Review of "Diagnosing the AMOC slowdown in a coupled model: a cautionary tale".Justin Gérard and Michel Crucifix, for Earth System Dynamics.

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General comments

This manuscript is primarily a response to a 2020 paper in the Journal of Climate on "What causes the AMOC to weaken in CMIP5?" by Samuel Levang and Raymond Schmitt. Levang and Schmitt concluded, using diagnostics based on the thermal wind relationship and zonal density gradients across the width of the basin that:

> "Therefore, in CMIP5, temperature dynamics are responsible for AMOC weakening, while freshwater forcing instead acts to strengthen the circulation in the net. These results indicate that past modelling studies of AMOC weakening, which rely on freshwater hosing in the subpolar gyre, may not be directly applicable to a more complex warming scenario."

The current manuscript uses cGENIE, an Earth System model of intermediate complexity, to test the diagnostics used by Levang and Schmitt. The principal finding is that, independent of whether reduced modellied AMOC is caused explicitly by rising CO_2 (temperature forcing) or hosing (freshwater forcing), the thermal wind diagnostics tend to identify changes to the temperature field as the cause of AMOC weakening.

The manuscript is generally well constructed, the results presented clearly, and appropriate conclusions drawn. While I am not an expert on Earth system models, the choice of the cGENIE model here seems well justified and appropriate. The results should be a useful addition to the field and I'm happy to recommend publication subject to authors addressing the minor comments below.

Specific comments

Title. Maybe "Diagnosing *the causes of* AMOC slowdown..."?

Introduction. Lines 29--31. I'm not sure about the characterisation of AMOC flows as 'clockwise' and 'anticlockwise'. I assume this is with reference to meridional sections, presented with North to the right, like those in Fig 1a,b. I think it is more understandable to describe these in terms of the location of downwelling and upwelling, and the direction of the upper and deep flows.

Results. Section 3.1 RCP8.5. Figures 1 and 2.

I found following this section, with reference to Figures 1 and 2, to be quite tricky. I think my difficult is in understanding the relationship between the density anomalies presented in Figure 2 and the maximum of the overturning streamfunction (and hence the AMOC variability) presented in Figure 1. I have some suggestions:

Line 177--179. The overturning streamfunction is not the vertical integral of the zonal density gradient, it is the vertical integral of the *zonal pressure gradient*. The pressure gradient at depth z is, in turn, the vertical integral from the surface to depth z, of the density gradient.

I think this is where my trouble is, the need to integrate twice in the vertical, in my mind, and then select a maximum, to get from the density anomaly profiles in Figure 2 to the AMOC strength in Figure 1c,d. I suggest:

- adding, either as additional panels in Fig 2 or as a new figure, the pressure anomalies in the vertical (i.e. the first vertical integral of Fig 2)
- similarly, add plots of the streamfunctions with depth (the second vertical integral of the zonal density differences).

These would help the reader understand the relationship between the density changes and the streamfunction changes.

Presenting the vertical structure of the thermal wind streamfunction diagnostic would also help to see how the temperature and salinity contributions in Fig 1c,d sum to produce the total geostrophic contribution. I assume the separate T and S contributions in Fig 1 do not simply sum to the total because the salinity and temperature streamfunction maxima appear at different depths?

Section 3.1 could possibly be usefully broken up with some subheading to aid readability

Section 3.2.

Line 271. 'in the latter case'. I think this refers to the freshwater flux hysteresis experiment, which is the former case in this sentence. Please clarify.

Lines 281+ This examination of the oscillating behaviour, while maybe interesting to Earth system modellers, seems out of place in this manuscript. I would recommend dropping it along with Fig 5.

Figure 4a,b. These each contain 4 lines (2 solid, 2 dotted). I think the text and caption only refer to two of these lines? Either delete the two which are not used, or, if they are mentioned somewhere and I've missed it, please differentiate the two pairs, maybe with different colours.

Figures A1 and A2, I'd recommend inclusion of these in the main body of the work.

Finally, a question about the use of depth-space, rather than density-space AMOC diagnostics. Particularly as far north as 54N where much of the analysis is based. At 54 N in the Atlantic Ocean there is a sizeable horizontal component to the overturning circulation which perhaps makes the e-w boundary thermal wind diagnostics less appropriate here than at lower latitudes (and is the reason why the OSNAP overturning monitoring array requires many more moorings than the RAPID array). Could the authors comment on this? Is, for example, the horizontal component of the overturning less significant in the coarser resolution, relatively simple model presented?

I enjoyed reading this and at the end feel I've learnt something useful. Thanks.

Alan Fox