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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 #1

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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:

This manuscript gives a very detailed description of the components of the Atlantic Freshwater budget and how it changes under future climate projections. The study uses two versions of the CESM with different resolutions, one which is strongly eddying and the other which parametrizes eddies in the ocean. The results show that the higher resolution model has smaller biases than the lower resolution model. The authors also show that there isn't much difference in the response of the AMOC to the future CO2 projections between the high and low resolution simulations. While there are aspects of the freshwater budget between the high and low resolution simulations that similar the high resolution simulation has the additional transport due to eddies at the gyre boundaries. I feel that the detail of the freshwater budget presented in this study is of interest to the scientific community. However, while I feel that the scientific content is sound and contents is well organized, I believe there is quite a bit of room for improvement to this study.

2 Major Comments:

1. *Throughout the entire study references to existing literature is a bit on the sparse side. For instance, the second paragraph of the introduction only has one reference. References where values based on observations/models are stated should be included (e.g. line 33 – Frajka-Williams et al. (2019) (or Smeed et al. (2018)) for AMOC strength or line 37 – Woodgate and Aagaard (2005) for Bering Strait through flow). Another example is when discussing the freshwater budget comparing how the results in the paper support or contradict other previous studies (i.e. Skliris et al. (2020) in their figure 11 do something very comparable to the manuscript's figure 9, Similarly, the studies Yin and Stouffer (2007) and Mecking et al. (2016) also do freshwater budgets).*
   (l.30) added Moat et al. (2018), Smeed et al. (2018), and Frajka-Williams et al. (2019) references to AMOC strength.
   (l.37) added Woodgate and Aagaard (2005) reference to Bering Strait transport.
   We will methodically go through the manuscript and check for missing references to previous work.
2. ✓ *A major difference between the high and low resolution simulations is the model's ability to handle eddies. Therefore, it would be nice to see a figure showing the differences in the eddy activity between the HIGH and LOW models (e.g. something similar to Delworth et al. (2012)*

their figure 14).
(Figs. 4 and A4, l. 149) added and Atlantic and global map of the standard deviation of the observed sea surface height and the model dynamic sea level as in Brunnabend et al. (2017).

3. ✓ *The short names HIGH and LOW used through the manuscript when they aren't followed by the word simulation makes a quick read of some sentences confusing (e.g.line 158,178,213,etc).*
Throughout the manuscript we changed the names of HIGH and LOW to HR-CESM and LR-CESM, respectively.

4. ✓ *It would be very helpful to have some of the figures include extra panels (redone to show anomalies (differences between CTRL and RCP) as opposed to just the absolute values. In particular, this would be very helpful for Figures 7 and 9, making it a lot clearer what the changes in the future climate projections are. The text would probably benefit from a few small tweaks to reflect these figures this as well.*
(Fig. 7) added panels to show the linear trends. We further removed horizontal grid lines and and legends in (b) to focus attention on the data.
(Fig. 9) added a panel to show only the trends. For further changes see comment below.

5. *For several reasons I find Figure 9 nice but at the same time too complex. I really like comparisons of budgets using bar charts since the make it easy to see the relative differences between the different components. Panel a is good but panel b contains too much information. There is a lot of information on panel b, perhaps breaking panel b down into several panels will make it simpler, e.g. have a panel of surface flux breakdown and another for advection break down. The axis on the bottom of panel b are also confusing, first of all that they are different sizes for horizontal and vertical and where do the units, Sv/100yr come into play? Also, the arrows on the end of the bars are difficult to see differences between the HIGH and LOW simulations. It would be helpful to make an anomaly version of this figure.*
(Fig. 9) We will update this figure.

3 Minor Comments:

1. ✓ *At a few points in the manuscript a freshwater and salinity budget is mentioned, but only the freshwater budget is discussed. Even though they are quite similar it is probably worth just mentioning freshwater budget. (e.g. line 1 and other places)*
(ll. 1, **??**, 357,494) removed mention of 'salinity budget'.

2. ✓ *Line 26 – Mecking et al. (2016) also showed this*
(l.25) reference added

3. ✓ *Lines 32 and 37 – Do you mean approximately instead of some?*
(ll. 30, 37) replaced 'some' with 'approximately'.

4. ✓ *Line 44/45 – What about heat flux changes? i.e. Gregory et al., 2005*
(l.47) We now mention the results of Gregory et al. (2005).

5. ✓ *Lines 51-66 – The references Weaver et al. (2012) and Liu et al., 2014 are also quite relevant for this paragraph.*
(l.66) added these references in the appropriate position.

6. ✓ *Line 66 – reference? Is this line even needed?*
(l.70) we added Drijfhout et al. (2011) as a reference for CMIP3 models and Weaver et al. (2012) for CMIP5 models to mention the sign of the two CMIP generations under increasing radiative forcing.

7. ✓ *Line 90 – You are comparing ocean-only (Deshayes et al., 2013) to coupled simulations (Mecking et al., 2017). The ocean only simulations use salinity restoring which is potentially the reason why the ocean only simulations have a negative F_{ovS} as opposed to the difference in resolution.*
(l.93) clarified that Deshayes et al. (2013) uses an ocean only setup while Mecking et al. (2017) analyzes coupled models.

8. ✓ *Line 113/114 – What impact does only using CO_2 have?*
(l.124) We added the following clarification to the description of the RCP scenario:

> In 2100, the radiative forcing of CO_2 alone is $6.9\,W\,m^{-2}$, or 80% of the $8.5\,W\,m^{-2}$ of the RCP8.5 scenario (Vuuren et al., 2011). Not prescribing land use changes, has no effect on the global mean surface temperaturature in the RCP8.5 scenario (Davies-Barnard et al., 2014). Compared to the mean warming in 2100 of the two RCP8.5 CESM1/CAM5 simulations submitted to CMIP5 at 4.4 °C (Meehl et al. (2013); time series available at https://climexp.knmi.nl/CMIP5/Tglobal/), our LOW RCP simulation warmed only 2.9 °C, or 66% of the RCP8.5 value. The reduced warming until 2100 is both because of the aforementioned reduced radiative forcing, but also the fact that our simulation started from a nearly equilibrated, and hence relatively warm, year 2000 control simulation.

9. ✓ *Line 139 – reference for HadISST missing*
(l.160) added Rayner et al. (2003) as the HadISST reference.

10. ✓ *Line 141-142 – Is there a warming trend in the beginning of CTRL before the reference period for this study is taken? Since the reference period of HadISST is 10 years before 2000 and 20 years after, I would naively not expect a warming.*
The surface climate of the CTRL simulation adjusts strongly until year 50 (as seen in the GMST), after this there is a very small warming trend (much smaller than the RCP GMST trend) as the deep ocean continues to equilibrate and a very small radiative imbalance remains. As mentioned in the text, since the CTRL simulation is nearly equilibrated it is expected to be warmer than a transiently warming historical climate (even if the 30 year observed climatology used here is actually centered around the year 2005).

11. ✓ *In some 2D figures there is a line of missing data for the LOW simulations (e.g. Figures 1,3c,i,4b,6).*
(Figs. 1,2,3,4,6,8) fixed this cartopy issue in all maps.

12. ✓ *Line 148 – Reference to indicate that the warming hole response is expected i.e. Drijfhout et al. (2012)*
(l.170) added sentence about the 'warming hole' and the suggested reference.

13. *Line 162/Figure 2e – The integration of the surface fresh water fluxes is typically done North to South because that's the direction of the barotropic flow through the Atlantic (i.e. Skliris et al. (2020) Figure 6, Mecking et al. (2017) Figure 6), this way it will line up to the difference between inflow into the Bering Strait and outflow at 34S*
(l.185) We only integrate P-E, not P-E+R as the mentioned studies, to compare with the P-E

ERA-Interim data. This data does not include a runoff mask that would enable us to compare this term as well.

14. ✓ *Table 2, it would be nice to also include the transports at 34°S (or as close to it as possible, see Bryden et al. (2011) for an observational estimate).*
(Tbl. 2) appended 24°S transports to the table.

15. ✓ *Table 2 – how is salt transport defined and how does it relate to freshwater transport?*
(Tbl. 2) added the definition to the table caption.

16. ✓ *Line 180 – I don't understand the barotropic streamfunction computation, the constant of integration should have an x/longitude dependence and not along the coast of the Atlantic side of Africa*
(l.202) In the calculation of the streamfunction the coasts of islands and continents have constant values implying no flow across these coasts. As we focus here on the Atlantic with only few and small islands, we display the streamfunction referenced to the African coast streamfunction value. In the HR-CESM simulation, the barotropic streamfunction is diagnosed and written out as ocean model output. Our approximation agrees well with this model output field. We chose to present a consistent estimate for both models.

17. ✓ *Line 211 – By colours do you mean shading?*
(l.240) changed wording as suggested.

18. ✓ *Line 221 – Reference for AMOC strength*
(l.249) added Frajka-Williams et al. (2019) reference for AMOC strength

19. ✓ *Line 229 – The freshwater loss seems large (i.e. larger than CMIP5, see Skliris et al. (2020) Figure 6)*
(l.258) we checked the calcuation again and this is the total surface flux, we noted that this is larger than the CMIP5 ensemble mean freshwater loss.

20. ✓ *Line 234 – Which linear trends are being referred to here?*
(l.264) clarified in the text that these linear trend values referred to the total surface freshwater flux.

21. *Figure 7 – why is 36N masked?*
(Fig. 7) We will fix this.

22. *Figure 7 – Do freshwater transports through the Bering Strait and Strait of Gibraltar change?*
(Fig. 7) They change very little compared to other changes, the trends will be included in a new version of Figures 7 and 9.

23. ✓ *Figure 7 and 10, most obvious in Figure 7, the thick and thin line thicknesses are very difficult to distinguish.*
(Figs. 7, 10) changed RCP to dashed linestyle.

24. *Line 256 (some other places later) – it would be nice to reference earlier figures/figure panels at the end of sentences to help make connections i.e. line 256 –Fig. 6k/l and line 266 Fig. 7*
We will add references to the figure panels.

25. *Figure 8 – Is it worth including 34S section and/or Atlantic zonal mean?*
We will plot the section salinity trends at 34°S similar to Fig. 3. The zonal mean may not add much information as azonal trends as seen in Fi. 8 are averaged out.

26. ✓ *Figure 8 – the freshening trend in the SPG is in line with the warming hole that goes along with a weakening AMOC (i.e. Menary and Wood (2018) Fig. 7) – should be mentioned in the text below*
    (l.331) added a sentence describing this and referening the suggested study.

27. ✓ *Line 305 – Any idea why there are these differences in salinity?*
    We abstain from speculating as this requires a more in-depth study of the local salinity budget which is outside the focus of this study.

28. ✓ *Line 321 – 'durface' should be 'surface'*
    (l.353) fixed typo

29. ✓ *Line 361 – Also in Liu et al. (2014)*
    (l.400) citation added

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