

Response to the editor and reviewer 2

We thank the editor and the reviewer for their careful assessment and constructive comments. Below we respond point by point and describe the changes made for the manuscript.

Summary:

The manuscript imposes a freshwater-forced AMOC collapse against the backdrop of an emission-driven 2°C stabilization pathway in GFDL ESM2M, using adaptive emissions to target a 2°C global warming level under different AMOC scenarios. Relative to the counterfactual, AMOC collapse strongly cools global mean temperature by 0.8°C, increases thermosteric sea level rise, lowers atmospheric CO₂ through Amazonian land uptake, and raises the remaining carbon budget for staying below 2°C. Similar to previous literature, the authors find that all climatic responses to AMOC weakening are state-dependent and are, except for atmospheric carbon concentrations, moderated by the warmer background climate.

Overall assessment:

The simulation setup is carefully designed and executed and the adaptive emissions method is a real advance over concentration-driven hosing experiments; the decomposition of the global temperature signal and the heat and carbon budget decompositions are thorough and very insightful. My reservations concern framing and interpretation rather than the modelling itself. Two results that carry weight in the abstract and discussion, the seasonal cooling pattern in selected cities, and the TCRE slope change under AMOC collapse, are stated more confidently than corroborated by current figures and statistics. Additionally, the gap the paper sets out to fill is somewhat ambiguous. I recommend minor revisions to this already very nice paper.

I really appreciate the structured and clear writing of the paper, which made it easy to follow and pleasant to read.

We thank the reviewer for the positive assessment of our manuscript and the helpful suggestions.

Major points:

1. Winter amplification of AMOC-induced cooling

- "reveals a pronounced winter amplification of the AMOC-induced cooling" (L273)
- "Cooling is substantially weaker in summer" (L280)

In Figure 3f only Reykjavik shows a visually clear seasonal cycle in the anomaly. For London, Bern, Bergen and Beijing the seasonal contrast is modest relative to their mean cooling and comparable to the ensemble spread. The stronger statements above do not seem supported by Fig. 3f and I would like to see statistical tests for difference between winter and summer months if this general statement is to be maintained. Given the

pronounced winter signal in Reykjavik and, in Fig. B4, the larger variability for Reykjavik, I am also curious about the influence of sea ice. Despite sea ice albedo effects being small in comparison to cloud effects, I still wonder whether some of the regional and seasonal temperature effects can be (partly) explained by sea ice. A map of March sea ice mean state in the 2171-2200 period might also help to visually see which land areas might be directly affected, especially in light of the results by van Westen and Baatsen (2025).

We thank the reviewer for raising this important point. To test if the seasonal contrasts are statistically significant we have conducted a pair-sampled t-test for the differences in the seasonal means. According to this statistical test, winter values are indeed significantly more negative than summer values in all selected cities. We have changed the text to: *"The monthly-mean temperature changes over selected cities (e.g. Reykjavik, London, Bern, Beijing, Bergen) in the Northern Hemisphere show cooling throughout the year relative to the 2°C-ref, with regional differences in its magnitude and seasonality (Fig. 3e,f). In the 2°C-ref simulation, monthly temperatures averaged over 2171–2200 in all selected cities are above preindustrial levels throughout the year (Fig. 3d). In contrast, under AMOC collapse conditions (2°C-hos-Eref), the selected cities experience monthly mean temperatures at (Beijing) or below (Reykjavik, London, Bern, Bergen) preindustrial levels for much of the year (Fig. 3e). Temperature generally remains above -2°C relative to 1861-1900 for most cities, except Reykjavik and London in winter. The largest cooling relative to 2°C-ref is simulated in late winter and early spring (Fig. 3f). Reykjavik exhibits the most pronounced seasonal contrast in ensemble-mean cooling, reaching -7.4°C in February (ensemble range: -10.2 to -3.8°C). In the other cities, the seasonal contrast is smaller relative to the mean cooling and ensemble spread. Nevertheless, cooling is significantly stronger in January–March than in July–September in all five cities (paired-samples t-tests, maximum p-value across cities all 0.0054). This seasonal contrast is consistent with previous studies (Bellomo et al. 2024, Jackson et al. 2015)."*

We also thank the reviewer for highlighting the potential role of sea ice. We have added an appendix figure showing the mean March sea ice distribution over 2171–2200 for the 2°C-ref and 2°C-hos-Eref simulations (Fig. B4). We have added the following sentence:

"Although the sea ice albedo contribution to the global temperature response to AMOC collapse is relatively small (see section 3.3), sea ice expansion may substantially affect regional temperatures, particularly in winter through reduced ocean-to-atmosphere heat release and changes in atmospheric circulation (van Westen and Baatsen 2025). In 2°C-hos-Eref, Arctic sea ice extends farther south than in 2°C-ref, surrounding Iceland (Fig. B4). This expansion may contribute to the pronounced winter cooling in Reykjavik (Fig. 3e). Changes in the proximity of the sea ice edge may also contribute to its greater temperature variability (Fig. B5)."

2. TCRE slope difference with a collapsed AMOC

"the slope in 2°C-ref is 1.00 °C (0.65 to 1.40) per 1000 GtC, which is slightly smaller than the corresponding slope in 2°C-hos-Eref of 1.25 (1.03 to 1.52) °C per 1000 GtC" (L389-390)

Looking at Fig. 10b does not visually suggest a different slope. The two confidence intervals also overlap substantially, yet the discussion builds on the difference: "AMOC-induced changes in ocean circulation modify to some extent the partitioning of heat and carbon

uptake by the global ocean, thereby altering the transient warming response to cumulative CO₂ emissions" (L466-468). Can the authors test whether the slopes are statistically distinguishable? Absent such a test, L466-468 should be marked as a conjecture.

An idea might also be to pool both experiments in a single regression that controls for AMOC strength, so that it would yield an overall TCRE conditional on the AMOC state, but this is just a suggestion.

We thank the reviewer for this important comment. We have conducted a paired-sample t-test and indeed the slopes are not statistically significant. We changed the sentence to: *"For cumulative emissions between year 2051 and 2150, the slope in 2°C-ref is 1.00 (0.65 to 1.40) °C per 1000 GtC, which is slightly smaller than the corresponding slope in 2°C-hos-Eref of 1.13 (1.06 to 1.19) °C per 1000 GtC. however the difference is not statistically significant."*

Generally, the discussion statement "this near-linear relationship breaks down when the AMOC collapses" (L462) raises the question of whether this is the first known mechanism to break TCRE linearity or whether there are analogous processes that have been previously studied. The permafrost literature comes to mind, for instance Steinert and Sanderson (2025, ESD).

We have added the following paragraph to clarify this: *"Numerous studies have demonstrated that TCRE provides a robust predictor of CO₂-driven warming under a wide range of forcing scenarios (Matthews et al. 2009, Allen et al. 2009, Tokarska et al. 2016, Matthews et al. 2020). Our findings, however, identify a temporary departure from the near-linear relationship between global warming and cumulative CO₂ emissions during AMOC collapse, as AMOC-induced cooling offsets the warming expected from continued emissions. Other processes can also modify this relationship. For example, permafrost carbon release can increase warming per unit anthropogenic emissions and thus increase TCRE, without necessarily disrupting near-linearity (Steinert and Sanderson, 2025; Georgievski et al. 2025). Temperature can also continue to evolve after net CO₂ emissions cease, despite unchanged cumulative emissions, because the warming effect of declining ocean heat uptake and the cooling effect of continued land and ocean carbon uptake do not necessarily compensate exactly (Frölicher et al. 2014, MacDougall et al. 2020). Furthermore, simulations of net-negative emissions that follow periods of positive emissions reveal hysteresis, with different warming and cooling responses per unit carbon emitted or removed (Zickfeld et al., 2016). Our results therefore illustrate an additional mechanism through which changes in the climate system can temporarily alter the warming–emissions relationship. Once emissions continue under the collapsed-AMOC scenario, however, the relationship between global warming and cumulative emissions returns to an approximately linear trajectory, similar to that in the simulation without AMOC collapse (2°C-ref)."*

3. What exactly is the gap in the literature?

"A systematic assessment of the climatic and carbon cycle consequences of a collapsed AMOC under emission-driven warming is therefore still lacking." (L79-80)

I understand that the contribution of this paper is manifold, in that it has an interactive carbon cycle in an emissions-driven setup, looks at global AMOC impacts under ongoing non-

idealised warming scenarios, and makes the genuinely novel link to TCRE and carbon budgets. Yet I found it a bit hard to parse which aspects of the contribution are considered the key innovations and would encourage a slight reformulation of this paragraph. After reading it several times, I appreciate that all the aspects of the contribution and the comparisons to other relevant papers are there, but I found it hard to understand during the first read.

Additionally, comparisons with Romanou et al. (2023) and Orbe et al. (2023) as well as with Nielsen et al. (2019) and Nian et al. (2026) could help situate this paper in the literature and clarify the gap that it fills.

We thank the reviewer for this helpful suggestion. We have revised the paragraph to distinguish more clearly between previous work on AMOC collapse under future warming, studies of its carbon cycle consequences, and the specific contribution of our study. We have added Romanou et al. (2023), Orbe et al. (2023), Nielsen et al. (2019), and Nian et al. (2026). Rather than claiming a broad absence of assessments under emission-driven warming, the revised text identifies our central contribution as connecting the climatic and carbon cycle consequences of AMOC collapse along a transient 2°C stabilization pathway to TCRE and the remaining emission budget. The paragraph reads:

“Despite growing interest, substantial uncertainties remain regarding the consequences of an AMOC collapse in a warming world, particularly its implications for the transient climate response to cumulative CO₂ emissions (TCRE) and the remaining emission budget. Many modeling studies investigated AMOC shutdowns under preindustrial or glacial boundary conditions (Vellinga et al. 2002, Menviel et al. 2008, Bozbiyik et al. 2011, Jackson et al. 2015, Orihuela et al. 2022, van Western et al. 2024, Ma et al. 2024), limiting their transferability to a climate system already altered by anthropogenic forcing. The impacts of AMOC weakening are strongly state-dependent and interact nonlinearly with background warming (Vellinga et al. 2008, Kageyama et al. 2024, Bellomo et al. 2024). Several studies have examined AMOC collapse superimposed on ongoing global warming, including its emergence under a mid-range future forcing scenario (Romanou et al. 2023) and its consequences for atmospheric circulation (Orbe et al. 2023). Other studies have focused on regional responses, such as European temperature extremes and European hydroclimate (Van Westen et al. 2025), used highly idealized future warming scenarios (Swingedouw et al. 2007, Bellomo et al. 2024), or employed coarse-resolution climate models (Drijfhout et al. 2015). Moreover, many previous studies did not include fully interactive carbon–climate interactions, preventing a comprehensive assessment of how an AMOC collapse would affect atmospheric CO₂, the TCRE, and the remaining emission budget (Drijfhout et al. 2015, Liu et al. 2020, van Westen et al. 2024, van Westen et al. 2026). Studies with interactive carbon cycles have demonstrated changes in oceanic and terrestrial carbon storage following AMOC collapse, including in a paleoclimate context (e.g. Nielsen et al., 2019) and in millennium-long simulations at different CO₂ levels with intermediate complexity models (Nian et al., 2026). Boot et al. 2024 examined carbon cycle responses to strong AMOC weakening under low- and high-emission scenarios using a comprehensive Earth system model, finding modest increases in atmospheric CO₂ driven by changes in oceanic and terrestrial carbon fluxes. These studies establish that AMOC changes can affect the carbon cycle, but leave unresolved how a collapse during a transient, emission-driven warming pathway jointly affects climate, carbon storage, TCRE, and the remaining emission budget for a specified

warming target. Here, we address this gap by investigating the coupled climate–carbon cycle response to an AMOC collapse under transient, emission-driven global warming using a comprehensive Earth system model with an interactive carbon cycle. We quantify how an AMOC collapse alters global temperature, oceanic and terrestrial carbon storage, and atmospheric CO₂, and assess the resulting implications for the TCRE and the remaining carbon budget for a specified global warming target.”

Minor points:

L16, L19-20: The abstract and results disagree on two headline numbers. Land carbon uptake is 44 GtC (33-53) here but +49.6 GtC (44.5-59.9) at L353. The remaining budget increase is 63% (54-72) here but 60% (49-73) at L386.

We thank the reviewer for this pointer. We have recalculated the numbers and they are now consistent with each other.

L21-22: "reduced climate sensitivity to AMOC collapse in a warmer world" brings up connotations with ECS or TCR in an abstract. L403-404 already has the clearer wording, "a reduced sensitivity of the climate system to AMOC collapse under warmer background conditions".

Changed to 'reduced sensitivity of the climate system to an AMOC collapse'.

L27: Several reference entries seem to have issues. "University of Potsdam and Rahmstorf, S." and "Griffies, S. M., NOAA, and Laboratory, G. F. D." parse institutions as authors; the AERA protocol appears as "Thomas, F., Jens, T., Fortunat, J., and Yona, S.", with given and family names swapped; I suggest checking all entries again.

We have corrected the listed entries, and also checked and corrected the remaining bibliography.

L93, L104, L105: "Dunne et al. (2013)" is cited as the ESM2M description and carbon-cycle evaluation, but the reference list gives "Reductions in Labour Capacity from Heat Stress under Climate Warming". The intended paper is presumably Dunne et al. (2013), J. Climate 26, 2247-2267.

Thank you for catching this error. Reference changed to the Dunne et al. 2013 ESM2M description paper.

L157: "string model biases" should probably be "strong" or "existing".

Corrected to "existing model biases".

L214: The choice of reported variables is not motivated. Sea level is analysed in detail while precipitation enters only through NPP. Relatedly, the gap statement promises "climatic and carbon cycle consequences" (L79-80), and it is not obvious how that maps onto the quantities actually analysed, which are the drivers of global temperature and thermosteric sea level. A sentence in the introduction stating which quantities are the focus and why would resolve this.

We have clarified the scope of our study at the end of the introduction: “*We focus on surface temperature, ocean heat storage, thermosteric sea level, and terrestrial and oceanic carbon uptake and storage*”.

L256: "although with a delay because emission adjustments are based on the warming and cumulative emissions since preindustrial". Could this delay be avoided, and would the budget results change if AERA tracked the cooling more directly and returned to the reference pathway earlier? Since the 60% increase is a headline number, it matters how much of it is a property of the AMOC response and how much of the AERA implementation.

We thank the reviewer for raising this point. The delayed adjustment reflects the AERA implementation used here, which estimates TCRE from warming and cumulative emissions since preindustrial. An alternative implementation designed to respond more rapidly to “abrupt” temperature changes could potentially reduce this delay. However, estimating the warming–emissions relationship over a shorter interval would require care because AMOC-induced cooling can temporarily produce a small or negative apparent slope. Faster adjustment would likely affect the timing of emissions and the approach to the temperature target. Whether it would substantially change cumulative emissions by 2200 cannot be established without additional sensitivity experiments. Nevertheless, the approximately linear warming–emissions relationship that is simulated under a sustained weak-AMOC state suggests that different adjustment schemes would yield similar budgets.”

L263: Almost all results are reported for 2171–2200. The AMOC is already close to its collapsed state around 2100 (Fig. 2a), so the more decision-relevant outcomes for this century are available and should at least be briefly mentioned. Impacts in this century might be more useful for risk assessments, which are hinted at in the closing statement.

We agree that 21st-century impacts are relevant for risk assessments. Our main analysis focuses on 2171–2200 to characterize the response after prolonged AMOC weakening, because temperature, ocean heat storage, and carbon reservoirs continue to adjust after the AMOC approaches its weak state. This period nevertheless represents a transient rather than an equilibrium response. To complement this analysis, we have added an additional paragraph in the discussion section with a summary of the AMOC, global temperature, atmospheric CO₂, and thermosteric sea-level responses for 2071–2100, including ensemble ranges. This highlights the impacts emerging within this century while retaining a consistent period for the detailed analysis.

We have added the following paragraph towards the end of the discussion section:
“*Although our main analysis focuses on 2171–2200, substantial impacts emerge already within the 21st century when idealized freshwater hosing is imposed in 2026. By 2071–2100, AMOC strength at 26.5°N declines to 7.2 Sv (ensemble range: [6.8–7.7]) in 2°C-hos-Eref. Relative to 2°C-ref, global mean surface air temperature is 0.65°C lower ([0.57–0.75]), atmospheric CO₂ is 7.5 ppm lower ([5.7–9.3]), and global thermosteric sea level is 1.23 cm higher ([1.13–1.36]). These changes demonstrate that the consequences of the imposed AMOC weakening are relevant within this century, rather than arising only after prolonged adjustment. Nevertheless, temperature, ocean heat storage, and carbon reservoirs continue to evolve after the AMOC approaches its weak state, motivating our complementary*

assessment of the later response. However, the timing and magnitude of these impacts are conditional on the idealized hosing imposed from 2026."

L265-266: the strongest anomalies "extend zonally into Western and Central Europe", but the next sentence introduces a separate over-land statement, which reads as if the first were not over land.

We agree. We changed 'Over land' with 'Averaged over their respective land areas'

L276: "Reykjavik"

Corrected.

L279: Vienna is not among the cities shown in Figure 3 (Reykjavik, London, Bern, Beijing, Bergen), so the winter cooling attributed to it cannot be checked against the figure.

Thank you for spotting this typo. "Vienna" has been corrected to "Bern".

Fig. 10b: I really like this figure. I think it is highly important and very suggestive.

Thank you very much for highlighting the value of this figure.

L409: The larger CO₂ decrease under 2°C warming is attributed to different sensitivities of ocean and terrestrial carbon uptake, but I am wondering whether this could also reflect different equilibration timescales, since 200 years is still very transient for the ocean carbon cycle, in contrast to atmospheric responses.

We agree that different adjustment timescales could contribute to the larger atmospheric CO₂ decrease under warmer background conditions. We have revised the text also taking into account the comment by reviewer 3: *"In both climate states, the atmospheric CO₂ response reflects enhanced terrestrial carbon uptake, partly offset by oceanic carbon outgassing. However, the relative magnitudes of these opposing fluxes differ between the two background states, resulting in a weaker net atmospheric CO₂ decrease under preindustrial conditions. Specifically, terrestrial carbon uptake is weaker under preindustrial conditions (21.2 [18.8–24.0] Pg C), but is also offset by a smaller oceanic carbon release (-1.7 [-3.3 to -0.4] Pg C). The stronger atmospheric CO₂ decrease under warmer conditions therefore reflects a different balance between terrestrial carbon uptake and oceanic carbon release. Our experiments, however, do not allow us to determine whether these differences arise from state-dependent carbon-cycle sensitivities, differences in adjustment timescales, or a combination of both."*

L418, L447: Parsons et al. (2014, Geophys. Res. Lett. 41, 146-151) ran hosing experiments in GFDL ESM2M with LM3V and found Amazonian carbon storage rising by about 47% through the same mechanism described here. This is the same model and the same result and should be cited and compared. They also find a large offsetting loss in Central America, so it would be useful to report the tropical American decomposition regionally and to say whether that signal appears here. Agreement within one model does not establish robustness, and the manuscript should be explicit that the +44 GtC may partly reflect the ESM2M/LM3V response.

We thank the reviewer for pointing us to Parsons et al. (2014). We have added this study to the Discussion and now explicitly compare both the Amazonian carbon gain and the offsetting Central American carbon loss with our results. We also clarify that agreement between studies using the same model does not establish robustness across models. The revised paragraph reads:

“Our results indicate that an AMOC collapse substantially alters the hydroclimate and carbon cycle of tropical land regions, with enhanced Amazonian carbon uptake dominating the global land-carbon response. The AMOC collapse induces a southward shift of the ITCZ, producing cooler and wetter conditions over the southern Amazon region that favor enhanced carbon storage. Parsons et al. (2014) found a similar response using the same model: Amazonian terrestrial carbon storage increased associated with changes in precipitation seasonality, a cooler and less severe dry season, and enhanced NPP. Our simulations also show a similar Central American response, where drying and terrestrial carbon losses partially offset the Amazonian carbon gain. The qualitative agreement across both regions suggests that this pattern occurs under both, their preindustrial scenario (Parsons et al. 2014) and our transient warming scenario. However, because both studies use the same model GFDL ESM2M, this agreement does not constitute independent evidence of robustness across models. The magnitude and regional distribution of the simulated land-carbon response may therefore partly reflect this model’s sensitivity to AMOC-induced hydroclimatic changes.

Our findings also agree with previous modelling studies showing that an AMOC tipping transition tends to increase rainfall in the southern Amazon and northeastern Brazil, potentially stabilizing parts of the rainforest (Ben-Yami et al., 2024; Ciemer et al., 2021; Nian et al., 2023; Wunderling et al., 2024). This provides broader support for the simulated hydroclimatic response, although regional differences remain: some studies show drying over the northern Amazon following an AMOC collapse, a feature absent from our simulations. Agreement on increased rainfall does not, however, establish agreement on the magnitude of terrestrial carbon uptake. Paleo-evidence provides a complementary perspective. Reconstructions of Heinrich Stadial 1 associate strongly reduced AMOC with a southward ITCZ shift and wetter conditions over southern tropical South America (Mulitza et al., 2017; Campos et al., 2019). Evidence of increased tree cover in southern Amazonia is also consistent with more favorable conditions for vegetation growth (Dupont et al., 2010). These findings support the proposed hydroclimatic mechanism, although differences in background climate and land use limit direct comparison with our future simulations. In particular, our simulations follow land-use changes from RCP2.6, in which the agricultural area does not further expand. Alternative land-use trajectories could alter forest resilience and carbon storage.”

L435: The contrast is drawn with Boot et al. (2024), but Nian et al. (2026) find CO₂ increasing by 47-83 ppm with about +0.2°C of additional warming, the opposite sign and an order of magnitude larger. That paper, along with Nielsen et al. (2019) and Parsons et al. (2014), is worth engaging with directly, especially on the South American land carbon response.

We thank the reviewer for highlighting these studies. We have expanded the Discussion to compare our findings with Nian et al. (2026) and Nielsen et al. (2019), alongside the regional comparison with Parsons et al. (2014) (see response above). The revised paragraph reads:

“Our simulations indicate that atmospheric CO₂ decreases under an AMOC collapse relative to the non-collapse experiment, primarily due to enhanced terrestrial carbon uptake. This outcome contrasts with Boot et al. (2024) using the CESM2 Earth system model, who find a small increase in atmospheric CO₂ following an AMOC shutdown, driven mainly by ocean outgassing. A substantially larger increase of 47–83 ppm, accompanied by approximately 0.2°C net warming at higher background CO₂ levels, is reported by Nian et al. (2026). Using the Earth system model of intermediate complexity CLIMBER-X, they investigate the equilibrium response over several millennia, with enhanced Southern Ocean convection releasing deep-ocean carbon. Their long-term response is therefore not directly comparable to our 175-year hosing experiments, and our transient CO₂ decrease does not preclude a subsequent increase on longer timescales. Nielsen et al. (2019) likewise demonstrate distinct response timescales, with an initial terrestrial carbon release followed by slower ocean outgassing. These contrasting modeling responses are consistent with the synthesis of Gottschalk et al. (2019), which highlights substantial uncertainty in the sign and magnitude of the carbon-cycle response to AMOC changes and its dependence on background climate and experimental design. Together, these studies emphasize the importance of regional hydroclimatic changes, ocean ventilation, and response timescale, alongside model-specific representations of carbon–climate feedbacks (Zickfeld et al. 2008). Coordinated experiments with harmonized forcing and integration lengths are needed to disentangle these influences and assess the robustness of the carbon-cycle response to AMOC collapse (Swingedouw et al. 2026).”

L558: The author contributions credit the temperature decomposition to "DW", which matches none of the author initials. Presumably it should be DS?

Thank you. Changed to DS.

References:

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van Westen, R. M., and Baatsen, M. L. J. (2025). European temperature extremes under different AMOC scenarios in the Community Earth System Model. *Geophysical Research Letters*, 52, e2025GL114611. <https://doi.org/10.1029/2025GL114611>