Comment on egusphere-2022-869

Anonymous Referee #2

Referee comment on "On the ocean's response to enhanced Greenland runoff in model experiments: relevance of mesoscale dynamics and atmospheric coupling" by Torge Martin and Arne Biastoch, EGUsphere, https://doi.org/10.5194/egusphere-2022-869-RC2, 2022

This paper explores how the North Atlantic Ocean responds to enhanced Greenland melt, using a suite of ocean modelling experiments. The authors carefully explore this problem by using twin experiments, with and without Greenland melt, while also examining the role of resolution (through the inclusion of high resolution nests) and forcing (by considering coupled as well as forced ocean model experiments). The authors also run their experiments for a length (100 years) sufficient to allow signals and different behaviors between experiments to develop. Key results include a compensating temperature feedback in the coupled simulations, which also have greater stability. Additionally, mesoscale dynamics, represented in the nests, play a key role, including penetration of freshwater in the sub-tropics.

This is an important topic, and the study nicely examines many aspects. The paper is generally well written and clear, with high quality figures. Thus, it definitely deserves publication, with EGU Sphere being an appropriate journal. That said, there are some ways the manuscript can be improved. There are some minor wording issues (such as unneeded adjectives). The manuscript also feels long, and given that it covers so much space, there are times that it feels like the main big picture goals get lost in the many details. So it might be good to try to tighten up the manuscript and make sure the focus is always on the main ideas and results. There are also a few technical items that could use further discussion.

We appreciate the overall positive and constructive comments by the reviewer. We will provide a detailed response together with the revised manuscript and will here just briefly touch upon the main points of the criticism. In our revision we will also try and tighten the manuscript as suggested here and also by Reviewer #1 and clarify the technical issues.

Salinity Restoring: This is first mentioned at line 102-103 when the authors mention they use a weak restoring. It would be good to explain why this is included. Also, given the authors are looking at salinity signals for Greenland melt, I have concerns about those signals being damped by this term. At the very least this is worth further discussion. Some comparison with other studies that don't use restoring, or have restoring of different strengths, would be good. Ideally, and even though the experiments with the nests are computationally expensive, it would be good to see what would happen if they were run without restoring, or at least compared to a 10 year integration period with the restoring.

In ocean-only models, salinity restoring is required to stabilize the AMOC in the forced configurations. The prescribed atmospheric forcing of the ocean-only model tends to create a fresh bias in the subpolar North Atlantic, which otherwise sends the AMOC on a declining trend. The restoring of sea surface salinity (SSS) as applied here is standard procedure and applied to many ocean-only models (e.g. Danabasoglu et al., 2014). For the simulations shown here, we emphasize that we apply a relatively weak correction ("rn_deds=-33.33 mm/d, i.e. 180 days for the model's top layer"), which is added to the surface freshwater flux but not under sea ice and not at grid nodes with runoff. In addition, we limit the SSS change per time step due to restoring to 0.5 psu (Behrens et al., 2013; Danabasoglu et al., 2014). This information is added to the manuscript as follows: "… applying a weak sea-surface salinity restoring towards the PHC3.0 climatology in the open ocean ($rn_deds=-

$33.33~mm/d, i.e. 180~days for the model's top layer, and $rn_sssr_ds=$0.5 additionally limits the associated salinity change) being added to the surface freshwater flux (c.f. Behrens et al., 2013; Danabasoglu et al., 2014).”

This being said, the restoring flux does indeed compensates some of the freshwater perturbation—or rather the response in P–E (net precipitation)—on basin scale.

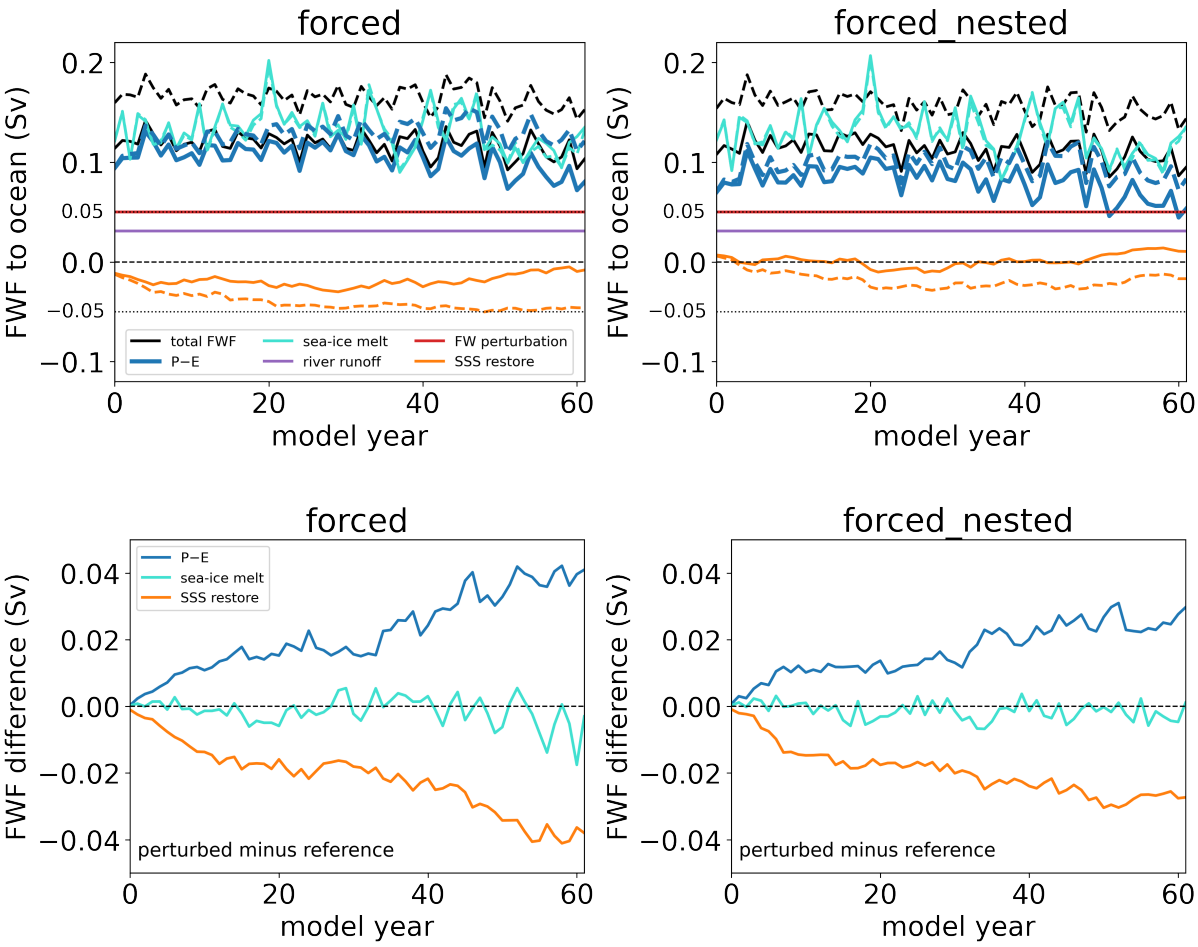

***Figure**: (upper row) Annual mean freshwater fluxes (FWF) and their decomposition integrated over the subpolar North Atlantic, incl. Nordic Seas and Baffin Bay. The total FWF includes P-E, runoff, SSS_restoring and FW perturbation; sea-ice melt is a separate contribution. All solid lines depict the reference run, dashed lines the perturbation experiment, the sea-ice melt flux barely changes, runoff and perturbation are climatological fields. (lower row) Differences of these fluxes between reference and perturbed run.*

Firstly, we note that the integrated restoring flux is always negative in the non-eddying configuration whereas it varies around zero in the nested, eddying one (solid orange lines in upper row panels in Figure above). This stronger freshwater withdrawal is necessary in the former because the simulation features a major fresh bias in the central North Atlantic (see Figure 3c in the manuscript). Secondly, the restoring flux accumulated over the SPNA (see figure above) is smaller than the freshwater perturbation injected, which holds in especially for the nested configuration (upper right panel) but less so for the non-eddying one (upper left). And thirdly, the cooling of the subpolar North Atlantic in the perturbation experiment reduces evaporation while precipitation is prescribed and hence unaffected by the simulated ocean state. Therefore, the freshwater perturbation creates an increase in the freshwater surface flux to the ocean. The restoring acts quite efficiently against this being of similar magnitude but opposite sign on basin scale (see figure above, bottom row). The restoring

thus mitigates part of the missing negative temperature feedback in the forced experiments by compensating for the temperature feedback on the surface freshwater flux (dotted black arrow T -> *F* in Figure 1 of Griffies et al. (2009)). The lack of atmospheric feedbacks still affects the surface heat flux acting to diminish the negative temperature feedback associated with a change in AMOC strength. All this is now also discussed in the paper (section Discussion/Atmospheric coupling).

Re-running the perturbation experiments without SSS restoring (or with restoring from reference runs prescribed) is beyond the timeline of the manuscript submission. Being able to quantify the effect of the restoring and explain its role here is sufficient, we think.

We are not aware of similar studies explicitly stating the role of SSS restoring in scenarios with freshwater perturbation ("hosing"), which could be used for a comparison. A systematic study on the influence of surface salinity restoring is found in Behrens et al. (2013), which is now referenced in the manuscript.

Historical vs Pre-Industrial: This is first discussed for lines 110-112. I know the authors work to justify this choice later in the paper, but I think this choice needs greater justification and discussion.

As we replied to Reviewer #1, we note that running the ocean-only experiment with historical (instead of repeated year) forcing but the coupled ones under pre-industrial control conditions was a compromise to have sufficient internal variability in the former and to isolate the impact of fresh water from other global warming signals in the latter. This will be further discussed in the detailed response and argued for in the revised version of the paper.

Averaging Periods: The authors explain why they use different averaging periods, and add Appendix A as a justification. This still feels like a concern in the transient experiments, since a longer averaging period means more Greenland meltwater added to the ocean, and a longer period that potentially means in can propagate farther. I would like to see some comparison with averaging over the same period, to help confirm that the results are not being biased by the variable averaging periods.

The effect of the averaging period on the response of the AMOC is, for example, included in Figure 4 and Table 2, where we also show the distribution and mean response for the 20-year period of 43-62 years after onset of the perturbation in the coupled runs. We discuss that this result is more prone to be influenced by multi-decadal variability. We argue against an expansion of the averaging period of the forced experiments to 50 years using Figure A1, which shows a clear trend in AMOC decline prior to year 40 of the simulation. The comment of having more freshwater added and allowing it to spread further by selecting a slightly later and longer averaging period for the coupled experiments is an interesting aspect. This certainly would be an issue in identifying time scales of the responses, which we refrain from doing, and focusing on the large-scale response patterns, it is again the coupled configurations with the later/longer averaging period, which show the weaker responses (despite having more freshwater added in the end). Figure A2 compares the SST response for different averaging periods (e.g. 20 and 50 years), and while there are local differences, the large-scale patterns are robust. Based on such investigations, we concluded that it is rather internal variability than the timing of the averaging period that causes the larger uncertainty and decided for a longer averaging period for the coupled experiments. Nevertheless, we will improve this discussion in the manuscript using the concerns expressed by the reviewer as guidance.

Specific points:

L23-25: "leaving the ice sheet at a negative net mass balance" doesn't read well.
rephrased to "so that the ice sheet's net mass balance was negative in each of the last 25 years"

L38: Note sure exactly what "indicate robustly" means
rephrased to "consistently show"

L71: "most critical improvements by the grid refinement" doesn't read well.
replacing "most critical" with "major"

L87: "including entire Greenland" is missing some words/explanation
rephrased: "… in the Atlantic to study subpolar processes and to include the entire coastline of Greenland for …"

L88: Might be worthwhile to clear explain what is meant by a strongly eddy ocean
adding "…, i.e. resolves the Rossby radius, …" for explanation

L92: McWilliams
done

L99: extended
preferring present tense here

L113: "much more" – much isn't needed, more pronounced says the same thing
correct

L113: What exactly is "strongly meandering" compared to just meandering?
removed "strongly"

L117: height
corrected

L122-123: "promotes intensified, partly overly pronounced deep convection" doesn't read well
rephrased to "yields more intense, sometimes overly strong deep convection"

L131: by "data extending beyond Greenland is not considered though", do you mean you haven't included the other non-Greenland glaciers in the dataset? If so, say it directly.
agreed, statement is now a separate sentence: "Data from glaciers outside of Greenland is not considered."

L131: on the annual mean
corrected

L133: What does "over 62 and 100" mean?
Figure 3 caption: What is an "Examplary improvements"?
changed to "Examples of improvements"

L142: Half of the icebergs melting in fiords – this needs to be referenced.
this would be Enderlin et al. (2016, doi:10.1002/ 2016GL070718.), citation added to manuscript

L144-145: "we find the prescribed freshwater rapidly mixed over the depth of the Greenland shelf by the ocean model also shifting the seasonal peak by a month" – no idea what this statement is trying to say.

Rephrased: "… we find that the ocean model effectively distributes the prescribed freshwater over depth on the Greenland shelf whereby a delay or accumulation arises so that the seasonal peak in freshwater content on the shelf is shifted by about a month compared to the prescribed perturbation."

L155 "much more pronounced" – more pronounced is good enough – the additional adjective doesn't really add anything
dropped "much", thanks for explaining

L169-171: Reference each of the listed process that the authors suggest the overturning is sensitive too.
We consider the dependencies between AMOC strength and subpolar North Atlantic temperature, salinity, surface heat flux and deep mixing as common knowledge. Also, this introductory sentence simply lists the parameters that we will investigate and discuss in the results section.

L175: Would be good to compare the model overturning strengths to observations, such as RAPID and OSNAP. Even if the paper's focus is understanding responses in different model configurations, it helps to understand the realism of different results/measures.
We added the following statement to provide an idea of a valid range for AMOC strength: "For comparison, the observed strength of the AMOC at 26.5°N is 17-18 Sv with a standard deviation of 3.4 Sv in monthly data (McCarthy et al., 2015; Biastoch et al., 2021)."

L187 "much more sensitive" – more sensitive is fine. Also, given the declines in Sv, might it be worth mentioning the percentage changes?
We removed "much" (also from other occurrences of "much more" in the text) and added percentages based on values given in Table 2.

Paragraph ending on L205: How does this propagation compare with other, previous, studies of Greenland melt.
We added the following sentence at the end of the paragraph: "The described redistribution pathways agree with earlier tracer simulations by \citet{Dukhovskoy2016} and \citet{Boening2016}."

L208: 'well' not needed, expressed is fine.
removed

L211: What is meant by the 'very eastern side'?
removed "very"; this addresses Figure 5b (as noted), specifically the freshening east of about 15°W. The sentence now reads: "… on the eastern side of the SPNA (east of $\sim$15°W), especially on the European shelf, …"

L220: coupled experiments – should it be plural? I.e. Is this behavior occurring in all coupled experiments?
No, this is intentionally excluding only the coupled (but not coupled_nested) experiment. And we found in the meantime, that it is mostly reduced evaporation (due to expanding sea ice) rather than sea-ice melt. Main text was amended accordingly.

L223: "Averaged over the top 200 m representative for the upper ocean" doesn't read well.
removed "representative for the upper ocean" (see next comment)

L224: "in some areas on annual mean" doesn't read well.
the sentence was rephrased:" Averaging over the top 200~m and all seasons, this cooling can reach 2.1--2.3°C in some areas."

L226: "This is except for" doesn't read well.
replaced by "An exception is"

L230: Nordic Seas.
corrected

L236: What is meant by "the mixed layer rather shoals"?
Replaced by "and accompanied by mixed-layer depth reduction in this case."

L238: Local regions of warming – Is this significant? Or just a minor detail?
rather minor detail, sentence removed

L243: sites
corrected

L243: "For simplification, we only show the spatial means for these areas and averaged over the top 200 m if not mentioned otherwise." Isn't clear and doesn't read well.
rephrased: "Further, we confine the volume-weighted averages of potential temperature and salinity to the top 200~m."
L244: Why does this comment about grid cell averaging suddenly appear? Is there any other way to compute averages on model grids where the area/volume spatially varies?
No, statement removed as it is the natural way to compute spatial means on grids.

L247: remove "being"
done

L255: "the large scale we focus on" – be quantitative, which will help this discussion.
replaced with "basin-scale changes we focus on"

L260: "...have approximately the same..."
done

L264-265: Why is the overflow water warmer if the mixed layers are deeper in the Nordic Seas?
Because a greater water volume is ventilated and would need to be cooled through approx.. the same surface area and over the same time period.

L280: "barely is a density change noticeable" doesn't read well.
replaced by "… the density change is minor."

L284: "presents with excessive freshening" isn't clear.
rephrased: "…where we find much greater freshening in the ENA."

Figure 10: Why use a line a latitude (60N) instead of the observational OSNAP line – would be useful for readers to look at the model fields where observations exists.
We have adjusted Figure 10 to show the model fields along the OSNAP cross-section. All main characteristics of the results also hold for this cross-section, which is located farther south on the western side of the Labrador Sea. Because of this shift, the section does not run through the main deep convection site of the models anymore and displayed mixed-layer depths are reduced in comparison to the line along 60N.

L289: Would a reference to Behrens et al be useful here?
agreed, citation added

L294: "it does not become clear" doesn't read well.
changed to "…it is not apparent from…"

Figure 11: Is MLD > 500 m appropriate for the ENA shelf? Additionally, can you try to estimate a formation rate in Sv, to help readers put the numbers in context compared to other studies? Also, how realistic are the areas of deeper MLs in the various simulations?
Deep convection in the ENA shelf region reaches as deep as 800m (in coupled configuration even 1000m) in March in the long-term mean of the reference experiment except for the forced configuration. Note, this is in fact the deep ocean region between 15°W and the European continental shelf.

L307: "enabling properties of the initialization fields still visible" – not sure what the authors are trying to say here.
sentence split and rephrased: "… coupled experiment (1500 years). Therefore, properties of the initialization fields are still visible in the deep ocean of the forced run."

L312: "show large content of overflow water" doesn't read well.
replaced by "feature large volumes of overflow water"

L318: "without though the higher resolution is not quite sufficient" – some words or explanation is missing
adding "… and hence lacks some mixing between the boundary and interior, which is well parameterized in the non-eddying configuration."

L330: shown
corrected, thanks

L339: suffers is probably not the best word here.
replaced by "is deficient due to"

L355: Irminger Rings entering the Labrador Sea interior – maybe add some references.
There are several references about eddies in this region included in the text already (Introduction and Discussion). In this sentence we describe what we find in our simulations and thus think no additional reference is required here.

L357: are crucial
corrected

L366: dynamically active (highly not needed)
dropped "highly"

L371: What does "largely improved" really mean?
replaced by "significantly mitigate"

L375: "well seen" doesn't strike me as formal scientific wording
changed to "illustrated by"

L388: In terms of imprints of the different Northwest Corner dynamics on meltwater tracer concentrations, are there any other studies that could be referenced/included in the discussion here?
Not that we are aware of. This topic is subject to a follow-on paper we are currently working on.

L418: "the coupled-nested configurations" – plural or singular – I.e. are you meaning the control and melt experiments with this setup?
singular, only the reference state is discussed here

Figure 14 caption: States magenta lines but I see red and yellow.
the ice edge of the reference state is depicted by a magenta outline in all panels

L428: "over the entire but mostly eastern SPNA" almost feels contradictory.
true, now reads "… across the entire SPNA with a maximum on its eastern side."

L445: Other studies have looked at the role of Ekman transports around the sub-polar gyre. Would be good to reference them.
references are included in the discussion section: "As shown here but also by earlier studies \citep{SchulzeChretien2018,Castelao2019,Duyck2022} upwelling favorable Ekman transport plays a significant role in spreading relatively fresh coastal waters offshore into the Labrador and Irminger Seas."

L450: Is this realistic. Is there a concern of the atmospheric scale being too coarse to look at processes around the narrow boundary currents. This comes up later in the paper, but would be good to mention here. Also, would be good to reference those works that have previously discussed Ekman transport's role in exchange from the WGC and LC.
By intention we gathered the entire discussion on this topic in Section 4 Discussion in order to prevent redundancy. This section is really only about our results and immediate interpretation.

L474: "explored in many model studies before" – add some references to those previous studies
these are listed in the introduction, to which we now refer in the beginning of the sentence: "As we note in the introduction, …"

L476: "passed decade already" – I think the authors may mean a previous decade?
we actually meant "past" but inserted "last" now

L484: being
corrected

L487: "Potentially in consequence thereof" doesn't read well
removed; sentence was rephrased: "Recently enhanced deep convection in the Irminger Sea \citep{Ruehs2021} may have compensated a lack of deep water formation in the Labrador Sea and hence offset an impact by recently increased runoff from Greenland." (see comment by Reviewer 1)

L492: What is meant by "does neither cover"?
replaced "cover" by "include"

L494: the objective of whether...
corrected

L501: Don't like the wording "quite typical" for the salinity bias – maybe explain this in more detail and more clearly.
Removed from sentence. Since we are asked to shorten the manuscript, we prefer to not go into this discussion.

L503: remove also
removed

L505: "doubting the importance" isn't a great choice of wording
sentence rephrased: "However, we cannot exclude a significant influence by the ocean and climate mean state, which differs between coupled and forced experiments."

L509: disagree may be a better word than oppose
good suggestion, thank you, changed accordingly

L522: I think Schulze-Chretien and Frajka-Williams should also be referred to here.
yes, indeed; reference added

L527: remove also
done

L535-540: Some other studies suggest resolutions up to 1/60th degree may now be needed.
Sure, this would improve the mesoscale and even submesoscale dynamics. But it is impractical at this stage of computational power for any global, multi-decadal ocean model applications. Also, Hallberg (2013) shows that the Rossby radius in most of the Labrador Sea can be resolved by a 1/20˚ model.

L548: remove "a"
corrected

L548: Gillard et al, (2022) – Ocean Modelling – looks at the some impacts on this exchange when changing the vertical resolution
Thank you for pointing this out. This reference is now included: "\citet{Gillard2022} recently highlighted the importance of vertical model grid resolution and an associated improved representation of the local topography for the exchange between the WGC and the interior Labrador Sea. An aspect our nested simulations do not reflect as the vertical resolution of 46 levels is the same as in the non-eddying configuration."

L585: Not sure what "presenting with the strongest weakening" is really saying
replaced "presenting with" by "simulating"

L636: "present with diverse sensitivity" doesn't read well.
This sentence has been removed. The final statement now says: "Climate model experiments disagree on the potential impact of Greenland meltwater among all other consequences of global warming \citep[e.g.][]{Swingedouw2006,Mikolajewicz2007} but typically result in a weaker AMOC response than forced ocean models \citep{Martin2022, Swingedouw2022}. However, such coupled simulations should not be dismissed per se in favor of very high resolution ocean-only configurations as recently conveyed by \citet{Swingedouw2022}. Our results emphasize that large-scale atmosphere-ocean feedback and local winds are as important as simulating a strongly eddying ocean."