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Title: The Atlantic's Freshwater Budget under Climate Change in the Community Earth System Model
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Point by point reply to reviewer #2

November 27, 2020

We thank the reviewer for their careful reading and for the useful comments on the manuscript. We marked the points we have already addressed with ticks.

1 Reviewer Summary:

The manuscript "The Atlantic's Freshwater Budget under Climate Change in the Community Earth System Model with Strongly Eddying Oceans" by Jüling, Zhang, Castellana, von der Heydt, and Dijkstra provides a detailed analysis of the salt/freshwater budget of the (North) Atlantic and the role of mesoscale eddies in meridional transport and changes thereof under global warming. This is a very thorough study also validating the importance of explicitly resolving mesoscale eddies in global ocean/climate simulations and estimates of the bistability of the AMOC. The analysis and results are well embedded in existing literature and thus are an important contribution to the on-going discussion on AMOC stability and eddy-resolving ocean simulations. I recommend publication of the manuscript after considering the following minor comments.

2 Minor Comments:

1. ✓ *Title: For most parts of the paper the discussion focusses on the salt/freshwater budget of the North Atlantic and only little analysis and information is provided for the South Atlantic and its import pathways through Drake passage and Agulhas leakage. I thus suggest to add "North" to the title: "The North Atlantic's Freshwater ..."*
   (Title) While we see the merits of the argument, we believe we should keep the more general title because we focus on the import of freshwater at 34°S, maps are provided for the whole Atlantic+Arctic, and we analyze the transport and flux terms also south of the equator.

2. ✓ *1f Please add specific ocean grid resolution information to the abstract: "We investigate the freshwater and salinity budget of the Atlantic and Arctic oceans in two configurations of the Community Earth System Model (CESM), one with a strongly eddying ocean on a* 0.1° *grid and one of coarser, non-eddying resolution (*1.0°*) typical of CMIP6 models."*
   (l.1) we added the grid spacing to the abstract.

3. ✓ *27 "salt-advection feedback" should have a reference, e.g. Peltier and Vettoretti (2014)? [add. references provided below]*
   (l. 27) reference added.

4. ✓ *28f This sentence could use a reference as well, for example Behrens et al. (2013)*
   (l. 28) added reference.

5. ✓ *33f "17 Sv at 26.5°N" Is this based on observations, e.g. RAPID, or your model simulations? Please add reference, for RAPID: Moat et al. (2018) or Smeed et al. (2018), the latter already used in the manuscript at a later point (line 423). Should be cited here as well.*
   (l. 30) added AMOC strength RAPID references: Moat et al. (2018), Smeed et al. (2018), and Frajka-Williams et al. (2019).

6. ✓ *37 "0.8 Sv of relatively fresh Pacific" reference? For example Woodgate and Aagaard (2005)*
   (l. 37) added the suggested reference for Bering Strait transport.

7. ✓ *39f "Freshwater is also exchanged with the Mediterranean Sea which is strongly evaporative." I would rather term this a salinity exchange, because Mediterranean outflow is very salty, i.e. the Atlantic provides "freshwater", which is in this terminology somewhat awkward.*
   (l. 40) rewrote sentence incorporating the suggestion.

8. ✓ *42 "... and advect salt meridionally when there is a zonal salinity gradient." In the same sentence it is said that the gyres are wind-driven. This part sounds like they are driven by a zonal salinity gradient. I suggest to rephrase this part: "... and advect any zonal salinity gradient also in meridional direction." In context with the previous sentence, this most importantly means that the salinity differences caused by precipitation patterns in the ITCZ are advected poleward by the gyres. Maybe this should be stressed more.*
   (l. 42) reformulated the sentence as suggested.

9. ✓ *43 "under greenhouse gas increases" rather is "under increasing greenhouse gas concentrations"*
   (l. 45) reformulated the sentence as suggested.

10. ✓ *58-66 very nice, brief explanation of the impact of freshwater import from the south on the salt-advection feedback. However, in principle the AMOC does not import freshwater to the North Atlantic but rather negative salinity anomalies (in models often a virtual salt flux anyways, see line 105). Also, a note on the calculation of F_{ovS} would be helpful, i.e. the typical reference salinity and whether zonally averaged velocities are used (AMOC streamfunction) or actual transports in 3-D are computed / is there a standard in place already?*
    (l. 60) We now mention the reference salinity in the introduction and refer to the appendix for the details of the calculations.

11. ✓ *117 How does the difference in vertical ocean grid resolution (42 vs. 60 levels) affect overflows in the North Atlantic? In particular resolution at Denmark Strait has the potential to significantly affect the AMOC.*
    (l. 132) additional text now clarifies this:

    > The 0.1° POP2 model grid has 42 levels to 6000 m while the LR-POP2 grid has 60 levels to 5500 m. In contrast to the HR ocean grid with its partial bottom cells and explicitely resolved overflows, the LR-CESM grid is defined with complete bottom cells and uses overflow parametrizations, e.g. between the Nordic Seas and the Atlantic (Smith et al., 2010). In the 0.1° POP2 model, the explicitely modeled Nordic Seas overflows compare favourably to observations (Ypma et al., 2019). The Mediterranean Outflow is not parameterized in the 1° POP grid but is modeled with a widened Strait of Gibraltar. Ultimately, the effect of the different vertical resolution is hard to disentangle as the horizontal mixing is represented very differently.

12. ✓ *132 more precise: "Green lines in Figure 1b,c mark the bounding latitudes which ..."*
(l. 154) specifically mentioning green lines now.

13. ✓ *165 Meaning of this introductory sentence not quite clear. Circulation changes between models must affect much more than just Bering Strait exchange and Mediterranean outflow. Maybe simply drop this sentence? Or move to line 199.*
(l. 188) rewrote this introductory sentence.

14. ✓ *174f please provide depth in meters (not km)!*
(ll. 194, 247 and others) changed all depth units to meters.

15. ✓ *174 The reference should more clearly point to Figure 3d for the northward extent of AAIW, which by the way seems not to reach 20°N (maybe 10°N) as stated here, and Figure 3j for the low salinity signature of AAIW.*
(l. 194) changed description as suggested.

16. ✓ *176 Figure 3i shows a section at 34°S and thus cannot serve as a reference for the addressed bias at 15-30°N.*
(l. 198) removed reference to Fig. 3i.

17. ✓ *191 a/dd "modelled" in "... this is the salinity of modelled North Atlantic ..."*
(l. 215) changed as suggested.

18. ✓ *227 I assume brine rejection is counted as negative freshwater flux into the ocean; add parentheses: "... sea ice melt (and brine rejection) ... defined as positive (negative) freshwater fluxes ..."*
(l. 255) made suggested changes.

19. *244, 284ff and Appendix B: It is not quite clear to me whether your method of computing eddy transports accounts for eddy induced velocities from the GM parameterization, which I believe is used in the LOW model run. While it is quite obvious that the velocity field of LOW is much smoother than the one in HIGH (as you point out for Figures 5d and 5e), the unresolved eddy fluxes should partly be compensated by the eddy mixing scheme (GM as noted only later in line 289), which would provide eddy induced velocities. These should be included in the eddy transport discussed with Figure 7. In this respect, the comment on line 280f should be moved upward and included in the introduction to section 3.3. [I was a bit impatient when reading this page and would have preferred to read the discussion of lines 280ff earlier. However, when reading this page again, I now think the structure is OK only that a small comment in line 259 would help, such as "In the following we take a closer look at these two differences."*
(l. 290) added the note as suggested.
(App. B) The GM induced velocities are significantly smaller than the explicitely calculated velocities (Nooteboom et al., 2020). The associated salt and freshwater transport is very small compared to the advection terms by the explicitely calculated velocities. We will add a Figure in Appendix B to show this.

20. *248 Providing the year 2100 value of the linear trend seems a good way to limit the effect of internal variability in illustrating the changes under RCP scenario. However, it would be helpful to also note the correlation and significance of the linear trends in the text (or a table?) as a goodness-of-fit affirmation.*

[how to best implement this?] appendix figure with R/p-values vs. latitude for HR/LR-CESM

21. ✓ *257 The obvious difference between the green CTRL lines (F_{eddy}) of HIGH and LOW runs should*

be pointed out first before discussing the deviations in trends, i.e. that LOW does not "exhibit the negative transport trend" at the SPG-STG boundary.
Each paragraph describes the CTRL mean state and RCP trend of one freshwater transport component. The second paragraph of the 'Meridional transport of freshwater' subsection is concerned with the total transport, while the paragraph starting at line (l. **??**) described the eddy term. In the original manuscript we introduced Fig. 9 only in the second paragraph before the total transport which may have obscured this structure. We now introduce the figure in the first paragraph and mention the concerned term in the beginning of each paragraph.

22. ✓ *272 here or earlier: reference Yang et al. (2020) for shift in ocean gyres [see full reference below].*
(ll. 211, 392) mentioned that the shift is expected under climate change and inserted the reference.

23. ✓ *275 It could be noted in addition to the present discussion that the differences between LOW and HIGH are not only due to resolving eddies but also due to a generally better representation of boundary currents (azonal flow system) in HIGH, which I assume is the case.*
(l. 304) mentioning this point now.

24. ✓ *280f this statement should be made earlier (see comments on line 244 and App.B).*
(ll. 287, **??**) as explained in above (line 257 comment), we changed the structure of the text to resolve the issue.

25. ✓ *343 It would be helpful if you indicate the bar color you are referring to. For the 32% vs 18% increase I assume you mean the total, i.e. red bars. This is a very nice, detailed analysis. And I also like Figure 9 very much but it is somehow difficult to keep track of the bars (colors) each sentence refers to. Adding hints for the color would help to link text and figure.*
(l. 379 and whole 'Freshwater budget' subsection) We added more color hints throughout the subsection and rephrased some sentences to facilitate the understanding of the plot. The percentage increases actually referred to the total surface freshwwater flux which has now been clarified in the text.

26. ✓ *345 If sea ice is one of the few bigger differences between HIGH and LOW worth noting, then please add this flux to Figure 9! A "(not shown)" is not very satisfying here.*
(l. 381) we recalculated the Arctic surface fluxes and focus now on the much stronger enhancement of the hydrological cycle of HR-CESM compared to LR-CESM; sea ice export and import only plays a minor role and we do not mention it anymore.

27. ✓ *363 I cannot see the advantage of presenting the spin up timeseries for this discussion. (see more comments on Figure 10)*
See comment for Fig. 10 below.

28. ✓ *391 one reference to Jüling et al. (2020) is sufficient in this line. Also, this citation lacks a journal and DOI in the reference list. Is the paper accepted/published already?*
(l. 434) removed the second reference. Jüling et al., 2020 is currently in the review process of Ocean Science and is thus accessible (DOI:10.5194/os-2020-85; see reference below).

Tables

1. ✓ *Table 1 In addition to the start year of the RCP run, please provide the length of the CTRL run (spinup?). Otherwise it looks like the high-resolution model was only spun up for 200 years, which*

would be very short to study the AMOC. Did these runs branch off of any longer CESM spinup?
(l. 118) we added a description of how the simulations were initialized.

2. ✓ *Table 2 I recommend to use sign + for transports into the Atlantic-Arctic basin. (I would think the Mediterranean provides a net (virtual) salt inflow to the system, i.e. +1.2 and -0.032.)*
(Tbl. 2) changed sign as suggested and updated table caption.

Figures

1. ✓ *Figure 1: please use fewer colors for all plots to enhances visibility of actual differences.*
(Fig. 1) reduced the color increments to 1 K (K/century) for all panels.

2. ✓ *Figure 5: the offset applied to the linear trend lines is not necessary, I think.Just use tone down the color a little bit, then it can be placed right in top of the smoothed timeseries without information loss.*
(Fig. 5) removed linear fit offset and changed color to grey.

3. ✓ *Figure 7: thick and thin lines of CTRL and RCP are barely distinguishable. Please increase the difference in thickness or use dashed lines for RCP.*
(Fig. 7) changed RCP to dashed linestyle.

4. ✓ *Figure 10: I depreciate the change in timescale. I think this gives a wrong impression on the trend under RCP w.r.t. the equilibration process. Also, I cannot grasp the purpose of showing the spinup period at all. Why do you not focus on the last 100 years in both cases? Further, I suggest to add labels for the regimes defined by the sign on the y-axis, e.g. on the righthand side y-axis.*
(Fig. 10) the time scale change has been removed and the values of both simulations are now plotted in a single planel. The regimes are labelled now on the right. The Figure caption and references to the figure in the text have been updated accordingly.
We still show all available data including the spin-up of the control simulation to make the point (l. 492) that the negative F_{ovS}/Σ sign of the HR-CESM simulation before 150 years is an artifact of it not being equilibrated. This is depite no trend being apparent in the first 100 years.

5. ✓ *Appendix B: Does the velocity v in your calculations for the LOW model include eddy induced velocity components from an eddy parameterization such as GM? I think this should be included for a fair comparison between LOW and HIGH. The salinity distribution will inherently include the effect of such parameterization but does v?*
See answer to comment 19 for a discussion of the GM term.

6. *511: Why annual mean? Since you have mean(vS) from model output you can compute the eddy transport also on monthly basis and thus eliminate the effect of seasonal variability from your calculation.*
(ll. **??**, 319, 555) calculated the eddy term now with the monthly output fields so as to avoid seasonal effects as suggested. We also adapted the text to reflect these changes.

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