Review on : “On the ocean’s response to enhanced Greenland runoff in model experiments: relevance of mesoscale dynamics and atmospheric coupling”

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General comment:

I thank the authors for the submitting this manuscript, they present here a new ensemble of freshwater forcing (FWF) experiments around Greenland with four simulations each in a different models configuration. The paper reads well and show a great knowledge of the processes governing North Atlantic ocean’s dynamics. Figures are clear and response of the increased freshwater is nicely described as well the role of atmospheric feedbacks and mesoscale dynamics. Summary and conclusions were particularly well-written. That being said, the paper is quite long and it would benefit from being synthesized. I also propose below some specific and technical comments in order to improve the paper.

Specific comments:

1 The forcing:

It unclear to me why the forced simulation were prescribed an atmosphere with a transient forcing while the coupled one have a preindustrial one. Comparing an historical forced simulation to a preindustial coupled simulation mixes the role of the atmospheric feedback and anthropogenic warming, why constant forcing was not used for the forced configuration?

2 The AMOC:

The AMOC has a large decadal to multidecadal to multicentennial variability (Ortega, 2015) depending on the model. The long term period chosen (100 years) is thus a rather short, AMOC could be experiencing a trend (see DOI: 10.1175/JCLI-D-13-00651.1, their figure 4). By comparing the period 51-100 years to the mean state 1-100 in the coupled simulations, you are mixing the response of the FWF and internal variability. Same period would appear to be a more clear comparison rather than artificially increasing the signal by changing the period. Additionally, AMOC could not respond to FWF the same way if it is on its stronger or weaker phase, so it would be useful to have a figure showing the time series of the annual AMOC, and what was its state when the perturbation was added. Last, the mean response stays within the limits of the internal variability, so it should be mentioned that it is not significant.

3 The periods of comparison and internal variability:

The choice of the periods of comparison is key to this study because it impacts all the results. This question is discussed only in the appendix while it seemed rather central to me, and it could benefit for being a bit more structured and clarify. Figure A2 shows averages over periods, but we are lacking some times series to give us an idea of the decadal variability in the Labrador sea for instance. The response in the coupled simulations could be a result of the internal variability: as the system is chaotic, changes in initial conditions could lead to another state. Seems to me that taking a long-term time mean is not enough to take out the several feedback effects from the system (Swingedouw, 2007b). I understand that this paper have chosen to do one member (run) per configuration but please maybe add a paragraph discussing and clarifying this issue (forced signal from FW versus forced signal from GW etc…).

Technical corrections:

1-Introduction

l.20: add a citation after “decay”-
“as well as” → “as well as is”
l. 58: “to shedding” → “to shed”
l.60: description of (a), (b) and (c) experiments is not clear, please describe the whole experiment in one time, for example: you will compare 4 simulations with freshwater forcing (FWF) to the same simulations without FWF and those 4 simulations are : one coupled, one forced, both with and without nest.
l.67: introduction the question of the mean state question is a bit abrupt, maybe explain a bit before line 66 why it is has to be addressed with one citation
l. 71: “by” → “from”

2-Model configurations and experiment

Table 1: the term “spatially varying” is bit misleading: the spatial distribution is kept constant in time right (*cf “The perturbation is constructed from the monthly-mean runoff plus discharge fluxes of Bamber et al. (2018) by averaging the period 1992–2016”, line 129*)? Please clarify that either in the legend of the Table 1 or in the text

l.107: specify or give a bit more information about what “model parameter” you are referring to

l.117: “hight” → “height”

l.127: “most” → “mostly”

l. 134 “62 and 100” → missing the word “years”

l. 138: not clear why “maximum runoff in June to August” is simplification, I guess this relates to the line l.144: “shifting the seasonal peak” maybe reformulate to make it easy to follow and specify what should be the real maximum month for the runoff to get out of the fjord into the open sea

l. 148: is the error calculated here computed from he loss of tracer concentration along the experiment? Please specify

3-Results

l.154 and 155: “variations in internal variability” → “internal variability”

l.155: “which evolve freely within the preindustrial boundary conditions provided” → “which atmosphere evolves freely under preindustrial forcing” the term “ boundary conditions” is used for regional modelling, when we prescribe values at the spatial boundaries of the model domain. For climate simulation, better to use the term “forcing”.

l. 157: suppress: “which are the same for each simulation”, already said line 98 and in Table 1

l.159: “can only be expected to exist after several decades” justify this choice of time frame, maybe by adding a citation

3.1 Ocean mean states and responses

AMOC

Table 2: “Denmark Strait (DS) overflow potential density” → is it the annual mean?

Figure 4: The caption is unclear, please explain what are the dark blue histogram and maybe add “(light blue)” after “perturbed states”.

l.180: you are not coupling to the same atmosphere, the slower AMOC in the coupled simulations could be the results of the transient forcing

Large-scale upper ocean salinity and freshening

l.199: add reference to Figure 1 to show transportation of FW

l. 201: Salinity is decreased a lot along the western coast of Europe in the coupled non-nested simulation. Are the FW leaking towards the subtropical gyre as seen in other hosing experiment (Swingedouw, et al. 2013; Devilliers et al , 2021), maybe showing a larger map could answer that?

l. 203: I do not see a more realistic Gulf Stream separation in the coupled nested response than in the coupled response (Fig5 b, left) Please correct the statement.

l.210: 1 → 1 psu

l.219: Add a figure of the sea-ice, or “(not shown)”

l. 229 ENA is defined later (line 247)

l. 230: “Nordic Sea” → “Nordic Seas”

l. 237: “The two nested experiments both feature an overall stronger inflow of Atlantic water into the Nordic Seas” I do not see that in the figure, please explain

Water-mass transformation:

This subsection is 6 pages it self, far larger than AMOC, salinity and temperature responses (1 to 2 pages each), please consider to reduce it or making it a 3.2 section to have some equilibrium

l. 243: “sights” → “sites”

l. 251: which is due to a weaker AMOC in the non-eddying simulations

l. 257: but coupled simulations also present with a stronger AMOC, bringing more warm water into SPNA

l. 264: question ?

l. 269: “source waters” → “water sources”

Figure 8: add the Labrador sea shelf region to be coherent with Figure 6

l. 270: Figure 9 shows that density seems more different between Labrador and Irminger sea in forced non-nested than in coupled nested

l. 273: “Moreover, the coupled runs exhibit a stronger salinity”: I see that only for the coupled nested simulations

l. 273: “thus density gradient” → “thus stronger density gradient”

l. 275: “more detail” → “more details”

l. 277: “The freshwater perturbation leads to a freshening and cooling in the ENA and on the ENA shelf in all configurations” → I disagree: Fig 9 shows a warming in the ENA shelf for the non nested simulations and in the ENA for the forced-nested (comparing circle and cross)

l. 291: “similar pattern” →“similar pattern to ENA”

l. 295: it is consistent with the reduction of the convection activity in the Lab. Sea (Fig 7, b)
l.305: “a consequence of the shallower deep convection in the forced configuration” → “a consequence of the shallower mixed layer in the forced configuration (see Fig. 7)”
l. 318: “but the 1/10◦ ones without though” → reformulate
l. 326: “not shown”, isn’t it shown in Figure 5 a)?
l. 330: “(not shwon)” → “(not shown)”
l. 331: “the least” → “less”. Figure 13 is cited before Figure 12, please exchange figure numbers.
l. 341: Figure 7 a) does not show that “mixing across the SPNA [...] is enhanced compared to the non-eddying configurations”, not for the coupled one at least
l. 342: “in both experiments” : which ones? It decreases more in the forced than in the forced-nested

3.2 Mesoscale dynamics
l.348: “This is” → “These are”
l. 352: you would need the same figure at ½ degree to compare to use the word “improves”, please change to “display a realistic...”
l. 354: “For example”: This is not an example of why “the finer resolution [...] is inadequate to simulate the full dynamical mesoscale spectrum.” please re-organize
l .356: “much closer to observations”, a citation is need here to justify the numbers
l. 360-361: “over/underestimation” is not the best term since there is no comparison to observation here so we do not know if the deep mixing is over/underestimated maybe use “stronger/weaker” instead?
l.362: “in the nested perturbation experiments”, please add the depth you are referring to (50 meters I guess)
l. 363: “highlight the necessity of using at least 1/20 ◦ grid resolution” → “suggest that the resolution may not be high enough with this model”
l. 370-373: add references to figures.
l. 390: “ocean below 1000 m.” → “ocean below 1000 m for the configurations with eddy parameterization.”
l. 395: “stronger meridional density gradient in the NAC region”, add a reference to Figure

3.3 Atmospheric coupling
Figure 14: there is one extra parenthesis in the caption. Seems like the coupled-nested displays values on a coarser grid than the forced-nested?
l. 417: “In the non-eddying configurations,” → “In the coupled non-eddying configuration,”
l. 421: “can adjust to changing” → “can adjust”
l. 426: “the upper ocean cooling [...] reduces the temperature difference between ocean and […] atmosphere” → you mean that in the forced model, it is the upper ocean who adjusts to the atmosphere to reach equilibrium? Maybe add a little more details about the surface heat flux estimation in a forced model, or a citation where this is explained
l. 455: “this results”: you should should mention you are referring to the response to to FWF
l. 457: “southward expansion of the sea-ice edge” : the extension is not very clear and wind response has a lot of noise, maybe worth to be mentioned
l. 458: “The particular reinforcement of the onshore Ekman transport” is that a stable feature in the the coupled-nested configuration or period dependent? Is it more or less constant along the simulation, have you tried different time-slices?

4 Discussion

l.475: “decade” → “decades”
l. 487: “to hindering” → “to hinder”
l. 488: “Potentially in consequence thereof, enhanced deep convection in the Irminger Sea has certainly offset any impact of recently enhanced runoff from Greenland on deep water formation.” I do not understand that statement as enhanced deep convection means impact on deep water formation
l. 493: why the plan of the results is not kept here? As: first mesoscale eddies and second atmospheric coupling
l. 503: “support stronger deep convection” you mean “support stronger reduction of deep convection”
l. 504: “surface heat loss is less than 10%” this is because atmosphere is adjusting along the simulation, not so sure this questions the importance of a positive feedback
l. 506: “to doubting” → “to doubt”
l. 510: “In” → “in”
l. 536: “The eddies resolved in our model are obviously not sufficient for bringing enough meltwater to the deep convention sites to achieve results comparable to Böning et al. (2016).” this is contradictory to the statement of before
l. 534: “larger eddies, […] which carry relatively fresh water from the boundary current into the interior Labrador Sea”, so the resolution is sufficient to carry the freshwater
l. 548: “a apply” → “apply”

5 Summary and Conclusion

l. 599: “deviates” → “deviate”
l. 604: “We note, that” → “We note that”
l. 610: “are” → “is”

Appendix:

Figure A1: These figure are hard to read, please zoom in the forced and forced-nested and add a figure showing the annual five year running mean to display the phase of the AMOC. Explanation of the orange line is unclear, maybe add a formula
l. 653: “varibaility” → “variability”. “we can attribute the larger variability to the explicit simulation of mesoscale eddies” → explain a little bit more maybe how the parametrization of the mesoscale processes leads to such a lower seasonal variability
l. 659: “By” → “by”
l. 662: “(cf. 2)” → “(cf. Figure A2 or section? 2)”
l. 663: “stabel” : stable – no I rather see steady decline, since each month AMOC_perturb – AMOC_control < 0
l. 664: again, I do not understand what you are summing here
l. 665: missing parenthesis
l. 666: “30+” → “30”. “we find a relatively stable state for the last 30+ years for the coupled experiments” you mean a stable difference?
l.667: “the adjustment period is likely shorter than in the forced experiments due to the overall weaker response.” I am not so sure about that see general comments
l. 668: “Therefore, we simply use the second half of these experiments to improve statistics.” not comparing with the same period, you are mixing the signals, maybe add a figure comparing the same period to show the difference
l. 669: “to improve statistics.” : to which statistics are you referring to?
Figure A2: move it before bibliography