- It would seem more could be explored about the relationship between zonal and meridional density gradients. Of course, the pycnocline scaling says nothing directly about zonal density gradients, in contrast with level of no motion (albeit somewhat indirectly).
- l. 116 mentions "monthly climatology of reanalysis wind stress is doubled"; by this I presume the reanalysis wind is available every six hours or thereabouts, and the six-hourly variability is preserved but the monthly mean wind is doubled? Or please explain. Does high-frequency forcing play any role?
- l. 153 "surface buoyancy fluxes change continuously": this could be said about any model with a seasonal cycle or interannual forcing. I think what is meant is that the 4xCO₂ experiment adjusts slowly to the step change, with the surface forcings changing as a function of ocean-atmosphere state in the coupled setup. In contrast the wind experiments are not coupled, and the adjustment is assumed to occur on a decadal time scale, simplifying the analysis to a comparison of 1991-2010 mean states.
- l. 280 by "inter-hemispheric regions" I presume the authors are referring to 30S-30N?
- l. 361 given that the discussion is not specific about various mechanisms of transport, would suggest wording as "heat is fluxed downward"
- l. 452 didn't follow sentence