

Limited influence of the Agulhas leakage on the Atlantic Meridional Overturning Circulation in present climate

Corresponding Author: Dr Shantong Sun

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Decision Letter:

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Dear Dr Sun,

Your manuscript titled "Limited impacts of the Agulhas Leakage on the Atlantic Meridional Overturning Circulation" has now been seen by 3 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to address these criticisms, we would be happy to look at a substantially revised manuscript. If you choose to take up this option, please either highlight all changes in the manuscript text file, or provide a list of the changes to the manuscript with your responses to the reviewers. In the following, we list our editorial thresholds:

Editorial Threshold 1: Provide compelling new insights into the effect of an increase in Agulhas leakage on the Atlantic Meridional Overturning Circulation, for example, by adding simulations with a higher-resolution oceanic model that captures the mesoscale effects on the Agulhas Leakage or by making a compelling case that the current low-resolution (1 deg) model is sufficient and discussing consequent limitations

Editorial Threshold 2: Improve the identification of the Agulhas Leakage in your simulations, for instance, using a Lagrangian approach, or provide compelling evidence that the current method appropriately captures the leakage

Editorial Threshold 3: Better describe the simulations and include validations or add appropriate references for those

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Please do not hesitate to contact us if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

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REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Please find my full review in the attached pdf.

Reviewer #3 (Remarks to the Author):

Review of "Limited impacts of the Agulhas Leakage on the Atlantic Meridional Overturning Circulation" by Zhang et al.

In this manuscript the authors use sensitivity experiments in an ocean model to assess the impact of an increase in Agulhas leakage (AL) on the Atlantic Meridional Overturning Circulation (AMOC). The main finding is that a strengthening AL only increases the AMOC by a fraction of the leakage increase and will therefore not offset a projected weakening of the AMOC under global warming. This was attributed to a compensating effect where the increase in density due to more saline Indian Ocean waters is counteracted by warming. As the main finding contradicts paleo-studies showing that increased AL played a role in the resumption of the AMOC in the past, an additional experiment starting from a collapsed AMOC was conducted in which the AMOC increased significantly with increasing AL. The different responses of the AMOC to an increase in AL could therefore be attributed to different states of the AMOC with it being more unstable after glacial conditions.

The authors address a very topical question but there are major issues especially relating to the methodology:

(1) Improve phrasing and classification of study compared to previous studies: After reading the abstract I had the impression that the impact of AL on the AMOC in the future is analysed which is misleading. It needs to be clarified that sensitivity experiments with an artificially changed AL are being analysed. If space allows including the result from the experiment with the collapsed AMOC would be a great addition to the abstract. Furthermore, the Introduction is not comprehensive and studies addressing the same topic are not discussed in much detail. Although this study quantifies the impact of AL on the response of the AMOC and its lower limb for the first time, other studies have already quantified the impact of AL on the upper limb of the AMOC. I also noticed that some studies were not cited for the right reason (see specific comments below).

(2) Does the coarse model capture important dynamics? I do acknowledge that the amount and runtime of sensitivity experiments used in this study is not feasible with eddy-resolving models. However, to simulate the exchange between the Indian and Atlantic Ocean not only quantitatively but also qualitatively correct a horizontal resolution finer than at least 1° is required (De Ruijter et al., 1999; Biastoch et al., 2008; Beal et al., 2011). You are correcting for the bias in AL strength, but the dynamics might not be well simulated. I am concerned that thermohaline properties from the Indian Ocean are not correctly advected in the absence of eddies (and possibly Agulhas Rings). The thermohaline properties reaching the North Atlantic might be too diluted in your model. An expanded model evaluation in the manuscript is needed: Are there any Agulhas Rings and if so, what are their characteristics? How does temperature and salinity in the western, tropical South Atlantic compare to observations? Show maps (like in Fig. 1a) of biases between ctrl/ AL28 and observations for different depths.

(3) Methodology to estimate AL: The most widely used and very established method to estimate AL is to use a Lagrangian approach (e.g. Biastoch et al., 2008; van Sebille et al., 2009; Rimaud et al., 2012; Schmidt et al., 2021). The Eulerian field is not suitable for estimating the exchange between the Indian and Atlantic Ocean (van Sebille et al., 2010) due to the turbulent nature of the Cape Basin and the proximity to the spatially varying position of the Agulhas Return Current and Antarctic Circumpolar Current. Lagrangian experiments, if run long enough, would additionally shed light on the evolution of thermohaline properties during the advection into the North Atlantic.

(4) Effect of heat vs salt on density: One of the main conclusions is that the effect of salinity on density is compensated by warmer temperatures and that this compensating effect has apparently been “overlooked” (l. 81) in previous studies. But Weijer et al. (2002) found that “Although Agulhas heat and salt input initially balance with respect to their effect on density, it is due to the strong thermal relaxation that a density anomaly can develop on its way north.” Also, Biastoch et al. (2013) concluded that “Changes in the upper-ocean water mass signatures are mainly restricted to salinity, since temperature anomalies are effectively damped by the surface heat fluxes (Figure 4b).” This needs better discussion. Furthermore, I am wondering why there is no increased heat loss to the atmosphere in the South Atlantic. What is the temporal evolution of heat loss? In an eddy-resolving model Rühls et al., (2019) found that Lagrangian particles of AL origin reach the mixed layer at least once in the South Atlantic which led to a salinification and cooling. Chang et al., (2020) found that in the high-resolution version of CESM the surface heat loss is better represented as the SST fronts are more realistic compared to the low resolution version you used. How does the heat loss and SST fronts compare to observations?

Overall, I have major concerns and think that making use of the high-resolution version of CESM or other models in combination with Lagrangian experiments would be needed to ensure robustness of the results.

Specific comments

I. 49 Please expand on what Agulhas Leakage is.

I. 58 Rühls et al., (2022) only analyse past evolution of AL but not the future. Please revise other citations to make sure studies are cited for the right reason. Durgadoo et al., (2013) who have shown that AL predominantly responds to the westerlies has to be referred to.

I. 64 and I. 135 Weijer et al., (2002) apply both a salinity and temperature perturbation.

II. 74-75: There is only an increase in AL of ~6 Sv in CMIP6 (Ext. data Fig. 1) which you probably refer to with “climate models” in I. 75. Only the High-res. CESM has an increase of 10 Sv. Therefore 10 Sv are probably more realistic, but it needs to be clearer what you refer to.

I. 93 The only two observational estimates (citing Rühls et al., (2022) is misleading here) suggest an AL strength of at least 15 Sv (Richardson et al., 20007) and 21.3 ± 4.7 Sv (Daher et al., 2020) considering the transport in the upper 1500 m and 2000 m, respectively. Therefore, I would suggest using the experiment with appr. 15-20 Sv as your CTRL experiment.

I. 95 What does “roughly” constant mean. Please provide details in the methods section e.g. include one standard deviation in I. 444.

II. 98-100 A 10 Sv increase in AL is not typical for CMIP6 models (see comment on II. 74-75)

I. 110 Why was the experiment with an AL of 28 Sv chosen for the comparisons? Is an increase of 20 Sv likely? Or does it best illustrate the mechanisms? But how does a more realistic increase compare?

I. 159 By how much is the salinity difference reduced?

I. 162 Give a range what you refer to by “longer timescales” e.g., of more than 500 years.

I. 220 Cite Richardson et al., (20007) and Daher et al., (2020), not Rühls et al., (2022).

I. 226 How does the inter-model correlation between AMOC and AL look like in the High-resolution MIP of CMIP6?

Fig. 1 Breaking the orange arrow up and adding a few eddies would make the schematic in (a) more realistic as the AL pathway is not comparable to the Agulhas Current and Return Current.

I. 374 Which observational product is used?

I. 390 Define AL28, figure captions should be overall self-explanatory.

II. 476-481 More details needed: What are the approximations and assumptions? Has this been used in previous studies? How big is the error e.g., difference between thermal wind relationship using left-hand side and right-hand side?

I. 482 Why isn't TEOS-10 used? It's the new standard and has replaced EOS 80.

II. 527-529 “low-resolution climate models cannot fully resolve the mesoscale processes involved in the Agulhas Leakage and may introduce biases” should be rephrased as these models cannot resolve mesoscale processes at all, instead often parametrizations are used, but biases are certainly introduced.

II. 529-531 What is the conclusion from the High-resolution CESM? What do models from the High-resolution MIP suggest? Extended data Fig. 8: Add a small map to show regions used for spatial average. Add thin lines for observations to make comparison easier.

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We hope you will find the reviewers' comments useful as you decide how to proceed. Specifically, for publication in Communications Earth & Environment to be appropriate, we would need you to meet our Editorial threshold:

Editorial Threshold: Provide a detailed description and validation of your methodology at a level of detail that will allow independent researchers to replicate your results, and answering the queries raised by reviewer 3 in points 1 and 2

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REVIEWER COMMENTS:

Reviewer #2 (Remarks to the Author):

Review of manuscript COMMSENV-24-0531-T:

"Limited impacts of the Agulhas leakage on the Atlantic Meridional Overturning Circulation in the present climate"
by Louise Rousselet

First I would like to acknowledge the rigorous work of the authors to reply to all authors comments. The revised manuscript has clearly improved from the previous version. After reading closely to the authors' responses to reviewers, I feel the authors carefully replied to all the questions raised by reviewers. Authors clearly addressed all my main concerns and add significant explanations and analysis to the other reviewers suggestions. Therefore in my opinion the revised manuscript is ready to be published after the minor revisions listed below (mainly typos) have been addressed (the lines refer to the revised marked up manuscript):

1) L606-620: Additional Lagrangian computations have been performed, which is valuable but no "numbers" are given. Please provide the Lagrangian and Eulerian estimates the authors computed.

2) L610: The main point when performing Lagrangian experiments is to clearly define the particle initialization, allowing for the experiment to be reproducible. In the text, two information are missing: i) the length of particle initialization (i.e. How long for the particles are released every ten days? Typically to avoid bias particles are initialized for the first year of the model run. If this is not the case and an other choice have been made, please explain; ii) the total number of particles initialized and the number of particles kept for the analysis/calculation (i.e. the ones crossing Good Hope in this case).

3) L613: "... are counted ..."

4) L616: "Here we are also using.."

Reviewer #3 (Remarks to the Author):

The authors have extensively revised the manuscript and thereby improved the manuscript considerably. But I still have comments I would like the authors to address before publication.

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Dear Dr Sun,

Your manuscript titled "Limited impacts of the Agulhas leakage on the Atlantic Meridional Overturning Circulation in the present climate" has now been seen by our reviewers, whose comments appear below. In light of their advice we are delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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“Limited impacts of the Agulhas Leakage on the Atlantic Meridional Overturning Circulation”

by Louise Rousselet

Within this manuscript the authors investigate the influence of an increase in Agulhas Leakage on the AMOC strength using a series of model experiments. They identify an overall increase of the AMOC transport associated with a Agulhas Leakage increase. Particularly, if the Agulhas Leakage reaches 10 Sv, a typical value estimated by climate models the AMOC is increased by about 1.5 Sv. This result shows that an Agulhas Leakage increase is not enough to counter-balance the projected AMOC weakening (of order 10 Sv) in the 21st Century. The authors also suggest this limited impact is due to the salinity increase being compensated by warming in the Atlantic.

The manuscript is well-written, and synthetic. The results offer new insights into the AMOC variability and propose a new scenario as for the AMOC response to an increased of Agulhas Leakage, as suggested by climate models, in future years. The research question and methodology clearly fits to the scope of Communications, Earth and Environment.

I really enjoyed reading the manuscript as it is clearly structured and the results are significant. Generally speaking all the material is here to illustrate the authors main points. However I think the manuscript lacks several information about the model representativeness of the global ocean system. These details are crucial to strengthen the results reliability. I would thus recommend to publish the manuscript after these few points have been addressed. Please find in the following my detailed comments.

Main comment:

Numerical simulations from model allow for testing several hypothesis which is very important to understand the processes that control large dynamics variability such as the AMOC. However I do believe one important point to trust model outputs is the validation against observations. For example, the authors compare the transport by Agulhas Leakage from their "CTRL" experiment with estimations from observation, which is great especially because changing the surface zonal wind stress reduces the modeled Agulhas Leakage closer to observations. However there are no details in the text about the MOC transport estimate or how the model estimate the ACC transport (review of observation-based estimation in Cessi (2019)). The later controls the main exchanges between the Atlantic and Indian Ocean, so it seems reasonable to expect the model to well represent the ACC flow. The minor comments below may be linked and clarify this point:

Minor comments:

L 32: "typical in climate model" predictions ?

34

35 L 86-88: Does this change affect the vertically integrated zonal transport of the ACC since this
36 current is a major component of the exchanges between Indian and Atlantic oceans Forget et al. (2015).

37 L 92-95: Can you please specify here the AMOC estimate from the CTRL experiment ? From Figure
38 1 I would say about 17 Sv but not sure. Also how this value compares with estimates from observations
39 (Cessi (2019))? More generally in this section, I would suggest to add a small description of the model
40 characteristics to help the reader.

41

42 Section "Atlantic warming compensates for salinity increase": The authors use the AL28 experi-
43 ment since it is the most likely scenario based on a 10 Sv AL increase. I should stated more clearly.

44

45 L 133-134: Is this warming only due to increased volume of Indian Ocean entering North Atlantic
46 or due to other processes ? What can we expect in the global warming context ?

47 L 178-184: Again here, I don't understand why the authors picked the specific value of 5 Sv to represent a
48 weaken AMOC? Is it arbitrary ?

49

50

51 **References**

52 Cessi, P., 2019: The global overturning circulation. *Annual review of marine science*, **11**, 249–270.

53 Forget, G., D. Ferreira, and X. Liang, 2015: On the observability of turbulent transport rates by argo:
54 supporting evidence from an inversion experiment. *Ocean Science*, **11** (5), 839–853.

55 Rousselet, L., P. Cessi, and G. Forget, 2021: Coupling of the mid-depth and abyssal components of the
56 global overturning circulation according to a state estimate. *Science Advances*, **7** (21), eabf5478.

Review of manuscript COMMSENV-24-0531-T: Limited impacts of the Agulhas Leakage on the Atlantic Meridional Overturning Circulation

In this manuscript the authors assess the impact of Agulhas leakage on the Atlantic Meridional Overturning Circulation (AMOC) under different climatic conditions. They do so by evaluating a series of sensitivity experiments performed with a coarse-resolution ocean-only model experiment corresponding to the oceanic component of the Community Earth System Model (CESM). Their sensitivity experiments for present climate conditions with step-wise increasing Agulhas leakage from 7.5 Sv to 40.4 Sv reveal an only moderate increase of the AMOC for leakage strengths smaller than 28 Sv, and no AMOC increase for leakage strengths larger than 28 Sv. The limited impact is related to compensating temperature and salinity changes. In contrast, additional sensitivity experiments for glacial climate conditions show that an increase in Agulhas leakage from 7.5 Sv to 28 Sv can evoke a substantial AMOC increase leading to a complete recovery of a previously nearly collapsed AMOC. This highlights the dependence of the impact of Agulhas leakage on the climatic background state.

I read the manuscript with great interest. The research objectives are timely and fit well to the scope of the journal, as they address a key uncertainty of future climate projections. However, I feel that the title and abstract of the manuscript do not accurately summarize the findings of the presented analysis and even may be misleading, as they solely focus on the simulated limited impact of Agulhas leakage on the AMOC under present climate conditions but do not mention the diagnosed dependence on the climatic background state (see major comment 1). Moreover, I have concerns regarding the chosen method for the quantification of Agulhas leakage (see major comment 2) and feel unable to judge the validity of the performed sensitivity experiments due to the very limited description of the experimental design (see major comment 3). Finally, there are several missing or inaccurate references to previous related work (see minor comments). All in all, I feel that major revisions are needed before I can assess whether the manuscript will make a convincing contribution to the field.

Major comments

- 1. I feel that the title and abstract of the manuscript, stating limited impacts of Agulhas leakage on the AMOC, do not really fit to the presented analysis as a whole. They are to some degree misleading, as the authors themselves show that Agulhas leakage can have a significant impact on the AMOC, depending on the background state.**

Within the manuscript, the authors discuss three different factors that can change to which degree a specific increase of Agulhas leakage impacts the AMOC strength: compensating warming effects, the timescale under consideration, and the climatic background state. The result highlighted in the title and abstract is mainly the consequence of a diagnosed compensating warming effect occurring under present climate conditions. However, additional analyses performed by the authors reveal that changes in the thermohaline background state under different climatic conditions can result in a significant impact of Agulhas leakage on the AMOC. This appears to be a very important point regarding the interpretation of state-of-the-art climate model results, which often fail to adequately represent the current thermohaline background state. Indeed, the authors conclusion that Agulhas leakage has limited impacts on the AMOC under present climate conditions is inferred from a model with originally too strong Agulhas leakage and (only vaguely mentioned) errors in the simulated thermohaline background state. How robust is the main conclusion formulated in the title with respect to these errors? Would models with a more accurate thermohaline background state eventually feature a stronger impact of Agulhas leakage on the AMOC for present climate conditions?

I suggest that the authors provide a more comprehensive assessment of the background states of their employed model simulations and implications of potential errors. Moreover, the title and abstract should be modified to include information on the sensitivity of the results to the climatic background state.

2. The employed method for the quantification of Agulhas leakage requires validation to ensure the results can be trusted.

If I interpret Lns. 434-437 correctly, the authors estimate Agulhas leakage via a section-integrated transport in the south-east South Atlantic, which is not common within the community. A lot of previous studies investigated how to accurately quantify Agulhas leakage in climate models; the estimation of Agulhas leakage via section-integrated transports has been suggested to be inadequate, and Lagrangian methods have become the most established methods (cf., Richardson. 2007; Biastoch et al., 2008; van Sebille et al. 2009; Durgadoo et al., 2013; Cheng et al. 2016; Daher et al., 2020; Schmidt et al., 2021; R  hs et al. 2022).

I strongly suggest recalculating Agulhas leakage using one of the established methods. If this should not be possible, a detailed introduction, motivation for, and validation of the approach chosen by the authors is necessary to be able to trust the presented results.

3. The description of the performed ocean-only model experiments is insufficient to fully understand the experiment design (and ensure reproducibility); the respective model data appears to be neither published nor described in a previous manuscript and is also not published (yet?) as part of the current manuscript.

If the ocean-only model experiments indeed have been newly performed for this manuscript, a more extensive experiment description should be provided for the “default setup”, “CTRL”, the 10 experiments with Agulhas leakage increase (e.g., within a supplementary), and the hosing experiments (“Hosing” and Hos28”). These should include information on the initialization, type of wind modifications for Agulhas leakage increase experiments, and region/strength of hosing for hosing experiments. Moreover, a basic quantitative validation of the CTRL experiment should be added. The provided Extended Data Fig. 8a,b and 9a,b are not sufficient to assess how well the current thermohaline background state and AMOC strength/structure are represented.

I suggest providing such extended experiment descriptions in a supplementary and to include a table or sketch summarizing all the different performed experiments, the introduced abbreviations, and their most important features.

Minor comments

- Ln. 26: “Agulhas Leakage is expected to increase as the Southern Hemisphere westerly wind shifts poleward” This statement made here as well as elsewhere in the main text with reference to Biastoch et al. (2009) has been contradicted by more recent studies such as Durgadoo et al. (2013) that suggest that Agulhas leakage is increasing primarily due to a strengthening of the westerlies (and that a poleward shift without strengthening of the winds could indeed lead to a reduction of the leakage strength).
- Ln. 31: “we show that a 10 Sv [...] increase in the Agulhas Leakage, typical in climate models, only strengthens the AMOC by a maximum of 1.5 Sv” This statement sounds as if there already exists scientific consensus on the magnitude of the projected increase in Agulhas leakage in state-of-the-art climate models. To my knowledge, prior to the present study, projected future changes in Agulhas leakage have only been quantified in individual climate models with partially conflicting results (e.g., Ivanciu et al. (2022) report an Agulhas leakage increase of 1.5 Sv until the end of the 21st century; Daher & Kirtman (2023) only find a significant increase of Agulhas leakage for a high emission scenario until 2040, but no significant increase for lower emission scenarios and the period after 2040).
- Lns. 33-34: “The projected AMOC weakening of order 10 Sv” This statement as well implies some consensus on the order of projected AMOC weakening. However, the magnitude of AMOC weakening greatly differs between climate models and is thought to be a key uncertainty for future climate projections (cf., IPCC AR6).

- Lns. 51-52: “Due to the thermohaline differences between the Atlantic and Indian Ocean water [...]” This sentence kind of implies that the absolute differences in the thermohaline properties between the Indian Ocean and Atlantic Ocean matter. However, in general, the Atlantic is the basin with the highest salinities in the world ocean and the crucial point is the difference in the thermohaline properties between inflowing and outflowing water masses.
- Lns. 66-67: “However, the AMOC response to salinity perturbations may differ from that due to an increase in Agulhas leakage” The authors raise a very important point here. However, for clarity, it would be good if this statement could be expanded by potential reasons for the differences.
- Lns. 81-81: “due to the compensating effect of warming in the Atlantic Ocean [...], which has been overlooked in previous studies” I would appreciate if the authors would formulate this in less general terms, as some previous studies indeed already discussed those potential compensating effects (e.g., Weijer et al., 2002).
- Lns. 93-94: “Agulhas Leakage is about 7.5 Sv, close to observations” This statement appears to be far-stretched, given that observation-based estimates yield values of 15 Sv in the upper 1000 m (Richardson, 2007) and 21.3 ± 4.7 Sv in the upper 2000 m (Daher et al., 2020).
- Ln. 100: “a typical 10 Sv increase in the Agulhas Leakage” How do we know it is typical? To my knowledge, there is no consensus (see comment above). The authors should add evidence in form of references or reformulate this statement.
- Lns 102-103: “the projected AMOC decline in climate models of order 10 Sv” Also here, to my knowledge, the scientific consensus is still lacking (see comment above). The authors should add evidence in form of references or reformulate this statement.
- Lns 103-105: “When the Agulhas leakage is over 30 Sv, the AMOC strength [...] does not respond to the Agulhas leakage increase” Very interesting finding, supporting the idea that the AMOC response to Agulhas leakage changes is dependent on the mean state, as noted already by Weijer and van Sebille (2014). However, isn’t this also suggesting that the current generation of climate models, which – due to their relatively coarse oceanic resolution – may feature a too laminar and hence too strong mean Agulhas leakage strength larger than 30 Sv (extended data Figure 1 indeed supports this statement, if one can trust the chosen method for the calculation of Agulhas leakage), potentially underestimate the impact of Agulhas leakage changes on the AMOC?
- Lns 142-144: “[...] we expect this damping to be weaker in coupled models, in which the atmosphere can respond to surface heat fluxes”. It would be helpful to explain why this is to be expected, respective references should be added.
- Lns 157-159: “the increased Agulhas leakage decreases salinity in the upper Indian Ocean” I feel this relation needs some causal explanation.
- Lns 176-178: “The 10 Sv increase in AMOC strength cannot be explained by the absence of warming compensations. Instead, this contradiction may be reconciled by considering the sensitivity to climate background” I find this a very important statement and the corresponding section of the manuscript appears the most relevant to me (see major comment above)
- Ln. 181: “the AMOC is weakened to around 5 Sv” What has been the initial AMOC strength?
- Lns. 184-185: “After about 600 years, the AMOC fully recovers and strengthens by over 12 Sv.” I am intrigued by the timescale. For the original experiments the AMOC strengthening in response to the Agulhas leakage increase stopped after ca 150 years and afterwards turned into a weakening. The response timescale for the recovery after the housing is much longer. I suggest discussing potential reasons.
- Lns 193-195: “[...] the collapsed AMOC increases the salinity in the upper South Atlantic, leading to a more negative Fov”. This implies that Fov already has been negative in the CTRL experiment. Yet, based on previous analyses, we know that Fov in the South Atlantic is actually positive in many state-of-the-art climate models including CESM (e.g., Mecking et al., 2017; Jackson et al., 2023). Was the negative Fov achieved here through the initial reduction in Agulhas leakage for the CTRL experiment? (also see comment below)?

- Lns. 198-201: I find this paragraph unnecessary, as the results of F_{total} are not really discussed. I suggest to either expand the discussion or delete the paragraph (and respective parts of the corresponding figure).
- Lns. 224-227: Important conclusion! I would find it necessary though to highlight again that this minimal sensitivity in the models potentially represents an underestimation of the real leakage impact (which also implies that we actually do not know how true the statement of a limited impact of Agulhas leakage on the AMOC actually is?). Yet, this result is not completely new, and a reference of previous studies suggesting this would be appropriate (see comment above).
- Ln. 437: What is the default set-up? If already published, official experiment identifier and reference should be provided; otherwise, this experiment needs a more extensive description
- Lns. 439-441: Why does a strengthening of the winds lead to a decrease in Agulhas leakage?
- Lns. 441-443: This would be a good place to describe a potential relation between biases in the thermohaline properties and Fov (i.e. which sign has Fov before/after “fixing the biases” for the CTRL experiment?)
- Lns. 445-448: “Agulhas leakage is modified to [...]” How? How much and in which way have the wind fields be altered to achieve these results?
- Ln. 462: “[...] density difference between the deep-water formation region and the southern interior Atlantic [...]” I find this formulation misleading, as the southern interior Atlantic is defined over a box in the mid-latitude northern Atlantic. How was this region chosen?

Figures

- All figures: I suggest to unify font sizes and font types
- Figure 1:
 - adjust topographie in (a) to match data and depth level
 - in legend for (a) add type of observational source

Formalities

- While the manuscript is not difficult to read, it contains quite a few minor grammatical mistakes that should be corrected (e.g., a lot of either missing or extensive use of “the” and “a”, as well as confusions regarding singular and plural forms)
- Agulhas leakage -> Agulhas leakage

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Review of revised manuscript by Zhang et al.

The authors have extensively revised the manuscript and thereby improved the manuscript considerably. I have the following comments I would like the authors to address and several minor suggestions.

- 1) I appreciate that the authors conducted Lagrangian experiments to estimate Agulhas leakage but necessary information on how they were executed is missing which prevents reproducibility and hinders interpretation of introduced error. Questions you might want to answer are: Which temporal resolution does the model output have which is used to advect particles? Which model years were used to advect the particles? How many particles were released? Is the result sensitive to the number of particles? For how long are particles released? For how long were the particles advected? At which resolution is the particle position saved? How is transport assigned to a particle? Over which period is the particle transport across the Good Hope line averaged to estimate Agulhas leakage?
- 2) I might have missed that in my earlier review, sorry about that. But describing the model set up in only 8 lines is insufficient. And unfortunately, the Text S1 does not add much more information other than the model year where the sensitivity experiments were branched off. Please provide details on e.g. initialization, spin-up, length of the simulations, and other specifications like salinity restoring if applied.
- 3) l. 146-147: I cannot see where the 2.5 Sv comes from by looking at the yellow line in Fig. 2c? I might be misinterpreting the figure. If the strength of Agulhas leakage is on the x-axis then I struggle to see a linear relationship, e.g. the AMOC increase between AL5Sv and AL15 Sv is appr. 3 Sv while the AMOC increases by appr. 2 Sv between AL10Sv and AL20Sv. If the Agulhas leakage increase is on the x-axis (please adjust label if so), then an increase of 10 Sv is related to an AMOC anomaly of 2 Sv.
- 4) l. 95,97, 477, 478, 479, ...: Why is the word “about” needed here? The numbers are probably an average calculated over a certain period and if this is made clear in the methods section (maybe together with a std or percentile range), “about” can be deleted so that readers don’t wonder what the exact value might be or if there were an error which would require the word “about”.
- 5) l. 505-506: This needs rephrasing. Although the correlation is really high the absolute numbers are quite different. This will probably not impact your results but should be stated clearly.

Minor comments:

l. 73-75: It should read: “Thus, to date, it remains elusive how ~~would~~ an increase in the Agulhas leakage may affect the AMOC 74 in a warming climate.”

l. 83: found → find

l. 90-91: improve readability: “To isolate the impact of an increased Agulhas leakage on the AMOC, ...”

l. 96: I would suggest rephrasing: “This is weaker than the poorly constrained observational-based estimates of 15 Sv (Richardson et al., 2007) and 21.3 +- 4.7 Sv (Daher et al., 2020).”

l. 119: Shouldn't it read: “The limited impacts of Agulhas leakage increase on the AMOC are partly due to the compensating effects of warming **and salinification** in the Atlantic.”?

l. 144-146: reference Fig. 2

l. 145-146: in high latitudes → at high latitudes

l. 154-156: Please rewrite this sentence “subsurface warming ventilates at the sea surface” does not make sense.

l. 166: delete “at”

l. 219: ... the AMOC may **have** become more unstable ...

l. 251: suggest → suggests

l. 413: schematics → schematic

l. 414: observation → observations

l. 475-476 and l. 33 in Suppl.: North Atlantic deep water → North Atlantic Deep Water

l. 479: drake passage → Drake Passage

l. 482: “in which we increase the Agulhas leakage up to 40 Sv.” → “in which we increase the Agulhas leakage to up to 40 Sv.” I would also suggest expanding this sentence by including how you increase Agulhas leakage (i.e. by modifying the strength of the surface zonal wind stress (see Table S1))

l. 499 count → counted (or considered)

Supplementary Information

At the moment Text S1 in the Supplementary material does not provide any additional information which is not yet in the main manuscript or should be in there.

Table S1: A better caption is needed, explaining for example what the number after the +- sign in AL_Eulerian is and why it's not given in AL_Lagrangian. “dyne” is not a SI unit, please convert.

Text S2: often a “the” is missing (e.g. “the AMOC response” (l. 52), “the Atlantic Ocean” (l. 53), etc)

l. 62-64: reference a figure

l. 70: reference figure S14

l. 71: left → north-westward

Reply to the first reviewer

We thank the reviewer for these helpful comments. We appreciate the time and efforts the reviewer invested in reviewing the previous version of the manuscript. We have addressed the reviewer's comments, which are in blue and italics, in the revised manuscript. Our replies are in black following each of the reviewer's comments.

Reviewer #1

Major comments

1. I feel that the title and abstract of the manuscript, stating limited impacts of Agulhas leakage on the AMOC, do not really fit to the presented analysis as a whole. They are to some degree misleading, as the authors themselves show that Agulhas leakage can have a significant impact on the AMOC, depending on the background state.

Within the manuscript, the authors discuss three different factors that can change to which degree a specific increase of Agulhas leakage impacts the AMOC strength: compensating warming effects, the timescale under consideration, and the climatic background state. The result highlighted in the title and abstract is mainly the consequence of a diagnosed compensating warming effect occurring under present climate conditions. However, additional analyses performed by the authors reveal that changes in the thermohaline background state under different climatic conditions can result in a significant impact of Agulhas leakage on the AMOC. This appears to be a very important point regarding the interpretation of state-of-the-art climate model results, which often fail to adequately represent the current thermohaline background state. Indeed, the authors conclusion that Agulhas leakage has limited impacts on the AMOC under present climate conditions is inferred from a model with originally too strong Agulhas leakage and (only vaguely mentioned) errors in the simulated thermohaline background state. How robust is the main conclusion formulated in the title with respect to these errors? Would models with a more accurate thermohaline background state eventually feature a stronger impact of Agulhas leakage on the AMOC for present climate conditions?

I suggest that the authors provide a more comprehensive assessment of the background states of their employed model simulations and implications of potential errors. Moreover, the title and abstract should be modified to include information on the sensitivity of the results to the climatic background state.

Reply: Thanks for this suggestion. In response to this comment, we have acknowledged the dependence of AMOC responses on the climate background in the revised abstract, which reads (Lines 33-37): *“However, the AMOC responses to Agulhas leakage increases may depend on the climate background state. Initialized from a collapsed AMOC, which likely occurred during the glacial period, an increased Agulhas leakage can significantly strengthen the AMOC due to a more unstable AMOC.”* However, as the bulk of our discussions are focused on the present climate, we decide to revise the title to read **“Limited impacts of the Agulhas leakage on the Atlantic Meridional Overturning Circulation in the present climate.”**

In the revised introduction, we also include a discussion about the dependence on climate background states, which read (Lines 82-85): *“We also discuss the dependence on climate background states and found that an increase in Agulhas leakage may have played a more active role in AMOC changes during the glacial period.”*

The dependence on climate background states of the AMOC responses to forcing perturbations is related to the thermohaline structure. For example, previous studies suggested that the sign of freshwater transport by the overturning circulation, across the southern boundary of the Atlantic Ocean, is an indicator for AMOC stability (Arumí-Planas et al., 2024; Rahmstorf, 1996). In the revised supplementary, we include a new section (“**Model assessment**”) that evaluates the thermohaline properties at the southern boundary of the Atlantic Ocean and at the Agulhas Current region. Compared to the coupled CESM, the salinity biases at the southern boundary of the South Atlantic are reduced in our ocean-only simulations forced by observation-based atmospheric boundary conditions, especially in the upper 500 meters (Fig. R1-1a&b). The better simulation of salinity inverts the sign of freshwater transport by the overturning circulation, F_{ov} (Fig. R1-1c), from positive to negative, making it more consistent with observations (Arumí-Planas et al., 2024). Thus, we reckon that our simulations of the present climate are relevant for the real ocean. In the revised manuscript, we added a discussion about this, which reads (Lines 550-555): *“Because the atmospheric forcing in the ocean-only experiment is based on observations, the salinity bias at the southern boundary of the Atlantic Ocean is smaller. The smaller salinity biases lead to a negative freshwater transport by the overturning circulation, F_{ov} , across the southern boundary of Atlantic Ocean in our experiment that is more consistent with observations (Arumí-Planas et al., 2024; Rahmstorf, 1996).”*

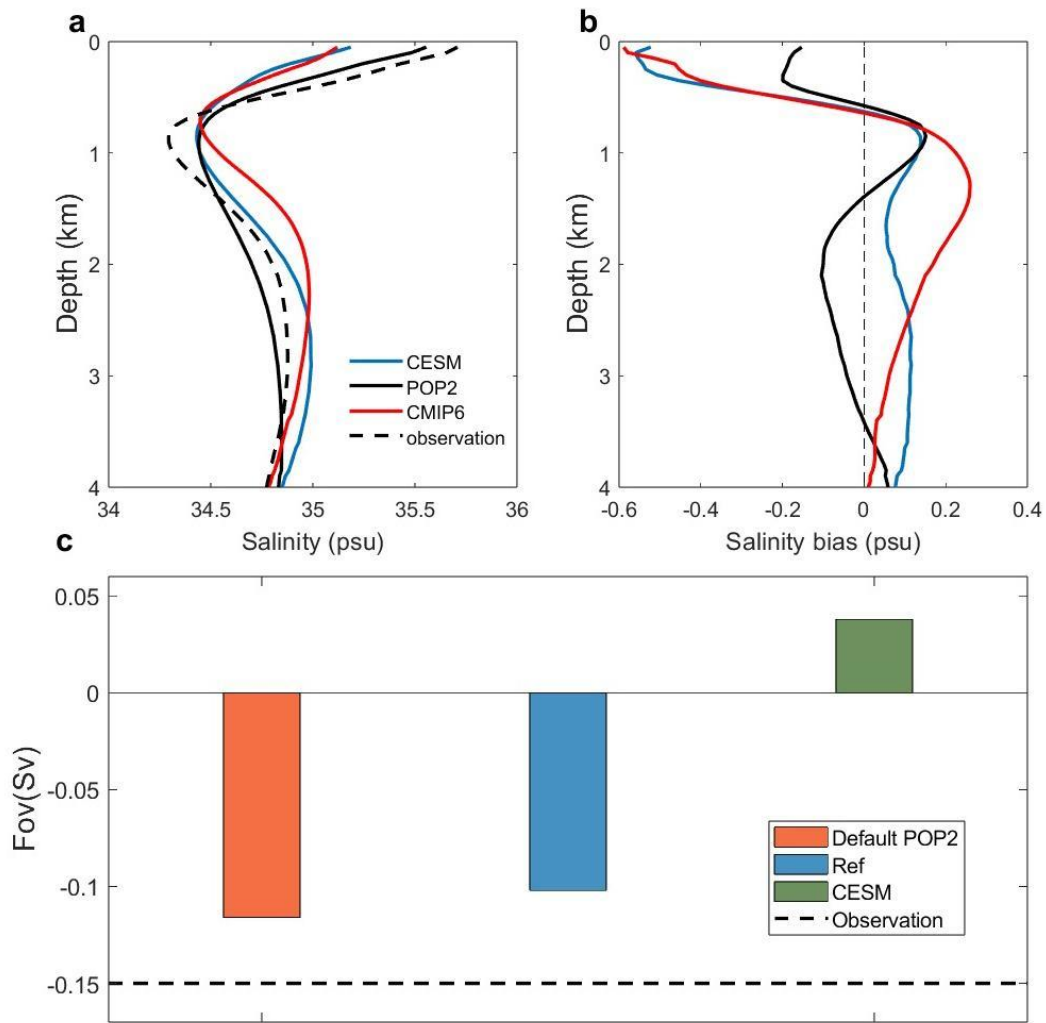


Fig. R1-1 **a**, Vertical salinity distribution, zonally averaged at the southern boundary (34°S) of the Atlantic Ocean. The black dashed line denotes the salinity distribution in Argo observation. **b**, Salinity biases zonally averaged over the southern boundary (34°S) of the Atlantic Ocean. **c**, Comparison of freshwater transport by the overturning circulation, F_{ov} , between our ocean-only POP2 simulations and the fully-coupled CESM simulation.

2. The employed method for the quantification of Agulhas leakage requires validation to ensure the results can be trusted.

If I interpret lns. 434-437 correctly, the authors estimate Agulhas leakage via a section-integrated transport in the south-east South Atlantic, which is not common within the community. A lot of previous studies investigated how to accurately quantify Agulhas leakage in climate models; the estimation of Agulhas leakage via section-integrated transports has been suggested to be inadequate, and Lagrangian methods have become the most established methods (cf., Richardson. 2007; Biastoch et al., 2008; van Sebille et al. 2009; Durgadoo et al., 2013; Cheng et al. 2016; Daher et al., 2020; Schmidt et al., 2021; Rühls et al. 2022).

I strongly suggest recalculating Agulhas leakage using one of the established methods. If this should not be possible, a detailed introduction, motivation for, and validation of the approach chosen by the authors is necessary to be able to trust the presented results.

Reply: Thanks for this suggestion. In the revised manuscript, we recalculate the Agulhas leakage using the Lagrangian method as in Schmidt et al. (2021). The results show that the Agulhas leakage estimated using the Lagrangian method is not significantly different from the Eulerian method at least in our model simulations. We agree that the Eulerian method may not accurately capture the variability of the Agulhas leakage in eddy-permitting/eddy-resolving models (e.g., van Sebille et al. 2009), but the Eulerian method can be used to estimate the climatological strength of Agulhas leakage in low-resolution climate models (see Fig. R1-2b). Nevertheless, we replace the Eulerian Agulhas leakage with the Lagrangian Agulhas leakage in the revised manuscript.

The Lagrangian particle model we used to calculate the Agulhas leakage is the “Probably A Really Computationally Efficient Lagrangian Simulator (Parcels)” (Delandmeter & Van Sebille, 2019). We release particles every ten days at the section of the Agulhas Current Time-series Experiment (ACT, blue line in Fig. R1-2a) at each grid point in the upper 1500 meters. Particles that cross the Good Hope section (red line in Fig. R1-2) are counted as part of the Agulhas leakage.

In the revised manuscript, we included a new section in Methods (“**Agulhas leakage estimate**”) to describe the Lagrangian method to estimate the Agulhas leakage, which reads (Lines 492-506): “*In the modelling experiment, the Agulhas leakage is estimated using a Lagrangian method based on “Probably A Really Computationally Efficient Lagrangian Simulator” (Parcels, Delandmeter & Van Sebille, 2019). We release particles every ten days at the section of the Agulhas Current Time-series Experiment (blue line in Fig. S12a) at each grid point in the upper 1500 meters. The Lagrangian pathway of particles are integrated using the fourth-order Runge-Kutta method. Particles that cross the Good Hope section (red line in Fig. S12a) are count as part of the Agulhas leakage. Due to insufficient output frequency of the velocity field in CMIP6 climate models, it is unfeasible to calculate the Agulhas leakage using the Lagrangian method in each climate models. As such, we use a Eulerian method to estimate the Agulhas leakage. We quantify the Agulhas leakage as the volume transport across the section between (11.5°E, 38.5°S) and (18.5°E, 31.5°S) (green line in Fig. S2b), integrated over the upper 1500 meters. We compared these two methods, and they are not significantly different at least in our model simulations (Fig. S12).*”

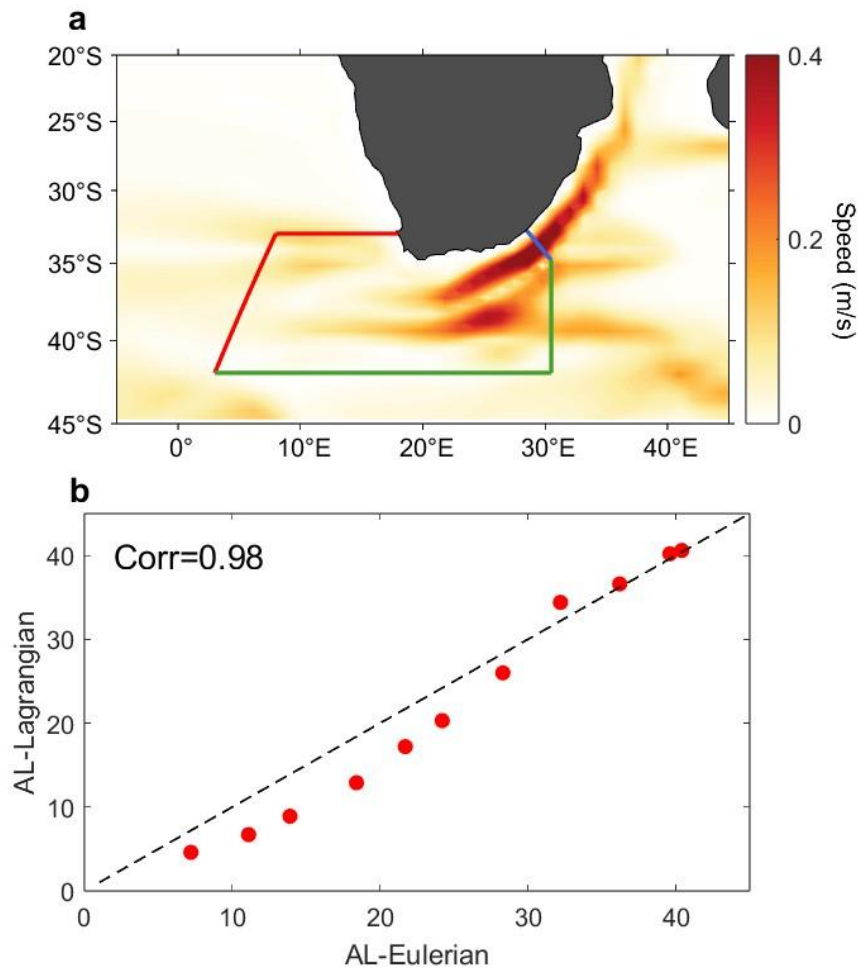


Fig. R1-2 Comparison of the Lagrangian particle method with the volume transport method in estimation of the Agulhas leakage. **a.** Sections for estimating the Agulhas leakage. The background shading denotes climatological speed averaged over the upper 500 meters. The blue line is the ACT section, where we release the particle. The red lines denote the good hope line used to estimate the Agulhas leakage. **b.** Comparison of the Agulhas leakage estimated using the Lagrangian method (y-axis) and the volume transport method (x-axis). The dashed black line denotes $y=x$.

3. The description of the performed ocean-only model experiments is insufficient to fully understand the experiment design (and ensure reproducibility); the respective model data appears to be neither published nor described in a previous manuscript and is also not published (yet?) as part of the current manuscript.

If the ocean-only model experiments indeed have been newly performed for this manuscript, a more extensive experiment description should be provided for the “default setup”, “CTRL”, the 10 experiments with Agulhas leakage increase (e.g., within a supplementary), and the hosing experiments (“Hosing” and Hos28”). These should include information on the initialization, type of wind modifications for

Agulhas leakage increase experiments, and region/strength of hosing for hosing experiments. Moreover, a basic quantitative validation of the CTRL experiment should be added. The provided Extended Data Fig. 8a,b and 9a,b are not sufficient to assess how well the current thermohaline background state and AMOC strength/structure are represented.

I suggest providing such extended experiment descriptions in a supplementary and to include a table or sketch summarizing all the different performed experiments, the introduced abbreviations, and their most important features.

Reply: Thanks for this suggestion. In the revised supplementary, we include a section (“Text S1 **Model setup**”) to describe the experiments and a second section (“Text S2 **Model assessment**”) to provide an assessment of the model.

Minor comments

▪ Ln. 26: *“Agulhas Leakage is expected to increase as the Southern Hemisphere westerly wind shifts poleward” This statement made here as well as elsewhere in the main text with reference to Biastoch et al. (2009) has been contradicted by more recent studies such as Durgadoo et al. (2013) that suggest that Agulhas leakage is increasing primarily due to a strengthening of the westerlies (and that a poleward shift without strengthening of the winds could indeed lead to a reduction of the leakage strength).*

Reply: Thanks for this suggestion. In the revised manuscript, we modified this statement as (Lines 24-25) *“The Agulhas leakage is expected to increase as the Southern Hemisphere westerly winds change under climate warming.”*

▪ Ln. 31: *“we show that a 10 Sv [...] increase in the Agulhas Leakage, typical in climate models, only strengthens the AMOC by a maximum of 1.5 Sv” This statement sounds as if there already exists scientific consensus on the magnitude of the projected increase in Agulhas leakage in state-of-the-art climate models. To my knowledge, prior to the present study, projected future changes in Agulhas leakage have only been quantified in individual climate models with partially conflicting results (e.g., Ivanciu et al. (2022) report an Agulhas leakage increase of 1.5 Sv until the end of the 21st century; Daher & Kirtman (2023) only find a significant increase of Agulhas leakage for a high emission scenario until 2040, but no significant increase for lower emission scenarios and the period after 2040).*

Reply: In response to this comment, we calculate the multi-model mean and standard deviation of Agulhas leakage changes in a warming climate among current climate models. Among the CMIP6 climate models, the multi-model mean of Agulhas leakage increase by 7.1 Sv during the 21st century with a standard deviation of 4.7 Sv. Among the HiresMIP high-resolution climate models, the multi-model mean of Agulhas leakage increase by 6.7 Sv from 1950 to 2050 and the standard deviation is 5.6 Sv. Overall, the climate models consistently project a strengthening in the Agulhas leakage, but its inter-model spread is significant. In the revised manuscript, we

deleted the statement of “*typical in climate models*”.

▪ *Lns. 33-34: “The projected AMOC weakening of order 10 Sv” This statement as well implies some consensus on the order of projected AMOC weakening. However, the magnitude of AMOC weakening greatly differs between climate models and is thought to be a key uncertainty for future climate projections (cf., IPCC AR6).*

Reply: Thanks for this suggestion. Indeed, the magnitude of AMOC weakening differs substantially between different models and scenarios (e.g., Weijer et al., 2020). The climate models consistently project a weakening in the AMOC, but its inter-model spread is significant. For example, the AMOC decline under the high-emission scenario between 4 Sv and 10 Sv in CMIP5 and between 1.5 Sv and 17.5 Sv in CMIP6. In the revised abstract, we deleted the statement of “*of order 10 Sv*”.

▪ *Lns. 51-52: “Due to the thermohaline differences between the Atlantic and Indian Ocean water [...]” This sentence kind of implies that the absolute differences in the thermohaline properties between the Indian Ocean and Atlantic Ocean matter. However, in general, the Atlantic is the basin with the highest salinities in the world ocean and the crucial point is the difference in the thermohaline properties between inflowing and outflowing water masses.*

Reply: The reviewer is right in the sense that the basin-average salinity is indeed higher in the Atlantic than the Indian Ocean. However, it is the local difference in thermohaline properties between the South Atlantic and the Agulhas leakage water that is crucial for the salinity budget in the Atlantic Ocean. In response to this comment, we have revised this statement in the manuscript to read (Lines 51-53): “*Due to the thermohaline differences between the Southeast Atlantic and Agulhas leakage water (Gordon, 1986; de Ruijter et al., 1999), variations in the Agulhas leakage can drive a net salt and heat exchange between ocean basins [...]*”

▪ *Lns. 66-67: “However, the AMOC response to salinity perturbations may differ from that due to an increase in Agulhas leakage” The authors raise a very important point here. However, for clarity, it would be good if this statement could be expanded by potential reasons for the differences.*

Reply: We appreciate the reviewer for this suggestion. In the revised manuscript, we have expanded this discussion, which reads (Lines 68-73): “*However, the thermohaline differences between the Southeast Atlantic and the Agulhas leakage water are most prominent at 500-1000 m depths, not at the sea surface (Guerra et al., 2018; Laxenaire et al. 2019). Thus, thermohaline perturbations applied at the sea surface cannot represent the vertical structure of thermohaline anomalies that may be induced by changes in the Agulhas leakage. As such, the AMOC response to surface perturbations may differ from that due to an increase in the Agulhas leakage.*”

▪ *Lns. 81-81: “due to the compensating effect of warming in the Atlantic Ocean [...], which has been overlooked in previous studies” I would appreciate if the authors would formulate this in less general terms, as some previous studies indeed already*

discussed those potential compensating effects (e.g., Weijer et al., 2002).

Reply: In previous studies (e.g., Weijer et al., 2002), the temperature effect is discussed in experiments that apply a surface temperature anomaly. However, the maximum temperature changes happen in the subsurface, which will not be quickly dissipated into the atmosphere and can affect the subsurface temperature in the North Atlantic. In the revised manuscript, we have modified this statement to read (Lines 149-151): *“This warming compensation was previously discussed (Weijer et al., 2002), but its impact has been underestimated. This occurs because their temperature anomalies were located at the sea surface and quickly damped due to surface heat loss.”*

▪ *Lns. 93-94: “Agulhas leakage is about 7.5 Sv, close to observations” This statement appears to be far-stretched, given that observation-based estimates yield values of 15 Sv in the upper 1000 m (Richardson, 2007) and 21.3 ± 4.7 Sv in the upper 2000 m (Daher et al., 2020).*

Reply: Thanks for this suggestion. In the revised manuscript, we have deleted “close to observations” and added references to these estimates of the Agulhas leakage based on observations (Richardson, 2007; Daher et al., 2020) (Line 96).

▪ *Ln. 100: “a typical 10 Sv increase in the Agulhas Leakage” How do we know it is typical? To my knowledge, there is no consensus (see comment above). The authors should add evidence in form of references or reformulate this statement.*

Reply: Climate models consistently project an increase in the Agulhas leakage in a warming climate, but the inter-model spread is significant. Among CMIP6 climate models, the multi-model ensemble mean of the Agulhas leakage increases by 7.1 Sv during the 21st century with a standard deviation of 4.7 Sv. In the revised manuscript, we modified this statement to read (Lines 103-105), *“a 10 Sv increase in the Agulhas leakage can only strengthen the AMOC by a maximum of 1.5 Sv, achieved after about 150 years, in our simulations (Fig. 1c).”*

▪ *Lns 102-103: “the projected AMOC decline in climate models of order 10 Sv” Also here, to my knowledge, the scientific consensus is still lacking (see comment above). The authors should add evidence in form of references or reformulate this statement.*

Reply: Considering the large inter-model spread of AMOC decline in a warming climate, we deleted the statement of “order 10 Sv”. In the revised manuscript, we revised this statement, which reads (Lines 105-110): *“Considering the magnitude of Agulhas leakage changes projected by climate models (7.1 ± 4.7 Sv) (Fig. S1), the AMOC changes due to Agulhas leakage increases are likely around 1 Sv, with a large uncertainty, under the high-emission scenario in 21st century. This is unlikely going to offset the projected AMOC decline -- the multi-model mean value of AMOC declines in CMIP models under the high-emission scenario is about 8 Sv (Weijer et al., 2020).”*

▪ *Lns 103-105: “When the Agulhas leakage is over 30 Sv, the AMOC strength [...]*

does not respond to the Agulhas leakage increase” Very interesting finding, supporting the idea that the AMOC response to Agulhas leakage changes is dependent on the mean state, as noted already by Weijer and van Sebille (2014). However, isn’t this also suggesting that the current generation of climate models, which – due to their relatively coarse oceanic resolution – may feature a too laminar and hence too strong mean Agulhas leakage strength larger than 30 Sv (extended data Figure 1 indeed supports this statement, if one can trust the chosen method for the calculation of Agulhas leakage), potentially underestimate the impact of Agulhas leakage changes on the AMOC?

Reply: Thanks for this suggestion. In the revised manuscript, we add a brief discussion of Weijer and van Sebille (2014), which reads (Lines 111-114): “*When the Agulhas leakage is over 30 Sv, the AMOC strength levels off and does not respond to the Agulhas leakage increases (red points in **Fig. 1c**). This decoupling between the AMOC and the Agulhas leakage was also noted in a previous study based on a correlation analysis of a climate model (Weijer and van Sebille, 2014).*”

Indeed, the CMIP model may underestimate the impact of Agulhas leakage increases on the AMOC. We discussed this in the revised discussion section, which reads (Lines 253-256): “*Due to the overestimated Agulhas leakage in CMIP models, the AMOC responses to Agulhas leakage increases may also be underestimated (Weijer and van Sebille, 2014) and hence, leading to a slight overestimation of future AMOC declines in response to anthropogenic warming.*”

▪ *Lns 142-144: “[...] we expect this damping to be weaker in coupled models, in which the atmosphere can respond to surface heat fluxes”. It would be helpful to explain why this is to be expected, respective references should be added.*

Reply: Thanks for this suggestion. In the revised manuscript, we explain the atmospheric response, which reads (Lines 159-160): “*In coupled models, we expect this damping to be weaker, as the enhanced surface heat fluxes warm the atmosphere, which, in turn, decreases ocean heat loss (Souza et al., 2014).*”

▪ *Lns 157-159: “the increased Agulhas leakage decreases salinity in the upper Indian Ocean” I feel this relation needs some causal explanation.*

Reply: Thanks for this suggestion. In the revised manuscript, we have modified this statement (Lines 174-179): “*On the other hand, the increased Agulhas leakage from the Indian Ocean would necessarily mean more water entering the Indian Ocean from the Pacific or the Southern Ocean to conserve volume, leading to a decrease in the Indian Ocean upper-ocean salinity (Fig. S6). This effect gradually decreases the efficiency of salty water export from the Indian Ocean and will cause surface freshening in the Atlantic Ocean beyond year 150.*”

▪ *Lns 176-178: “The 10 Sv increase in AMOC strength cannot be explained by the absence of warming compensations. Instead, this contradiction may be reconciled by considering the sensitivity to climate background” I find this a very important*

statement and the corresponding section of the manuscript appears the most relevant to me (see major comment above)

Reply: Thank for this suggestion. In response to this comment and the major comment above, we have revised the abstract and highlighted the critical role of climate background states (see our response to the major comment above).

▪ *Ln. 181: “the AMOC is weakened to around 5 Sv” What has been the initial AMOC strength?*

Reply: In the revised manuscript, it now reads (Line 201): “the AMOC strength is weakened from 17 Sv to around 5 Sv.”

▪ *Lns. 184-185: “After about 600 years, the AMOC fully recovers and strengthens by over 12 Sv.” I am intrigued by the timescale. For the original experiments the AMOC strengthening in response to the Agulhas leakage increase stopped after ca 150 years and afterwards turned into a weakening. The response timescale for the recovery after the housing is much longer. I suggest discussing potential reasons.*

Reply: We appreciate the reviewer for this suggestion. We reckon that the different response timescales are related to the time it takes to advect salinity perturbations from the South Atlantic into the deepwater formation region. In the original experiments, the AMOC is around 17 Sv, much more effective in transporting salinity anomalies from the South Atlantic into the subpolar North Atlantic. In response to this comment, we included the following discussion in the revised manuscript, which reads (Lines 205-208): “The 600-year time scale is much longer than the 150-year response time scale with an active AMOC discussed above. This is because a weaker AMOC is less efficient in transporting salinity anomalies from the South Atlantic into the subpolar North Atlantic, where deep water forms.”

▪ *Lns 193-195: “[...] the collapsed AMOC increases the salinity in the upper South Atlantic, leading to a more negative Fov”. This implies that Fov already has been negative in the CTRL experiment. Yet, based on previous analyses, we know that Fov in the South Atlantic is actually positive in many state-of-the-art climate models including CESM (e.g., Mecking et al., 2017; Jackson et al., 2023). Was the negative Fov achieved here through the initial reduction in Agulhas leakage for the CTRL experiment? (also see comment below)?*

Reply: The reviewer is right that the freshwater transport, F_{ov} , is typically positive in many state-of-the-art climate models, including CESM. In our ocean-only simulations discussed in this study, this freshwater transport bias is corrected by using an atmospheric forcing that is more consistent with observations.

In response to this comment, we estimate the freshwater transport by the overturning circulation across the southern boundary of the Atlantic Ocean in our ocean-only runs, including CTRL and the default POP2 run (no correction for Agulhas leakage), and the coupled CESM model. Consistent with previous studies, the coupled CESM model indeed shows a biased positive F_{ov} . However, the freshwater transport, F_{ov} , is

negative in both ocean-only runs (Fig. R1-1c). We reckon that this bias correction is related to the atmospheric forcing used in our ocean-only runs that is more consistent with observations. Previous modelling experiment also indicates that the coupled climate models can simulate a negative F_{ov} after flux adjustment with observed sea surface salinity and temperature, although its default version simulates a positive F_{ov} (Liu et al., 2014). The correction for the Agulhas leakage does not appear to affect F_{ov} significantly (compare the blue and orange bars in Fig. R1-1c). By comparing the thermohaline structures in the South Atlantic, we show that the improvement in F_{ov} is due to a smaller salinity bias (Fig. R1-1b), especially in the upper 500 m, in our ocean-only runs than the coupled CESM run.

In response to this comment, we added a discussion in the revised manuscript, which reads (Lines 550-555): *“Because the atmospheric forcing in the ocean-only experiment is based on observations, the salinity bias at the southern boundary of the Atlantic Ocean is smaller. The smaller salinity biases lead to a negative freshwater transport by the overturning circulation, F_{ov} , across the southern boundary of Atlantic Ocean in our experiment that is more consistent with observations (Arumí-Planas et al., 2024; Rahmstorf, 1996).”*

▪ *Lns. 198-201: I find this paragraph unnecessary, as the results of F_{total} are not really discussed. I suggest to either expand the discussion or delete the paragraph (and respective parts of the corresponding figure).*

Reply: Thanks for this suggestion. In the revised manuscript, we have deleted the discussion of F_{total} .

▪ *Lns. 224-227: Important conclusion! I would find it necessary though to highlight again that this minimal sensitivity in the models potentially represents an underestimation of the real leakage impact (which also implies that we actually do not know how true the statement of a limited impact of Agulhas leakage on the AMOC actually is?). Yet, this result is not completely new, and a reference of previous studies suggesting this would be appropriate (see comment above).*

Reply: Thanks for this suggestion. In the revised manuscript, we added a discussion on the potential impact on future AMOC changes, which reads (Lines 253-256): *“Due to the overestimated Agulhas leakage in CMIP models, the AMOC responses to Agulhas leakage increases may also be underestimated (Weijer and van Sebille, 2014) and hence, leading to a slight overestimation of future AMOC declines in response to anthropogenic warming.”*

▪ *Ln. 437: What is the default set-up? If already published, official experiment identifier and reference should be provided; otherwise, this experiment needs a more extensive description*

Reply: In response to this comment, we added a section in the supplementary to describe the model set-up (“**Model setup**”).

▪ *Lns. 439-441: Why does a strengthening of the winds lead to a decrease in Agulhas leakage?*

Reply: Previous studies suggest that the Agulhas leakage is controlled by the surface wind stress curl (Durgadoo et al., 2013). By strengthening the zonal local wind (0°E-35°E, 35°S-45°S), Durgadoo et al. (2013) showed that a positive wind stress curl anomaly would increase the Agulhas leakage. In this study, to weaken the Agulhas leakage without directly affecting the Southern Ocean circulation, we increase the zonal wind stress only in the Agulhas leakage region (10°E-40°E, 35°S-40°S). Therefore, a strengthening of local winds in our experiments represents a negative wind stress curl anomaly in the Southern Ocean latitudes and hence leads to a decrease in Agulhas leakage, broadly consistent with Durgadoo et al. (2013). In the revised manuscript, we add this statement about wind stress, which read (Lines 473-474): *“This regional change in surface wind is confined to the Agulhas leakage region to avoid significant modifications to the Southern Ocean upwelling ...”*

▪ *Lns. 441-443: This would be a good place to describe a potential relation between biases in the thermohaline properties and Fov (i.e. which sign has Fov before/after “fixing the biases” for the CTRL experiment?)*

Reply: In response to this comment, we included a discussion about the freshwater transport across the southern boundary of the Atlantic Ocean, which reads (Lines 550-555): *“Because the atmospheric forcing in the ocean-only experiment is based on observations, the salinity bias at the southern boundary of the Atlantic Ocean is smaller. The smaller salinity biases lead to a negative freshwater transport by the overturning circulation, F_{ov} , across the southern boundary of Atlantic Ocean in our experiment that is more consistent with observations (Arumí-Planas et al., 2024; Rahmstorf, 1996).”*

▪ *Lns. 445-448: “Agulhas leakage is modified to [...]” How? How much and in which way have the wind fields be altered to achieve these results?*

Reply: In response to this comment, we include a section (“**Model setup**”) in the supplementary to provide a detailed description of the experiment set-up.

▪ *Ln. 462: “[...] density difference between the deep-water formation region and the southern interior Atlantic [...]” I find this formulation misleading, as the southern interior Atlantic is defined over a box in the mid-latitude northern Atlantic. How was this region chosen?*

Reply: In this study, we use the thermal wind relation to reconstruct the AMOC streamfunction at around 45 °N. This follows the formulation of Nikurashin and Vallis (2012) to relate the overturning circulation streamfunction to the density difference between the basin interior and the deep-water formation region. In this formulation, the meridional volume transport is related, by volume conservation, to the zonal volume transport, which can be reconstructed from the meridional density gradient in the North Atlantic. Here, we choose the regions north of 45 °N to represent the deepwater formation region and the region south of 45 °N to represent the interior

basin. Similar approaches have been used in previous studies to reconstruct AMOC changes from temperature and salinity fields (e.g., Bonan et al., 2022).

In the revised manuscript, we have modified this statement, which now reads (Lines 511-512): *“the density difference between the deep-water formation region and the subtropical northern Atlantic.”*

Figures

- *All figures: I suggest to unify font sizes and font types*

Reply: Modified as suggested. We have unified font types/sizes in the revised figures.

- *Figure 1:*

o adjust topographie in (a) to match data and depth level

o in legend for (a) add type of observational source

Reply: Modified as suggested.

Formalities

- *While the manuscript is not difficult to read, it contains quite a few minor grammatical mistakes that should be corrected (e.g., a lot of either missing or extensive use of “the” and “a”, as well as confusions regarding singular and plural forms)*

- *Agulhas leakage -> Agulhas leakage*

Reply: We have carefully revised the text and tried our best to resolve these grammatical mistakes.

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Reply to the second reviewer

We thank the reviewer for these helpful comments. We appreciate the time and efforts the reviewer invested in reviewing the previous version of the manuscript. We have addressed the reviewer's comments, which are in blue and italics, in the revised manuscript. Our replies are in black following each of the reviewer's comments.

Reviewer #2

Main comment:

Numerical simulations from model allow for testing several hypothesis which is very important to understand the processes that control large dynamics variability such as the AMOC. However, I do believe one important point to trust model outputs is the validation against observations. For example, the authors compare the transport by Agulhas Leakage from their "CTRL" experiment with estimations from observation, which is great especially because changing the surface zonal wind stress reduces the modeled Agulhas Leakage closer to observations. However there are no details in the text about the MOC transport estimate or how the model estimate the ACC transport (review of observation-based estimation in Cessi (2019)). The later controls the main exchanges between the Atlantic and Indian Ocean, so it seems reasonable to expect the model to well represent the ACC flow. The minor comments below may be linked and clarify this point:

Reply: Thanks for this suggestion. In CTRL, the ACC volume transport across the Drake Passage is about 119 Sv, slightly smaller than observations (Whitworth et al., 1985; Donohue et al., 2016). In CTRL, the AMOC strength is about 17 Sv, close to observation-based estimates (e.g., Cessi 2019; Rousselet et al. 2021). In this study, we primarily focus the impact of salty and warm Indian Ocean subtropical water on the AMOC, not all the water exchanges between the Atlantic and Indian Ocean. Therefore, the ACC volume transport is probably not going to directly affect the Agulhas leakage (Cessi and Jones, 2017). In the revised manuscript, we added a description of the AMOC and ACC in the method, which reads (Lines 477-580): *"In Ref, the AMOC strength is about 17 Sv, close to observation-based estimates (Cunningham, 2007; Cessi 2019; Rousselet et al. 2021). The volume transport of the Antarctic Circumpolar Current (ACC) across the Drake Passage is about 119 Sv, slightly smaller than observations (Whitworth et al., 1985; Donohue et al., 2016)."*

Minor comments:

L 32: "typical in climate model" predictions?

Reply: In response to this comment and comments from another reviewer, we estimated the Agulhas leakage changes in a warming climate among CMIP climate models. Among the CMIP6 climate models, the multi-model ensemble mean of Agulhas leakage increases in the 21st century is around 7.1 Sv with a standard

deviation of 4.7 Sv. Considering the large inter-model spread and the length limit of the abstract, we have deleted this statement in the revised manuscript.

L 86-88: Does this change affect the vertically integrated zonal transport of the ACC since this current is a major component of the exchanges between Indian and Atlantic oceans Forget et al. (2015).

Reply: The ACC transport stays roughly constant as we increase the Agulhas leakage – it decreases by only 0.4 Sv for a 10 Sv increase in the Agulhas leakage (Fig. R2-1). In the experiment, we only modify surface wind local to the Agulhas leakage region, while the ACC is likely controlled by the circumpolar integration of surface wind stress (Allison et al., 2010)

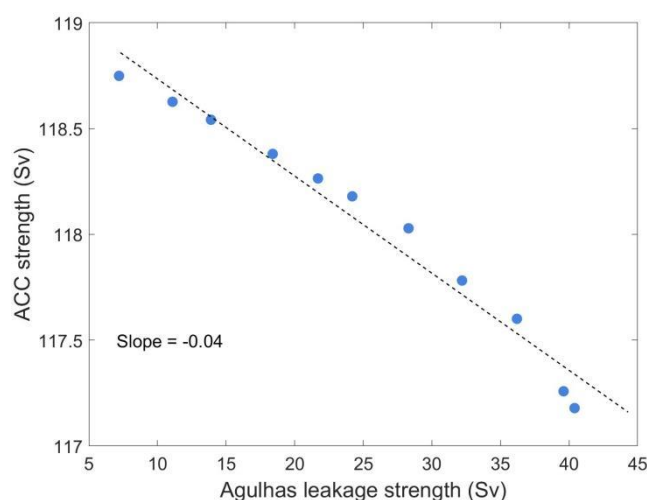


Fig. R2-1 Changes in the Antarctic Circumpolar Current (ACC) transport as we increase the Agulhas leakage in our ocean-only experiments. The dashed line denotes its linear regression.

L 92-95: Can you please specify here the AMOC estimate from the CTRL experiment ? From Figure I would say about 17 Sv but not sure. Also how this value compares with estimates from observations (Cessi (2019)) ? More generally in this section, I would suggest to add a small description of the model characteristics to help the reader.

Reply: Thanks for these suggestions. In the revised manuscript, we included the climatological AMOC streamfunction in CTRL in Fig 1b and discussed the AMOC strength, which reads (Lines 96-97): “The AMOC strength in “Ref” is about 17 Sv, close to observation-based estimates (Cunningham, 2007; Cessi 2019; Rousselet et al. 2021).” Besides, we also compare the AMOC streamfunction at 26.5N with RAPID. The result indicates that the AMOC is largely reproduced in our experiments (Fig. R2-2).

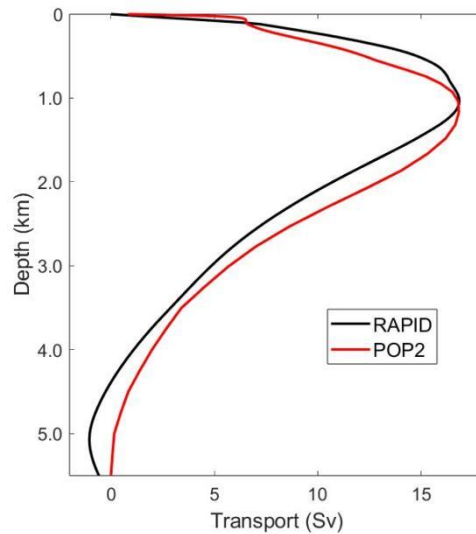


Fig. R2-2 Comparison of the AMOC streamfunction in 26.5 °N between model simulations (which experiment is this?) and RAPID.

Section "Atlantic warming compensates for salinity increase": The authors use the AL28 experiment since it is the most likely scenario based on a 10 Sv AL increase. It should state more clearly.

Reply: Thanks for this suggestion. In the revised manuscript, to be consistent with the observed Agulhas leakage, we compare the experiments with Agulhas leakage of 17.2 Sv ("AL17") and 26.5 Sv ("AL26") to illustrate the mechanism. The Agulhas leakage in AL17 is close to the observation-based estimate and the Agulhas leakage increase from AL17 to AL26 is close to the multi-model ensemble-mean Agulhas leakage changes projected by CMIP models under the high-emission scenario. In the revised manuscript, we have revised this statement to read (Lines 119-125): "To demonstrate this compensating effect, here we compare two experiments, "AL17" and "AL26", in which the Agulhas leakage is 17.2 Sv and 26.5 Sv, respectively. The Agulhas leakage in AL17 is close to the observation-based estimate (Richardson, 2007; Daher et al., 2020) and the Agulhas leakage increase from AL17 to AL26 is close to the multi-model ensemble-mean Agulhas leakage changes projected by CMIP models under the high-emission scenario (Fig. S1)."

L 133-134: Is this warming only due to increased volume of Indian Ocean entering North Atlantic or due to other processes? What can we expect in the global warming context?

Reply: In the perturbation experiments, we only change local surface wind to modify the volume transport of Agulhas leakage. Thus, we expect that most of the Atlantic warming is caused by the increased volume of Indian Ocean water entering North Atlantic.

In the global warming context, the increased Agulhas leakage may also contribute to

Atlantic warming, but it is likely weaker than contributions due to surface heat uptake, e.g., in the subpolar North Atlantic.

L 178-184: Again here, I don't understand why the authors picked the specific value of 5 Sv to represent a weakened AMOC? Is it arbitrary?

Reply: In the hosing experiment, we added an additional 0.06 Sv of freshwater to the subpolar North (50-60°N, 60°W-0°). After 200 years, the AMOC weakens to 5 Sv to represent a collapsed state. It is somewhat arbitrary, but we don't think this will affect our results.

In the revised manuscript, we describe this experiment design in detail, which reads (Lines 485-490): “The “hosing” experiment (“Hosing”) was designed to represent a collapsed AMOC state. This is achieved by applying a freshwater perturbation of 0.06 Sv in the subpolar North Atlantic (50-60°N, 60°W-0°), initialized from Ref at year 601. Branched from Hosing at Year 200, when the AMOC has collapsed, we conduct another experiment that runs in parallel with Hosing but with increased Agulhas leakage to 26 Sv (“Hos26”).”

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Reply to the third reviewer

We thank the reviewer for these helpful comments. We appreciate the time and efforts the reviewer invested in reviewing the previous version of the manuscript. We have addressed the reviewer's comments, which are in blue and italics, in the revised manuscript. Our replies are in black following each of the reviewer's comments.

Reviewer #3

(1) Improve phrasing and classification of study compared to previous studies: After reading the abstract I had the impression that the impact of AL on the AMOC in the future is analysed which is misleading. It needs to be clarified that sensitivity experiments with an artificially changed AL are being analysed. If space allows including the result from the experiment with the collapsed AMOC would be a great addition to the abstract. Furthermore, the Introduction is not comprehensive and studies addressing the same topic are not discussed in much detail. Although this study quantifies the impact of AL on the response of the AMOC and its lower limb for the first time, other studies have already quantified the impact of AL on the upper limb of the AMOC. I also noticed that some studies were not cited for the right reason (see specific comments below).

Reply: Thanks for these suggestions. In response to this comment, we have revised the abstract to make it clear the experiments represent a hypothetical change in the Agulhas leakage. We also discussed results from the experiments with a collapsed AMOC in the revised abstract (Lines 33-37).

We also expanded the discussion about previous studies in the introduction, which reads (Lines 65-72): *“Previous studies that explore the influence of changes in the Agulhas leakage on the AMOC typically apply a surface salinity/temperature perturbation in the southeast Atlantic Ocean in models. However, the thermohaline differences between the Southeast Atlantic and the Agulhas leakage water are most prominent at 500-1000 m depths, not at the sea surface (Guerra et al., 2018; Laxenaire et al. 2019). Thus, thermohaline perturbations applied at the sea surface cannot represent the vertical structure of thermohaline anomalies that may be induced by changes in the Agulhas leakage. As such, the AMOC response to surface perturbations may differ from that due to an increase in the Agulhas leakage.”*

We have also revised the text to make sure we cite the right references (see response below).

(2) Does the coarse model capture important dynamics? I do acknowledge that the amount and runtime of sensitivity experiments used in this study is not feasibly with

eddy-resolving models. However, to simulate the exchange between the Indian and Atlantic Ocean not only quantitatively but also qualitatively correct a horizontal resolution finer than at least 1° is required (De Ruijter et al., 1999; Biastoch et al., 2008; Beal et al., 2011). You are correcting for the bias in AL strength, but the dynamics might not be well simulated. I am concerned that thermohaline properties from the Indian Ocean are not correctly advected in the absence of eddies (and possibly Agulhas Rings). The thermohaline properties reaching the North Atlantic might be too diluted in your model. An expanded model evaluation in the manuscript is needed: Are there any Agulhas Rings and if so, what are their characteristics? How does temperature and salinity in the western, tropical South Atlantic compare to observations? Show maps (like in Fig. 1a) of biases between ctrl/ AL28 and observations for different depths.

Reply: The reviewer raised a potentially important caveat in our study. We agree with the reviewer that higher resolutions are needed to resolve Agulhas rings and the thermohaline changes due to increases in the Agulhas leakage may be more diluted in the North Atlantic, when compared to eddy-resolving models. However, we think the processes discussed in this study is still relevant in the real ocean, as discussed below.

Indeed, our ocean model has a nominal horizontal resolution of 1° and does not resolve Agulhas rings. Thus, the water exchange between the Indian and Atlantic Ocean are in the form of mean flow. We release Lagrangian particles from the section of the Agulhas Current Time-series Experiment (ACT) to show the pathway by which the Agulhas leakage water enters the Atlantic (Fig. R3-1). The Lagrangian pathway resembles that in the eddy-resolving models (Biastoch et al., 2009 & Fig. R3-7c), although it is by advection rather than Agulhas rings.

We also compared the temperature and salinity distributions in the South Atlantic between our model simulations and observations. Broadly speaking, our model largely reproduces the salinity and temperature distribution in observations (Fig. R3-2 & Fig. R3-3). Following an increase in the Agulhas leakage (Ref vs AL26), there is an increase in subsurface temperature and salinity over the whole south Atlantic Ocean. In the western tropical South Atlantic, a stronger Agulhas leakage significantly increases salinity and temperature, especially at depths of 300-1000 m (Fig. R3-4). Thus, even though our model does not capture the eddy dynamics with Agulhas rings, we can roughly represent the consequences after an increase in the Agulhas leakage.

Nevertheless, we recognize that the thermohaline signal might indeed be diluted due to unresolved eddies, but we expect the physical processes, by which the Agulhas leakage impacts the AMOC, to be similar between our model and eddy-resolving models. In the revised manuscript, we include a discussion about this caveat, which reads (Lines 238-244): “Our ocean models cannot resolve meso-scale eddies, including the Agulhas rings. Thus, the water exchange between the Indian Ocean and the South Atlantic is in the form of mean flows. The thermohaline properties reaching

the North Atlantic may be diluted in our model experiments, leading to underestimated thermohaline responses (Weijer and van Sebille, 2014). However, we reckon that the processes discussed in this study are still relevant for the real ocean. In the future, high-resolution modelling experiments are still needed to address this caveat.”

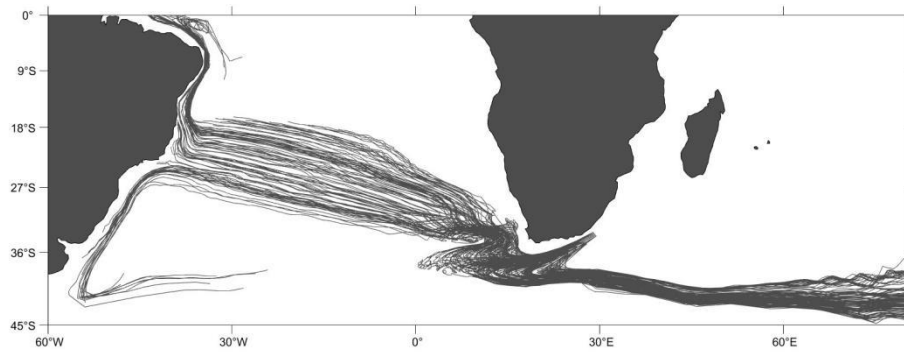


Fig. R3-1 Trajectories of Lagrangian particles released from the section of the Agulhas Current Time-series Experiment (ACT).

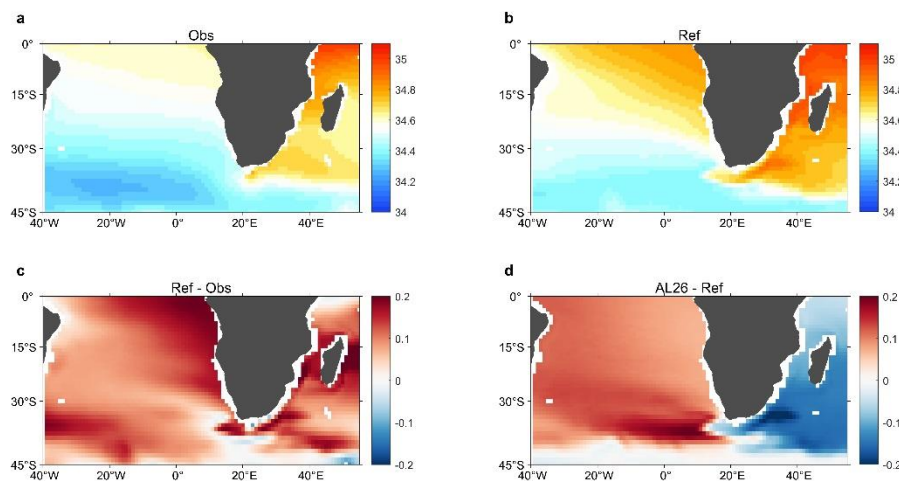


Fig. R3-2 Comparison of salinity distribution between models and Argo. **a**, salinity distribution in Argo observation averaged from 500 to 1200 meters. **b**, Same as a, but for the salinity distribution in the reference run. **c**, the salinity difference between the reference run and Argo observation averaged from 500 to 1200 meters. **d**, the salinity difference between the AL26 and the reference run.

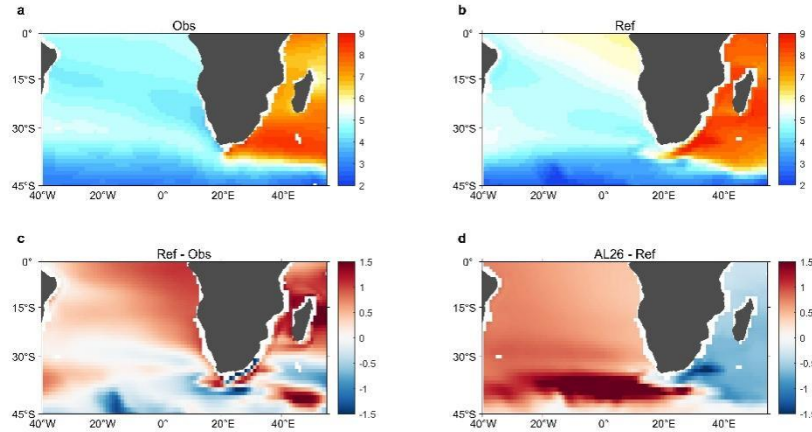


Fig. R3-3 Same as Fig R3-2, but for the temperature difference.

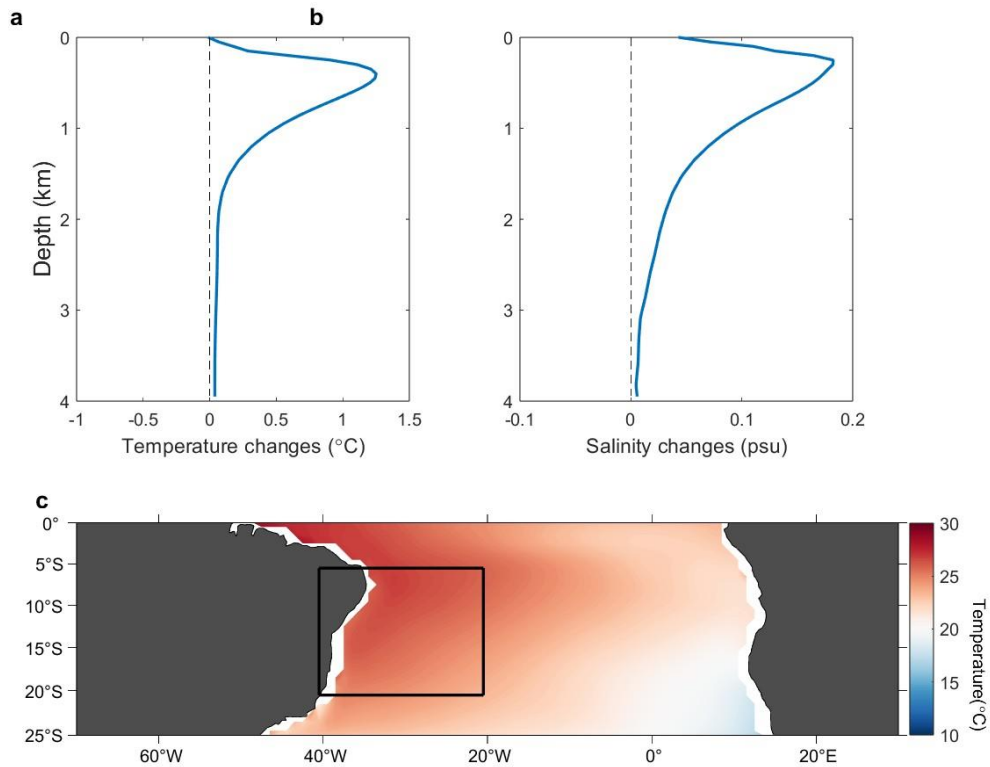


Fig. R3-4 Western tropical southern Atlantic thermohaline changes. **a**, Vertical distribution of temperature difference of the western tropical southern Atlantic (40 °W-20°W, 5°S-20°S) between AL26 and Ref. **b**, Same as **a**, but for the salinity difference. **c**, the black box in **c** denotes the region used to average the western tropical southern Atlantic thermohaline property.

(3) Methodology to estimate AL: The most widely used and very established method to estimate AL is to use a Lagrangian approach (e.g. Biastoch et al., 2008; van Sebille et al., 2009; Rimaud et al., 2012; Schmidt et al., 2021). The Eulerian field is not suitable

for estimating the exchange between the Indian and Atlantic Ocean (van Sebille et al., 2010) due to the turbulent nature of the Cape Basin and the proximity to the spatially varying position of the Agulhas Return Current and Antarctic Circumpolar Current. Lagrangian experiments, if run long enough, would additionally shed light on the evolution of thermohaline properties during the advection into the North Atlantic.

Reply: Thanks for this suggestion. In the revised manuscript, we recalculate the Agulhas leakage using the Lagrangian method as in Schmidt et al. (2021). The Lagrangian particle model we used to calculate the Agulhas leakage is the “Probably A Really Computationally Efficient Lagrangian Simulator (Parcels)” (Delandmeter & Van Sebille, 2019). We release particles every ten days at the section of the Agulhas Current Time-series Experiment (ACT, blue line in Fig. R3-5a) at each grid point in the upper 1500 meters. Particles that cross the Good Hope section (red line in Fig. R3-5) are counted as part of the Agulhas leakage. The results show that the Agulhas leakage estimated using the Lagrangian method is not significantly different from the Eulerian method at least in our model simulations. We agree that the Eulerian method may not accurately capture the variability of the Agulhas leakage in eddy-permitting/eddy-resolving models (e.g., van Sebille et al. 2009), but the Eulerian method can be used to estimate the climatological strength of Agulhas leakage in low-resolution climate models (see Fig. R3-5b). Nevertheless, we replace the Eulerian Agulhas leakage with the Lagrangian Agulhas leakage in the revised manuscript.

In the revised manuscript, we included a new section in Methods (“Agulhas leakage estimate”) to describe the Lagrangian method to estimate the Agulhas leakage, which reads (Lines 492-506): *“In the modelling experiment, the Agulhas leakage is estimated using a Lagrangian method based on “Probably A Really Computationally Efficient Lagrangian Simulator” (Parcels, Delandmeter & Van Sebille, 2019). We release particles every ten days at the section of the Agulhas Current Time-series Experiment (blue line in Fig. S12a) at each grid point in the upper 1500 meters. The Lagrangian pathway of particles are integrated using the fourth-order Runge-Kutta method. Particles that cross the Good Hope section (red line in Fig. S12a) are count as part of the Agulhas leakage. Due to insufficient output frequency of the velocity field in CMIP6 climate models, it is unfeasible to calculate the Agulhas leakage using the Lagrangian method in each climate models. As such, we also use a Eulerian method to estimate the Agulhas leakage. We quantify the Agulhas leakage as the volume transport across the section between (11.5°E, 38.5°S) and (18.5°E, 31.5°S) (green line in Fig. S2b), integrated over the upper 1500 meters. We compared these two methods, and they are not significantly different at least in our model simulations (Fig. S12).”*

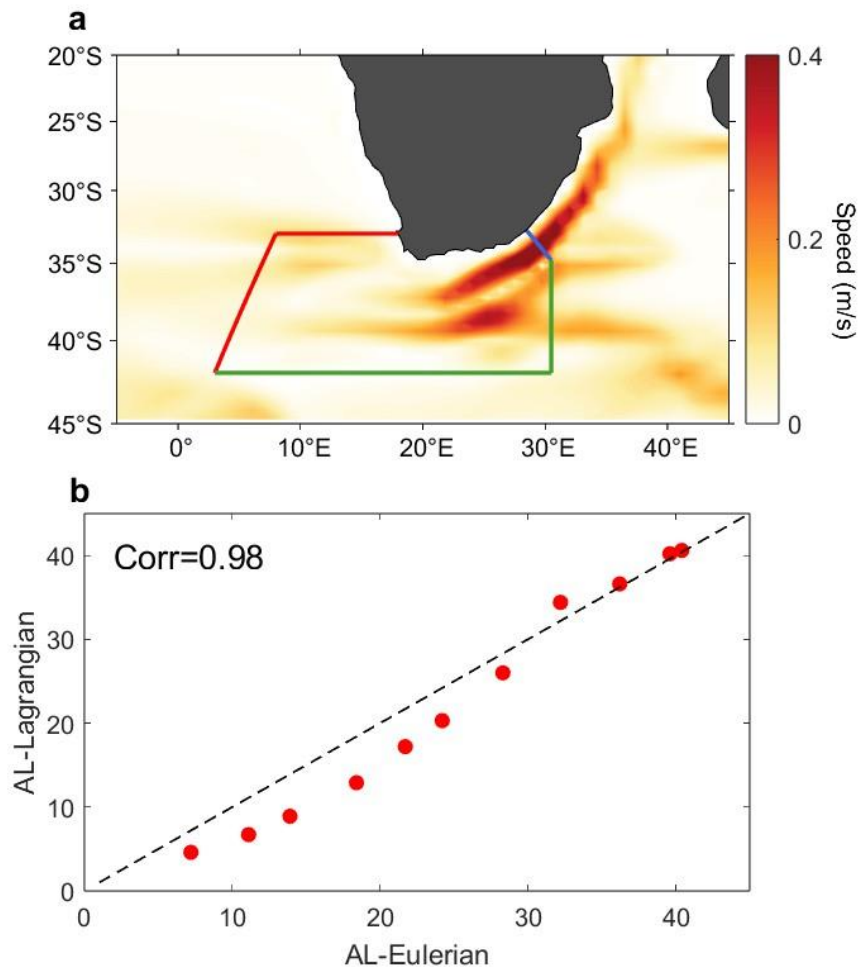


Fig. R3-5 Comparison of the Lagrangian particle method with the Eulerian volume transport method in estimation of the Agulhas leakage. **a.** Sections for estimating the Agulhas leakage. The background shading denotes climatological speed averaged over the upper 500 meters. The blue line is the ACT section, where we release the particle. The red lines denote the good hope line used to estimate the Agulhas leakage. **b.** Comparison of the Agulhas leakage estimated using the Lagrangian method (y-axis) and the volume transport method (x-axis). The dashed black line denotes $y=x$.

(4) Effect of heat vs salt on density: One of the main conclusions is that the effect of salinity on density is compensated by warmer temperatures and that this compensating effect has apparently been “overlooked” (l. 81) in previous studies. But Weijer et al. (2002) found that “Although Agulhas heat and salt input initially balance with respect to their effect on density, it is due to the strong thermal relaxation that a density anomaly can develop on its way north.” Also, Biastoch et al. (2013) concluded that “Changes in the upper-ocean water mass signatures are mainly restricted to salinity, since temperature anomalies are effectively damped by the surface heat fluxes (Figure 4b).” This needs better discussion. Furthermore, I am

wondering why there is no increased heat loss to the atmosphere in the South Atlantic. What is the temporal evolution of heat loss? In an eddy-resolving model Rñhs et al., (2019) found that Lagrangian particles of AL origin reach the mixed layer at least once in the South Atlantic which led to a salinification and cooling. Chang et al., (2020) found that in the high-resolution version of CESM the surface heat loss is better represented as the SST fronts are more realistic compared to the low-resolution version you used. How does the heat loss and SST fronts compare to observations? Overall, I have major concerns and think that making use of the high-resolution version of CESM or other models in combination with Lagrangian experiments would be needed to ensure robustness of the results.

Reply: The reviewer raised an important point that was not clarified well in the original manuscript. In Weijer et al. (2002), the impacts of Agulhas leakage increases are represented as a surface temperature/salinity perturbation in the Southeast Atlantic. As such, the surface warming is quickly damped due to the relaxation boundary condition, leaving a density anomaly mainly due to salinity increases. Similar surface temperature damping and increased surface heat loss are also present in Biastoch et al. (2013) and our simulations. In our simulations, there is significant surface warming in the Southeast Atlantic, but no significant surface warming in other areas of the South Atlantic Ocean, which is damped by the enhanced surface heat loss in the Southeast Atlantic Ocean (Fig R3-6). In the revised manuscript, we include a discussion about the sea surface heat flux, which reads (Lines 156-158): *“Similar processes occur in the Southeast Atlantic Ocean, the strengthened Agulhas leakage induces significant sea surface warming, which is quickly damped due to surface heat loss (Fig. S5) (Weijer et al. 2002; Biastoch et al., 2013).”*

However, in our simulations, we found that the thermohaline responses to Agulhas leakage increases are most prominent in the subsurface, not in the sea surface. Thus, it is subject to less damping effect as compared to the surface. In Biastoch et al. (2013), there is also substantial subsurface warming in the South Atlantic (see Figure 4d in Biastoch et al. 2013). The significant subsurface warming is consistent with the structure of Agulhas Rings. By tracking the evolution of the thermohaline properties in Agulhas rings, Laxenaire et al. (2019) found the Agulhas Ring induces significant subsurface warming from 200 to 1000 meters depth, not in the sea surface.

We agree that the SST front and surface heat loss could be better simulated in the high-resolution version of CESM. However, the subsurface warming below the surface mixed layer would not be damped significantly as in the surface. To verify this, we show a composite of temperature and salinity evolutions of Agulhas Rings in the high-resolution CESM. We identify 412 Agulhas Rings during 150 model years of the pre-industrial simulation. As the rings travels toward the west in Fig. R3-7, surface warming rapidly decreases, but the subsurface warming persists during the course, consistent with our low-resolution simulations. In the revised supplementary (section **“model assessment”**), we include the discussion about temperature evolution in

high-resolution CESM.

Nevertheless, we agree with the reviewer that the coarse resolution of our model brings uncertainty with it. In response to this comment, we acknowledge this caveat in the revised manuscript, which reads (Lines 238-244): *“Our ocean models cannot resolve meso-scale eddies, including the Agulhas rings. Thus, the water exchange between the Indian Ocean and the South Atlantic is in the form of mean flows. The thermohaline properties reaching the North Atlantic may be diluted in our model experiments, leading to underestimated thermohaline responses³⁹. However, we reckon that the processes discussed in this study are still relevant for the real ocean. In the future, more high-resolution modelling experiments are still needed to address this caveat.”*

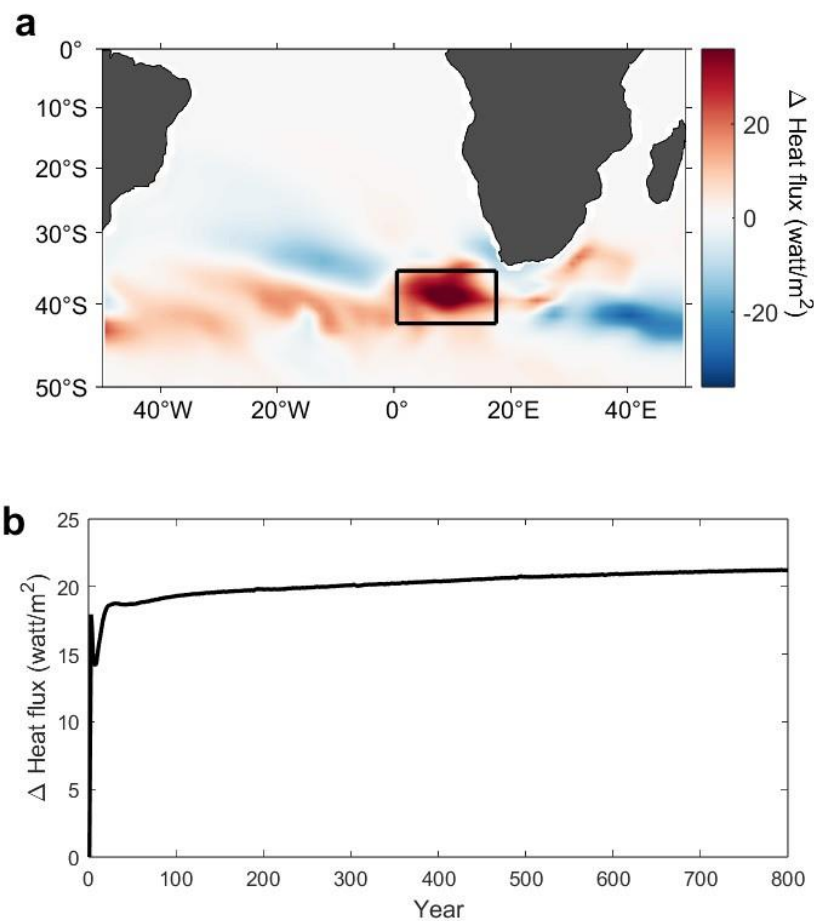


Fig. R3-6 **a**, Sea surface heat flux changes (positive value indicates more ocean loss heat) after strengthening the Agulhas leakage. The value is calculated as difference of sea surface heat flux between the experiments with Agulhas leakage of 26.5 Sv (AL26) and Agulhas leakage of 17.2 Sv (AL17). **b**, Evolution of sea surface heat flux averaged over the black box in **a** (0°-17 °E, 35 °S-45 °S).

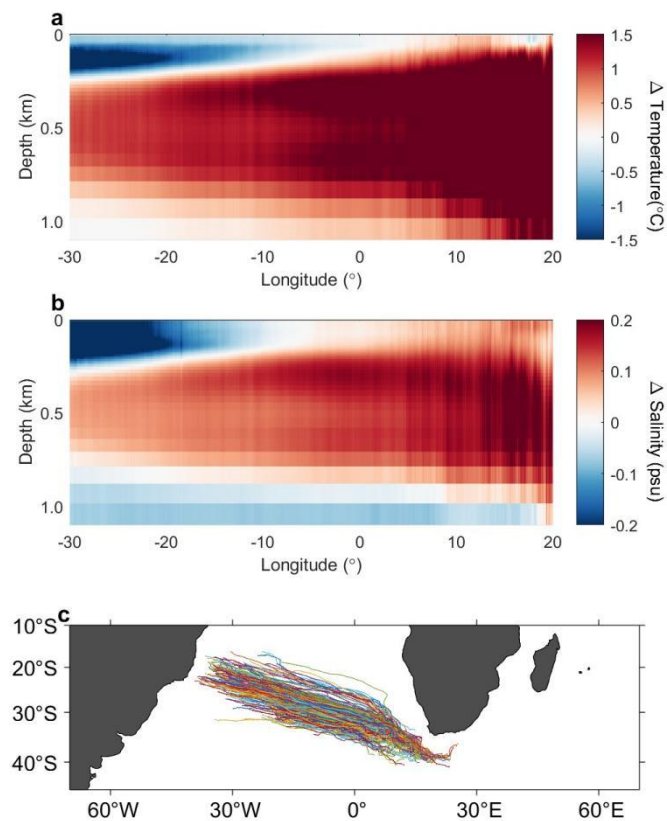


Fig. R3-7 Thermohaline evolution of the Agulhas Ring in the high-resolution CESM. **a**, Temperature anomaly evolution of the Agulhas Rings during their journey westward. The temperature anomaly is calculated as the difference between the temperature in Agulhas Rings with the local climatological temperature. **b**, Same as **a**, but for the salinity anomaly. **c**, trajectories of Agulhas rings used to obtain the composite evolution of thermohaline anomaly.

Specific comments

l. 49 Please expand on what Agulhas Leakage is.

Reply: Thanks for this suggestion. In the revised manuscript, we added a description of the Agulhas leakage, which reads (Lines 47-49): “The Agulhas leakage, an inflow of warm, salty water from the Indian Ocean into the South Atlantic, is a crucial component of the global ocean overturning circulation.”

l. 58 Rñhs et al., (2022) only analyse past evolution of AL but not the future. Please revise other citations to make sure studies are cited for the right reason. Durgadoo et al., (2013) who have shown that AL predominantly responds to the westerlies has to be referred to.

Reply: In response to this comment, we have revised the citation in the revised manuscript (Line 58). Also see discussions in response to a similar comment below.

l. 64 and l. 135 Weijer et al., (2002) apply both a salinity and temperature perturbation.

Reply: In response to this comment, we revise the statement about previous studies, which reads (Lines 65-73): *“Previous studies that explore the influence of changes in the Agulhas leakage on the AMOC typically apply a surface salinity/temperature perturbation in the southeast Atlantic Ocean in models (Weijer et al., 2002, Nuber et al. 2023, Cimadoribus et al. 2023). However, the thermohaline differences between the Southeast Atlantic and the Agulhas leakage water are most prominent at 500-1000 m depths, not at the sea surface (Guerra et al., 2018; Laxenaire et al. 2019). Thus, thermohaline perturbations applied at the sea surface cannot represent the vertical structure of thermohaline anomalies that may be induced by changes in the Agulhas leakage. As such, the AMOC response to surface perturbations may differ from that due to an increase in the Agulhas leakage.”*

(Lines 149-151) *“This warming compensation was previously discussed (Weijer et al., 2002, but its impact has been underestimated. This occurs because their temperature anomalies were located at the sea surface and quickly damped due to surface heat loss.”*

ll. 74-75: There is only an increase in AL of ~6 Sv in CMIP6 (Ext. data Fig. 1) which you probably refer to with “climate models” in l. 75. Only the High-res. CESM has an increase of 10 Sv. Therefore 10 Sv are probably more realistic, but it needs to be clearer what you refer to.

Reply: In response to this comment and comments from another reviewer, we calculated the multi-model mean and standard deviation of the Agulhas leakage changes in a warming climate in CMIP6 models under the high-emission scenario. The multi-model ensemble mean of Agulhas leakage increase over the 21st century is around 7.1 Sv with a standard deviation of 4.7 Sv. Among the HiresMIP climate models, the multi-model ensemble mean of the Agulhas leakage increase is 6.7 Sv from 1950 to 2050 and the standard deviation is 5.6 Sv. Overall, the climate models consistently project a strengthening in Agulhas leakage, but with substantial inter-model spread. In the revised manuscript, we have modified this statement, which reads (Lines 103-105): *“When the Agulhas leakage is lower than 30 Sv, a 10 Sv increase in the Agulhas leakage can only strengthen the AMOC by a maximum of 1.5 Sv, achieved after about 150 years, in our simulations”*

l. 93 The only two observational estimates (citing R  hs et al., (2022) is misleading here) suggest an AL strength of at least 15 Sv (Richardson et al., 2007) and 21.3 ± 4.7 Sv (Daher et al., 2020) considering the transport in the upper 1500 m and 2000 m, respectively. Therefore, I would suggest using the experiment with appr. 15-20 Sv as your CTRL experiment.

Reply: Thanks for this suggestion. When the Agulhas leakage is smaller than 30 Sv, the relationship between the Agulhas leakage and the AMOC is linear. Therefore,

using small Agulhas leakage as CTRL experiment will not affect the results.

In the revised manuscript, we deleted “close to observations” and revise this statement to read (Lines 95-96): *“For the reference run (“Ref”), the Agulhas leakage is about 4.6 Sv. This is weaker than the observation-based estimate, at around 15 Sv (Richardson et al., 2007, Daher et al., 2020).”*

Additionally, we now compare the two experiments with Agulhas leakages of 17.2 Sv (AL17) and 26.5 Sv (AL26) to illustrate the mechanism. The Agulhas leakage in AL17 is close to the observation-based estimate and the Agulhas leakage increase from AL17 to AL26 is close to the multi-model ensemble-mean Agulhas leakage changes projected by CMIP models under the high-emission scenario.

1. 95 What does “roughly” constant mean. Please provide details in the methods section e.g. include one standard deviation in l. 444.

Reply: In the revised manuscript, we add the standard deviation (see Table S1 in supplementary). Because the model is forced by climatological dataset, the inter-annual variability of Agulhas leakage is small.

11. 98-100 A 10 Sv increase in AL is not typical for CMIP6 models (see comment on ll. 74-75)

Reply: The reviewer is right. Please see our response above. In the revised manuscript, we have modified this statement, which now reads (Lines 103-105): *“When the Agulhas leakage is lower than 30 Sv, a 10 Sv increase in the Agulhas leakage, can only strengthen the AMOC by a maximum of 1.5 Sv, achieved after about 150 years, in our simulations (Fig. 1c).”*

1. 110 Why was the experiment with an AL of 28 Sv chosen for the comparisons? Is an increase of 20 Sv likely? Or does it best illustrate the mechanisms? But how does a more realistic increase compare?

Reply: In the revised manuscript, we chose the two experiments with Agulhas leakages of 17.2 Sv and 26.5 Sv, respectively, for comparison. The Agulhas leakage in AL17 is close to the observation-based estimate and the Agulhas leakage increase from AL17 to AL26 is close to the multi-model ensemble-mean Agulhas leakage changes projected by CMIP models under the high-emission scenario.

In the revised manuscript, we have revised this statement to read (Lines 119-125): *“To demonstrate this compensating effect, here we compare two experiments, “AL17” and “AL26”, in which the Agulhas leakage is 17.2 Sv and 26.5 Sv, respectively. The Agulhas leakage in AL17 is close to the observation-based estimate and the Agulhas leakage increase from AL17 to AL26 is close to the multi-model ensemble-mean Agulhas leakage changes projected by CMIP models under the high-emission scenario.”*

l. 159 By how much is the salinity difference reduced?

Reply: The amount of reduction in salinity difference depends on the magnitude of Agulhas leakage changes. When the Agulhas leakage is greater than 30 Sv, there is almost no difference in salinity between the Southwest Atlantic and Agulhas Current region.

l. 162 Give a range what you refer to by “longer timescales” e.g., of more than 500 years.

Reply: Thanks for this suggestion. In the revised manuscript, we have modified this statement to read (Lines 181-183): “Taken together, our results suggest an even diminished influence of the Agulhas leakage on the AMOC on multi-centennial to millennial timescales.”

l. 220 Cite Richardson et al., (2007) and Daher et al., (2020), not R  hs et al., (2022).

Reply: Modified as suggested (Line 248).

l. 226 How does the inter-model correlation between AMOC and AL look like in the High-resolution MIP of CMIP6?

Reply: In the High-resolution MIP of CMIP6, there is no inter-model correlation between the AMOC and the Agulhas leakage (Fig. R3-8).

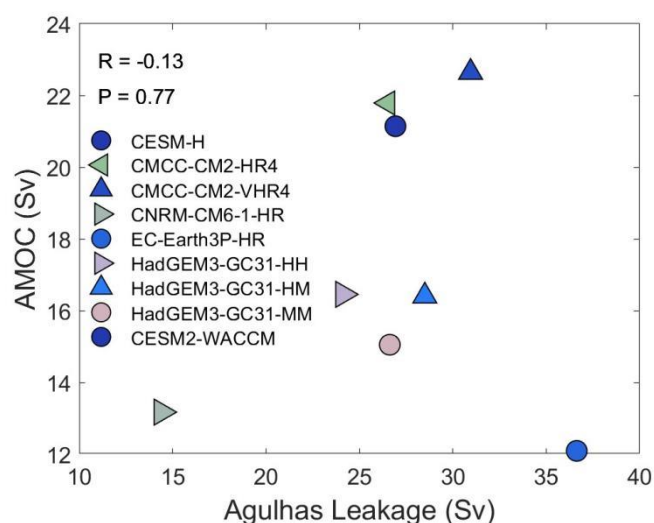


Fig. R3-8 Inter-model relationship between the AMOC and the Agulhas leakage among Highres-MIP climate models.

Fig. 1 Breaking the orange arrow up and adding a few eddies would make the schematic in (a) more realistic as the AL pathway is not comparable to the Agulhas Current and Return Current.

Reply: Modified as suggested (see revised Fig1a).

l. 374 Which observational product is used?

Reply: We used Argo. In the revised manuscript, we have modified this statement,

which now reads (Line 414): “The background color represents climatological mean salinity at 500 m depth from Argo observation.”

1. 390 Define AL28, figure captions should be overall self-explanatory.

Reply: Thanks for this suggestion. In the revised manuscript, we have modified this statement, which now reads (Lines 431-434): “The density anomaly is calculated as the difference between AL26 and AL 17, in which the Agulhas leakage is 26.5 Sv and 17.2 Sv, respectively, evaluated at year 150 after modifying the Agulhas leakage. “

11. 476-481 More details needed: What are the approximations and assumptions? Has this been used in previous studies? How big is the error e.g., difference between thermal wind relationship using left-hand side and right-hand side?

Reply: To isolate the effects of temperature and salinity change on the density change, we decomposed density changes into contributions due to temperature and salinity using the equation of state of sea water. This method has been used in the low frequency (centennial to millennial) variability of the AMOC (Bonan et al., 2022). In the revised manuscript, we added the difference between thermal wind relationship using density and temperature/salinity, which now reads (Lines 527-534):

“To isolate the effects of temperature and salinity changes on the AMOC, we decompose the density changes into temperature-induced and salinity-induced contributions:

$$\begin{aligned} \Delta\rho &= \rho(T_2, S_2, p) - \rho(T_1, S_1, p) \\ &= \rho(T_2, S_1, p) - \rho(T_1, S_1, p) + \rho(T_1, S_2, p) - \rho(T_1, S_1, p) + Res \\ &= \Delta\rho_T + \Delta\rho_S + Res \end{aligned} \quad (2)$$

where $\rho(T, S, p)$ is the state equation of sea water, using the UNESCO 1983 (EOS 80) polynomial, and Res is the residual due to nonlinearity in the equation of state.”

In the revised Fig. 3a, we add the result of Res, which indicate the Res is small enough to be neglected.

1. 482 Why isn't TEOS-10 used? It's the new standard and has replaced EOS 80.

Reply: In POP2, the equation of state is based on EOS80.

11. 527-529 “low-resolution climate models cannot fully resolve the mesoscale processes involved in the Agulhas Leakage and may introduce biases” should be rephrased as these models cannot resolve mesoscale processes at all, instead often parametrizations are used, but biases are certainly introduced.

Reply: Thanks for this suggestion. We revised this statement, which now reads (Lines 579-580): “These low-resolution climate models cannot resolve mesoscale processes, which are parameterized, and thus, may introduce biases.”

11. 529-531 What is the conclusion from the High-resolution CESM? What do models

from the High-resolution MIP suggest?

Reply: The Agulhas leakage in the High-resolution CESM increases by about 10 Sv under the high-emission scenario during the 21st century. However, there is substantial inter-model spread among the high-resolution climate models. From 1950 to 2050, the multi-model ensemble mean of Agulhas leakage increases is about 6.7 Sv (Fig. R3-9) and the standard deviation is 5.6 Sv. In the revised manuscript, we include the result from High-resolution MIP in the Fig. S1.

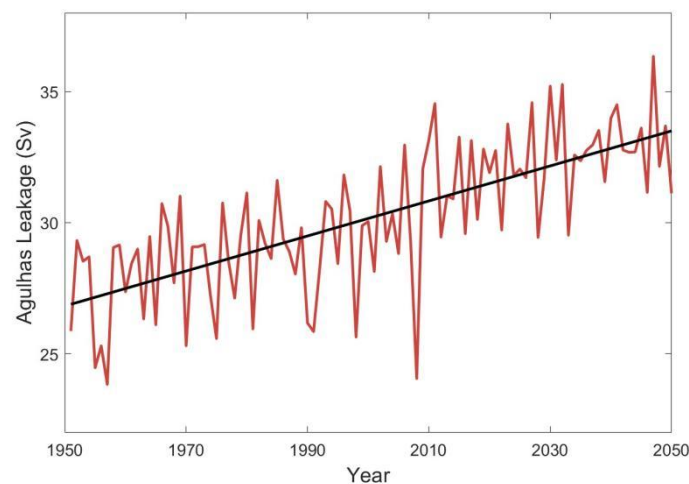


Fig. R3-9 Multi-model ensemble mean of the Agulhas leakage during 1951-2050 in HighRes-MIP (red line) and its linear trend (black line).

Extended data Fig. 8: Add a small map to show regions used for spatial average. Add thin lines for observations to make comparison easier.

Reply: Modified as suggested.

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Reply to the second reviewer

We thank the reviewer for these helpful comments. We appreciate the time and efforts the reviewer invested in reviewing the previous version of the manuscript. We have addressed the reviewer's comments, which are in blue and italics, in the revised manuscript. Our replies are in black following each of the reviewer's comments.

Reviewer #2

1) L606-620: Additional Lagrangian computations have been performed, which is valuable but no "numbers" are given. Please provide the Lagrangian and Eulerian estimates the authors computed.

Reply: Thanks for this suggestion. We provide the Lagrangian- and Eulerian-estimated Agulhas leakage in the supplementary Table S1. In the revised manuscript, we revised our discussion on the estimate of Agulhas leakage, which reads (Lines 526-531): *"Here we also use a Eulerian method to estimate the Agulhas leakage, defined as the volume transport across the section between (11.5°E, 38.5°S) and (18.5°E, 31.5°S) (green line in Fig. S2b) over the upper 1500 meters. The Eulerian estimate largely reproduces the Lagrangian estimate of Agulhas leakage with a standard error of 2.1 Sv (Fig. S12). The correlation between these two estimates is at 0.98."*

2) L610: The main point when performing Lagrangian experiments is to clearly define the particle initialization, allowing for the experiment to be reproducible. In the text, two information are missing: i) the length of particle initialization (i.e. How long for the particles are released every ten days? Typically to avoid bias particles are initialized for the first year of the model run. If this is not the case and an other choice have been made, please explain; ii) the total number of particles initialized and the number of particles kept for the analysis/calculation (i.e. the ones crossing Good Hope in this case).

Reply: Thanks for this suggestion. In the revised manuscript, we modified the descriptions of the Lagrangian particle methods in estimating Agulhas leakage and included more details, which read (Lines 511-526): *"We use 10-day mean 3-dimension velocity during model years 751-755 in each experiment to estimate the Lagrangian pathway of Agulhas leakage water, which is integrated using the fourth-order Runge-Kutta method with a time step of 1 hour. The velocity fields are linearly interpolated in space and time. We release particles every ten days at the section of the Agulhas Current Time-series Experiment (blue line in Fig. S12a) with a longitudinal spacing of 0.1° over the upper 1500 meters. The particles are released throughout model year 751 in each experiment and tracked for four years. In total, we released 20,592 particles in each experiment and each of the particles represents a*

fraction of the Agulhas Current volume transport (Fig. S12b). The location of each particle is saved every ten days. Those particles that cross the Good Hope section (red line in Fig. S12a) at least once are counted as Agulhas leakage. As an example, in AL17, a total of 3680 particles crossed the Good Hope section, representing a volume transport by Agulhas leakage of 16.9 Sv. We also test the sensitivity of our results to the number of particles. When we release the particles every 5 days, the estimated strength of Agulhas leakage slightly changes, but within 1.5 Sv.”

3) L613: "... are counted ..."

Reply: Modified as suggested (Line 522).

4) L616: "Here we are also using ..."

Reply: In the revised manuscript, we modify this statement, which reads (Line 526):
“Here we also use ...”

Reply to the third reviewer

We thank the reviewer for these helpful comments. We appreciate the time and efforts the reviewer invested in reviewing the previous version of the manuscript. We have addressed the reviewer's comments, which are in blue and italics, in the revised manuscript. Our replies are in black following each of the reviewer's comments.

Reviewer #3

Major comments:

1) I appreciate that the authors conducted Lagrangian experiments to estimate Agulhas leakage but necessary information on how they were executed is missing which prevents reproducibility and hinders interpretation of introduced error. Questions you might want to answer are: Which temporal resolution does the model output have which is used to advect particles? Which model years were used to advect the particles? How many particles were released? Is the result sensitive to the number of particles? For how long are particles released? For how long were the particles advected? At which resolution is the particle position saved? How is transport assigned to a particle? Over which period is the particle transport across the Good Hope line averaged to estimate Agulhas leakage?

Reply: Thanks for these suggestions. In the revised manuscript, we included more detail for the Lagrangian particle method, which reads (Lines 511-526): *"We use 10-day mean 3-dimension velocity during model years 751-755 in each experiment to estimate the Lagrangian pathway of Agulhas leakage water, which is integrated using the fourth-order Runge-Kutta method with a time step of 1 hour. The velocity fields are linearly interpolated in space and time. We release particles every ten days at the section of the Agulhas Current Time-series Experiment (blue line in Fig. S12a) with a longitudinal spacing of 0.1° over the upper 1500 meters. The particles are released throughout model year 751 in each experiment and tracked for four years. In total, we released 20,592 particles in each experiment and each of the particles represents a fraction of the Agulhas Current volume transport (Fig. S12b). The location of each particle is saved every ten days. Those particles that cross the Good Hope section (red line in Fig. S12a) at least once are counted as Agulhas leakage. As an example, in AL17, a total of 3680 particles crossed the Good Hope section, representing a volume transport by Agulhas leakage of 16.9 Sv. We also test the sensitivity of our results to the number of particles. When we release the particles every 5 days, the estimated strength of Agulhas leakage slightly changes, but within 1.5 Sv."*

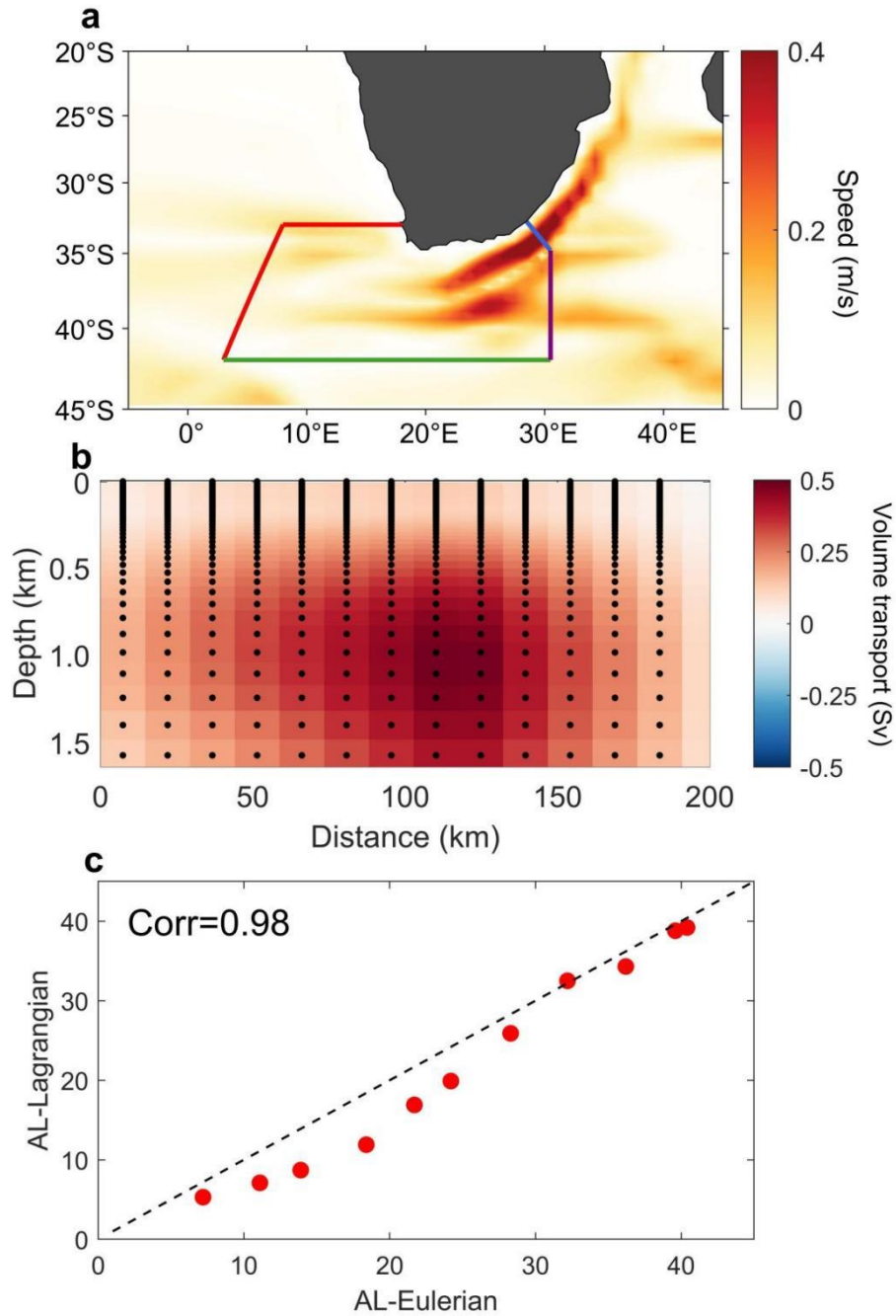


Fig. R3-1 **a**. Sections for estimating Agulhas leakage. The background shading denotes climatological speed averaged over the upper 500 meters. The blue line is the ACT section, where we release the particle. The red lines denote the good hope line used to estimate the Agulhas leakage. The purple line and green line are used to calculate the particles leaving this domain from eastern and southern boundaries, respectively. **b**. The background shading denotes the volume transport of Agulhas Current in each grid cell. The black dots denote the virtual particles being tracked. **c**. Comparison of the Agulhas leakage estimated by the Lagrangian method (y-axis) with estimates using the Eulerian method (x-axis). The dashed black line denotes $y=x$.

2) I might have missed that in my earlier review, sorry about that. But describing the model set up in only 8 lines is insufficient. And unfortunately, the Text S1 does not add much more information other than the model year where the sensitivity experiments were branched off. Please provide details on e.g. initialization, spin-up, length of the simulations, and other specifications like salinity restoring if applied.

Reply: Thanks for these suggestions. In the revised manuscript, we included the following information, which reads (Lines 472-483): “The model is initialized from a state of rest with observed January-mean climatological Polar Science Center Hydrographic Climatology (PHC2) potential temperature and salinity. The model is forced with normal year forcing from the Coordinated Ocean-Ice Reference Experiments version2 (COREv2) dataset, including air temperature, specific humidity, longwave radiation, precipitation, runoff, shortwave radiation, and wind speed. In the simulation, surface salinity is weakly restored to observed climatological sea surface salinity with a restoring timescale of 4 years (Danabasoglu et al. 2012). Unresolved mesoscale eddies are parameterized using the Gent-McWilliams scheme with a thickness diffusivity that is a function of local density stratification. Vertical mixing is handled by the non-local K-Profile Parameterization (KPP) scheme. We spin up the model for 600 years (“ref”). Another ten sensitivity experiments were branched from year 601 and integrated for another 1000 years.”

3) l. 146-147: I cannot see where the 2.5 Sv comes from by looking at the yellow line in Fig. 2c? I might be misinterpreting the figure. If the strength of Agulhas leakage is on the x-axis then I struggle to see a linear relationship, e.g. the AMOC increase between AL5Sv and AL15 Sv is appr. 3 Sv, while the AMOC increases by appr. 2 Sv between AL10Sv and AL20Sv. If the Agulhas leakage increase is on the x-axis (please adjust label if so), then an increase of 10 Sv is related to an AMOC anomaly of 2 Sv.

Reply: The 2.5 Sv increase of AMOC is calculated based on previous Eulerian-estimated Agulhas leakage. In response to this comment, we recalculate the gradient of the yellow line. Averaged within the range of 5 Sv to 30 Sv, the gradient of the yellow line is 0.22. In the revised manuscript, we revised this statement, which now reads (Lines 145-147): “When the Agulhas leakage is less than 30 Sv, a 10 Sv increase in the Agulhas leakage would have strengthened the AMOC by 2.2 Sv, in absence of the temperature compensation.”

4) l. 95,97, 477, 478, 479, ...: Why is the word “about” needed here? The numbers are probably an average calculated over a certain period and if this is made clear in the methods section (maybe together with a std or percentile range), “about” can be deleted so that readers don’t wonder what the exact value might be or if there were an error which would require the word “about”.

Reply: Thanks for this suggestion. In the revised manuscript, we have deleted “about” in these sentences and add the exact value.

5) l. 505-506: This needs rephrasing. Although the correlation is really high the absolute numbers are quite different. This will probably not impact your results but

should be stated clearly.

Reply: In the revised manuscript, we modified this statement, which now reads (Lines 526-531): “Here we also use a Eulerian method to estimate the Agulhas leakage, defined as the volume transport across the section between (11.5°E, 38.5°S) and (18.5°E, 31.5°S) (green line in Fig. S2b) over the upper 1500 meters. The Eulerian estimate largely reproduces the Lagrangian estimate of Agulhas leakage with a standard error of 2.1 Sv (Fig. S12). The correlation between these two estimates is at 0.98.”

Minor comments:

l. 73-75: It should read: “Thus, to date, it remains elusive how ~~would~~ an increase in the Agulhas leakage may affect the AMOC 74 in a warming climate.”

Reply: Modified as suggested (Lines 73-75).

l. 83: found → find

Reply: Modified as suggested (Line 83).

l. 90-91: improve readability: “To isolate the impact of an increased Agulhas leakage on the AMOC, ...”

Reply: Modified as suggested (Lines 89-90).

l. 96: I would suggest rephrasing: “This is weaker than the poorly constrained observational-based estimates of 15 Sv (Richardson et al., 2007) and 21.3±4.7 Sv (Daher et al., 2020).”

Reply: Modified as suggested (Lines 94-96).

l. 119: Shouldn't it read: “The limited impacts of Agulhas leakage increase on the AMOC are partly due to the compensating effects of warming and salinification in the Atlantic.”?

Reply: Thanks for this suggestion. Modified as suggested (Lines 117-118).

l. 144-146: reference Fig. 2

Reply: Modified as suggested (Lines 145).

l. 145-146: in high latitudes → at high latitudes

Reply: Modified as suggested (Lines 144-145).

l. 154-156: Please rewrite this sentence “subsurface warming ventilates at the sea surface” does not make sense.

Reply: In the manuscript, we revised this statement, which read (Lines 154-155): “subsurface warming spreads into the sea surface”

l. 166: delete “at”

Reply: Modified as suggested (Line 166).

l. 219: ... the AMOC may have become more unstable ...

Reply: Modified as suggested (Lines 219-220).

l. 251: suggest → suggests

Reply: Modified as suggested (Line 251).

l. 413: schematics → schematic

Reply: Modified as suggested (Line 414).

l.414: observation → observations

Reply: Modified as suggested (Line 415).

l.475-476 and l. 33 in Suppl.: North Atlantic deep water → North Atlantic Deep Water

Reply: Modified as suggested (Lines 488-489).

l. 479: drake passage → Drake Passage

Reply: Modified as suggested (Line 492).

l. 482: “in which we increase the Agulhas leakage up to 40 Sv.” → “in which we increase the Agulhas leakage to up to 40 Sv.” I would also suggest expanding this sentence by including how you increase Agulhas leakage (i.e. by modifying the strength of the surface zonal wind stress (see Table S1))

Reply: Thanks for this suggestion. In the revised manuscript, we revised this statement, which now reads (Lines 496-498): “Branched from “Ref”, we modify local surface zonal wind stress and conduct a total of 10 perturbation experiments (see Text S1 and Table S1 in supplementary), in which we increase the Agulhas leakage to up to 40 Sv.”

l. 499 count → counted (or considered)

Reply: Modified as suggested (Line 522).

Supplementary Information

At the moment Text S1 in the Supplementary material does not provide any additional information which is not yet in the main manuscript or should be in there. Table S1: A better caption is needed, explaining for example what the number after the +- sign in AL_Eulerian is and why it's not given in AL_Lagrangian. “dyne” is not a SI unit, please convert.

Reply: In the revised Supplementary, we revised the caption of Text S1 to “Configuration of the perturbation experiments”. The number after +- is the standard deviation of AL_Eulerian. In our simulations, because the model is forced by climatological external forcing, the inter-annual variability of the circulation is very small. For example, the standard deviation of inter-annual variability of the Agulhas

Current, Eulerian-estimated Agulhas leakage and AMOC is less than 0.5 Sv. Therefore, we deleted the standard deviation and included a statement in the revised manuscript, which reads (Lines 493-495): *“Because the model is forced with repeating normal year external forcing, the inter-annual variability of the ocean circulation is weak. The inter-annual variability of the Agulhas Current and the AMOC is less than 0.5 Sv.”* In the revised Table S1, we convert the unit of wind stress from dyne/cm² to Pa.

Text S2: often a “the” is missing (e.g. “the AMOC response” (l. 52), “the Atlantic Ocean” (l. 53), etc)

Reply: Modified as suggested.

l.62-64: reference a figure

Reply: Modified as suggested (Line 61).

l. 70: reference figure S14

Reply: Modified as suggested (Line 67).

l. 71: left → north-westward

Reply: Modified as suggested (Line 68).