Review of: Long-term Variability and Trends of Agulhas Leakage and its Impacts on the Global Overturning", by Großelindemann et al.

In this paper, the authors analyze Agulhas Leakage and its drivers and impacts in a suite of eddy-resolving coupled climate simulations. They find that Agulhas Leakage is well represented by the model, according to several metrics. They find positive correlations between Agulhas Leakage and several metrics of wind stress, and find that this model projects an increase in Agulhas Leakage in response to an aggressive forcing scenario, despite a weakening of the Agulhas Current itself.

I found the paper very well written, and a pleasure to read. The methodology is clearly described, the analysis is convincing, and the results are interesting and relevant. I do have a list of minor comments, which will require only minor revisions.

> We are delighted that the reviewer received our manuscript very positively. We would like to thank him for his time reviewing the manuscript and providing very constructive comments and suggestions.

l. 125: employes -> employs

> Changed.

Fig. A1: I think it would be better to make subplots for the piControl and FOSI simulations. It is hard to see the individual curves representing the FOSI simulations. Besides, the variability in the two sets should not be expected to be identical, so combining them in the same plot does not make much sense. This plot is important, as it allows the reader to judge the accuracy of the method applied. Despite the muddled mess, the three FOSI curves don't seem to track each other very well.

> We agree with the reviewer and have changed the figure to have subplots. There is a very slight temporal shift between the FOSI monthly and the 5-daily time series, which is reflected in the correlation values we use to judge the methods. The shift in absolute transport values is discussed in the text as well.

Fig A2: This figure does not show a black rectangle, as the caption claims.

> This has been corrected, while the appendix figures were reordered.

L. 158: plausible -> would accurate be a better word here?

> We think that "plausible" better reflects what we want to convey. 'accurate' may imply that we could somehow precisely measure the difference between the exact transport in the model and our estimate.

ll. 207-208, 233-235: Duplicative.

Removed it from l.212ff, as that paragraph is more about winds and the second about circulation.

l. 227 and elsewhere: Fov is the freshwater flux /induced by the overturning circulation/.

Good point, we added this information wherever it is first introduced in each section.

l. 244: Uptream -> Upstream

Changed.

l. 265, 285, 276: Unless you can find a way to make a more robust significance estimate of this spectral peak at 14 years, I would not put that much emphasis on it; especially if it is only one estimate that sticks out, instead of a few adjacent estimates. At 95% confidence level, one is to expect 5 'false positives' for every 100 measurements.

Given the high level of intrinsic variability, we think that it is worth mentioning the spectral peak at 14 years, even though it is only significant at 95% confidence.

l. 273: So minimum zonal wind stress represents the easterlies in the subtropical belt?

Yes, we have added the following sentence to the methods section: "The maximum and minimum zonal wind stress represent the Southern Hemisphere Westerlies and the Subtropical Easterlies, respectively.", l. 211

l. 283: Filtering might also play a role in spreading out the signal.

Good point. We have added the following sentence: "Barotropic and baroclinic adjustment processes as well as filtering of the time series can explain the 0-3-year spread in the range of lead times (Anderson et. al 1977)." l. 289

Figure 6: I find the correlations between Fov and AL suspicious, as they are significant stronger than -0.4 for lags between +/- 7 years. What does that mean? Do both time series have decorrelation times of several decades? The paper says that the time series have been detrended, can you confirm?

The Fov time series shows an increasing trend in the first ~100 years, reaches a maximum, and then decreases until the end due to model drift and adjustment times. We have detrended it, but a linear trend does not completely get rid of the non-linear long-term variation. However, the peak at three years is robust when we calculate the correlation for the increasing and decreasing parts separately. But to be consistent with the other metrics, we chose to show the values from the full time series calculation. We added a short paragraph about this in that part of the manuscript as follows:

"We note that the Fov timeseries in the model has an increasing trend in the first 100 years and then decreases again. This impacts the detrending of the timeseries before calculating the correlations and leads to high correlations across all lead times. The peak

at a 3-year lag remains robust even when increasing and decreasing parts of the time series are considered separately. ” l.320

l. 294 and beyond: I’m wondering if this argument could be taken one step further by actually calculating an appropriate east-west gradient (upper ocean pressure, SSH, or maybe even the depth of the 10 degree isotherm) and comparing that to AMOC strength at 34S.

We have tried the reviewer’s suggestion, but did not find any meaningful connections between a simple SSH-gradient and AMOC strength at this latitude. Properly calculating a gradient is not straightforward, and since there is no meaningful signal from a simple calculation and an AL - Rossby wave/eddy connection is also vague, we have concluded that a more detailed analysis would be beyond the scope of the manuscript.

309: stable -> stabilizing, negative?

Yes, we have modified the text as follows: “We find a mean northward freshwater transport of 0.1 ± 0.03 Sv over the simulation years 150-520 which remains positive during this entire time, indicating a stabilising salt-advection feedback.” l. 314

l. 321: I think it is important to mention the trend over that same period for the control simulation, since I suspect it is not much smaller than that of the historical + future simulations. If even the control has a significant trend over that period, then I suspect that that will modify the conclusion.

The control has an insignificant trend of 0.01 Sv per decade with a p-value of 0.42. We have added this information to the text and the related figure as follows:”The PIcontrol shows a small, statistically insignificant trend of only 0.01 Sv/dec.” l. 336

l. 362: are -> is. Essential for what?

Rephrased it as follows: “Investigating how Agulhas leakage varies on longer timescales is thus essential to understand its impacts on the Atlantic Ocean in the presence of global warming.” l.374

l. 358: It may be worth noting that the Fov as a metric of the salt-advection feedback would asymptote to zero (from a positive value) upon a decreasing AMOC. The fact that it crosses zero suggests changes in the stratification at 34S, giving credence to the conclusion here. Is there a way to quantitatively compare Fov with the salt flux induced by AL?

The suggested analysis would need a closed salt budget calculation. The domain we track the particles in is not big enough to track all the particles that cross 34S at some point during their lifetime. We therefore do not have a complete inventory of all the particle salt transports directly at 34S and hence cannot really calculate the salt budget.

Additionally, the other reviewer correctly mentioned that “The quantity FovS can only be used under (quasi-)equilibrium conditions (Rahmstorf, 1996) and these conditions include the equilibration of the Atlantic Ocean interior. Under strong climate change (RCP8.5) or large freshwater flux changes (Orihuela-Pinto et al., 2022; Jackson et al., 2022) the FovS responses and its effect on AMOC stability are much more difficult to interpret.” We thus refrain from over-interpreting the Fov-AL relation in our transient experiments. Instead we have more carefully formulated our conclusions in this respect. Please view our comments to reviewer Van Westen.

l. 393: Correct parentheses around reference.

Done.

l. 397: This conclusion is more or less unless contradicted a few lines later, where it is claimed that no distinction can be made between Rossby waves or propagating rings. I don’t see it as a problem that we don’t quite know the dynamical character of these propagating signals (maybe they are one and the same!), but it would be good to be consistent. Also, l. 303 acknowledges a potential role for winds, which is missing here.

In response to the reviewer’s comment, we have changed the phrasing from “is controlled by Rossby wave dynamics” to “could be controlled… “. With this change, we think it is fine to discuss that statement in the following sentences. We have also added mesoscale eddies as an extra possibility and not just as a description for Agulhas Rings in lines 309 and 417 . We agree with the comment on the role of winds. We moved the wind-related discussion from the results to the discussion section: It fits better there.

l. 403: It may be semantics, but I’m not sure if Agulhas Rings can be classified as a mesoscale eddies. In my mind, they have a different character and dynamical origin, not in the least because of the barotropic nature of rings that contrasts with the baroclinic character of eddies.

Please see the above response.

l. 427: conductive -> conducive. Correct double ‘of’.

Corrected.

ll. 468-469, 16-17, 450, and other places: I’m uncomfortable with the strong statements that are made regarding the links between Agulhas Leakage and the stability of the AMOC and potential for collapse. Even though I think that the link between Fov and AMOC (bi-)stability is a compelling theory, there is still a lot of work to do to confirm this theory (for instance, by demonstrating it in an eddy-resolving climate model). I personally would not go beyond a statement along the lines of ‘with potential implications for the stability of the AMOC’.

You’re right, we should be more careful: We have adjusted our phrasing accordingly in various places in the manuscript. The other reviewer had similar

comments about this as well. We now just say that our analysis suggests that there could be a connection or a potential role of the salt transport of the Agulhas leakage to AMOC stability, but that further research is needed here.