Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-41-AC1, 2020

Ocean Science
Discussions
Open Access

Interactive comment on "Deep water formation in the North Atlantic Ocean in high resolution global coupled climate models" *by* Torben Koenigk et al.

Torben Koenigk et al.

torben.koenigk@smhi.se

Received and published: 5 August 2020

Thank you for your time and the constructive comments. Below, we will respond to all comments. All changes we suggest to include in a revised manuscript (including changes in figures and tables) are highlighted in the "SuggestedChanges_Manus-Tables-Figures_4August2020.pdf" (see supplement). Although, we are aware that we are not supposed to include a revised manuscript in this reply, we found it more practical for both us as authors and you as reviewer to highlight the suggested improvements into the manuscript itself rather than attaching all figures and tables separately.

The aim of this paper is to investigate the effect of increasing the ocean and/or atmo-

Printer-friendly version

Discussion paper

sphere resolution on the modelled deep convection in the North Atlantic. Let's start with the major comments: 1) The results shown are far less clear than described. The authors work with 7 models. The abstract indicates that for "most models, higher ocean resolution leads to increased deep convection". In the text (line 230), this already is reduced to "generally", NEMO models only (5/7). But tables and figures show that it is not the case for EC Earth or CMCC, so we are down to 3/7. ... I suggest a less exciting but more honest rephrasing of the results section, which clearly explains that the results are model-dependent. And which investigates what in the ocean component (or in the atmosphere component, as you later suggest it is all set by the wind) causes it.

Response: We modified the abstract to make clear that not all 7 models increase the ocean resolution but only 5 of them do (ECMWF, HadGEM, CNRM, EC-Earth, AWI). The other two (MPI-ESM, CMCC) only increase the atmospheric resolution (see Table 1). 4 out of these 5 models where the ocean resolution is increased show a clear increase of Labrador Sea convection with increased ocean resolution (even EC-Earth, see Figures 1 and 3 and table 2). The only model where ocean resolution does not lead to increased deep convection in the Labrador Sea is thus the AWI-model and here - as discussed at several places in the manuscript - this can be linked to a (unrealistic) strong reduction in the wind stress in the high resolution version. The MPI-model uses the same atmosphere and suffers from the same problem in the high-resolution model; thus the impact of increased atmosphere resolution in MPI on convection is very likely linked to this. In the CMCC model, increased atmosphere resolution has hardly any effect on the convection in the Labrador Sea. Thus, we find for all NEMO-models, increased deep convection in the Labrador Sea with increased ocean resolution. The effect of atmosphere resolution seems to be smaller or is dominated by an unrealistic decrease of wind forcing (AWI, MPI). The question remains if this is only a NEMO-feature. The relation between increased ocean resolution and reduced deep convection in the GIN Sea is not as robust. We modified this part of the sentence in the abstract to make this clear.

2) Section 4 is not robust, and its methods are not detailed. Section 4.2 treats of the potential relationship between horizontal freshwater fluxes and deep convection. Nowhere are the freshwater calculations presented. All that we know comes from line 386, that the liquid component is the "vertical integrated liquid freshwater export". How did you do that? Was it on each model's grid, or interpolated onto a regular section? What is your reference salinity? Did you take the same for all models (e.g. use the same ref. salinity as in past literature), or have one per model (e.g. each model's mean deep salinity in the GIN seas)? Moreover, you show no data / result for most of the section. At least, add the mean or min/max transport values to Table 2. Same for section 4.1: the SHF discussion is based on data you show, but not the NAO/wind one. Show it! Show where the NAO lies in these models (centres are most likely shifted depending on the resolution, see literature).

Response: To calculate the liquid freshwater transport, we used the model grid lines on the native grids of the models that are closest to the geographical landmarks that define Fram Strait (across 78°N), Northern Baffin Bay (78°N) and Denmark Strait (66°N). The freshwater has been defined as the amount of zero-salinity water required to reach the observed salinity of a seawater sample starting from a reference salinity. Specifically, liquid freshwater transport (fwt, in m3/s) is estimated as

fwt=$\int_{p1}^{p2}\int_{D}^{\eta}$âŰŠãĂŰ((S-Sref)/Sref)ãĂŮ dzdx

(please find a better view of the formula in the supplement) for salinity S (in practical salinity units). Reference salinity Sref was considered as 34.80 psu. The integration along z is performed from the bottom at depth D to the sea surface at height η (in this case η=0). p1 and p2 are the landmarks and the integration was done considering dx as the length (or depth for dz) between every grid point. The solid freshwater transport is calculated from the sea ice transports across the sections assuming a constant ice salinity of 5 psu. We added these explanations to the manuscript.

Printer-friendly version

Discussion paper

We added a new Table 4 showing the freshwater exports through Fram Strait, Baffin Bay and Denmark Strait and discuss the results in section 4.

We added a figure showing the NAO-winter pattern in all the models (new Figure 12): We performed a Pearson correlation between geopotential height at 500 hPa and North Atlantic Oscilation (NAO) index during winter (JFM mean) for ERA5 and PRIMAVERA models. The periods used, were 1979-2019 for ERA5 and 1950-2014 for PRIMAVERA models. The NAO index was considered as the leading EOF of geopotential height on the 500 hPa pressure surface over the European/Atlantic sector (80W-40E, 20-90N). While the pattern varies across models and across different resolutions of the same model, no clear effect of the resolution on the NAO-pattern can be found.

—————————-

3) How is accuracy defined? In section 3, your comparison observation - models was hard to follow as you never defined what an accurate model should be. For example, line 237, you write "the only simulation that shows similar values in the Labrador Sea as ARGO is EC-Earth3P-HR". ARGO is at 3.95; EC-Earth HR, at 0.95. But HadGEM-LL and MPI XR are at 4.3 and 4.6 respectively, and CNRM CM6.1 is at 1.09. What is wrong with these, that made you reject them in favour of EC Earth?! Same again line 298: ARGO is at 6.5 and AWI HR at 6.1, but this model is not mentioned. It feels like an extra criterion is required for you to accept a model as accurate, but you never specified it, so the reader is left with their confusion.

Response: We clarified the table description to make clearer that only the row with the observations (row 2) of table 2 shows absolute values, while the other rows show the ratio between modeled and observed values. A value of 1 would mean that the modelled DMV (or SHF) is the same as the observed one, 0.5 that it is half as large and 2 that it is twice as large. Thus, a value of 0.95 for EC-Earth means that the EC-Earth value is close to the ARGO value while a value of e.g. 6.1 for AWI-HR means that the DMV is 6.1 times as large as the ARGO value. Thus, comparing these two models, EC-Earth is much closer to ARGO than AWI. This hopefully clarifies the confusion with the model-observation comparison in section 3. We fully agree that it is difficult to clearly formulate how large a model is allowed to deviate from ARGO and still can be called “realistic”. It is very subjective to formulate a criteria for ”accurate” and given the fact that the DMV results from ARGO are rather uncertain as well (see discussion in the manuscript), we prefer to not formulate such criteria. We thus replaced the statements that a model is well reproducing the observations by stating more specific how much the different models deviate from the observations.

---

4) Other methodological, rather major comments You purposely exclude the Irminger Sea, even though a lot of models convect here instead of the Labrador Sea. Why? And why not investigating whether deep convection shifts from the Irminger to the Labrador Sea depending on the resolution?

Response: Figure 1 shows that none of the models used in this study convect in the Irminger Sea instead of the Labrador Sea. Only the ECMWF-LR model shows the largest mixed layer depth in the Irminger Sea but this convection is not very strong either, and thus not compensating in any way for the missing convection in the Labrador Sea in ECMWF-LR. Some models have the largest convection not directly in the Labrador Sea but shifted to the southeast to the south of Greenland. However, the Labrador Sea box is chosen such that it also covers convections south of Greenland and not only directly in the Labrador Sea. Figure 1 shows that there is no clear shift of convection centres from Irminger Sea to Labrador Sea or vice versa with increased resolution. We focus on deep convection here, defined as convection potentially contributing to the AMOC, and the convection in the Irminger Sea can if it all only for short periods be classified as deep convection.

Line 200, you admit that comparing the models to the ARGO period would be better, but you do not. For the frequency analysis, I agree that you should not. But at least in table 2, you should.

Response: We added a column to table 2 where we compare the period 2000-2014 of the modelled DMV to ARGO and added discussions of these results to the manuscript. For the Labrador Sea, generally smaller DMV values occur in 2000-2014 compared to 1950-2015 as expected and thus somewhat reduced overestimations for most models. The two models, which fit best to ARGO using 1950-2014 as comparison, underestimate ARGO in 2000-2014. For GIN Seas, both increases and decreases of the DMV in 2000-2014 compared to the entire period occur. For some simulations these differences are substantial. This is also due to the large natural variability of the DMV with decadal or longer periods with strong or weak convection activities. Since we can not assume that models are in the same phase of natural variability as the observed period 2000-2015, it is not clear if comparing only this period in the models is really reducing the uncertainty in the comparison (which was the reason that we initially only compared the entire 65 years).

Section 5 is introduced as having already been done in Roberts et al. (subm). So, why having section 5 at all? What is different from Roberts et al.?

Response: Roberts et al. have analysed only mean values from the historical runs by showing scatterplots of mean AMOC against mean DMV. Here, we analyse the variability by showing the cross-correlations of the AMOC against DMV in the control runs to isolate the unforced internal variability. Therefore, both studies are different and complementary. We clarify this issue in the manuscript by changing L415-427 of the original manuscript from: to “Roberts et al., (submitted) also analysed the relation between temporal mean values of the DMV and the average AMOC in the historical runs. They found that there is a strong relationship between DMV and the AMOC strength across models; models with more deep water production in the Labrador Sea have a stronger AMOC. Also for all single models (apart from AWI- CM-1-0), simulations with larger DMV are linked to a stronger AMOC. This relationship is less robust between DMV in the Greenland Sea and the AMOC as expected from the reduced DMV with increased resolution in most of the models (see sections 3.2.2). To investigate the impact of variability in the deep water formation on the variability of the AMOC, we performed cross-correlation analyses between the DMV in Labrador and Greenland Seas and the AMOC. . . “

——————————— To finish, here come some more minor comments, by order of appearance:

The introduction is mostly about the AMOC and its relationship to deep convection. But the AMOC is not the topic of this paper (apart from section 5, see above). Please modify the introduction, focussing on deep convection and its importance for the ocean and the climate in general, including the AMOC sure, but not only.

Response: We rewrote almost the entire introduction to better structure it and focus more on the deep convection itself. We first introduce deep water formation, focusing then on it is importance for water masses, followed by the effects on local and remote climate. This is followed by a longer section focusing on the AMOC and potential linkages between deep convection and AMOC before we shortly describe the aim of the study and discuss potential effects of high resolution.

———————————

Throughout the text, you use Greenland Sea when you mean GIN or Nordic Seas. Please correct.

Response: We changed it to GIN Seas.

———————————

The figures really need to be improved. The most crucial ones are the line plots, especially Fig. 3, where black and blue, or magenta and red, or red and green, are on the same panel, with the same line style. First, as the lines are supposed to represent an increase in resolution, what about some colours that are more intuitive? With e.g. LR in blue, moving to green, then yellow (with black contours), then orange, and finishing at XR with red. Then, to help the reader distinguish the lines, vary their styles. Again, just an example, make every other line dashed, and/or vary their thicknesses.

Response: Since model grids are different and ocean and atmosphere resolution increase differently between different models, it is difficult to find a color scale, which shows the resolution of each model configuration across all models. We decided therefore to change the colors in all the time-series and line figures as follows: Blue (solid line) for the standard resolution for each single model. Red (solid line) for the high-resolution version of each model. For models with intermediate resolution versions (ECMWF, HadGEM), we added dashed green and orange lines.

Figs 6 and 9, as the aim is resolution comparison, give the same y-axis to all panels. And since in the text you comment on the "peak around 10 years", use a log10 x-axis instead of a log2.

Response: We changed the x-axis in Figures 6 and 9 following your suggestions. We tested to use the same y-axis as well, however, prefer to keep different y-axis for the different models since the power varies strongly among models and using the same y-axis makes it very hard to see anything on the plots. However, we changed it to log-scale as well.

Fig 11, the increase of SHF with resolution is not visible as the colour scales are saturated. This information can be retrieved from table 2, so up to you whether to also improve Fig 11. Response: We changed the color scale in Figure 11 to make it better visible.

———————————-

Line 260 (and Fig 4), which density are you using?

Response: We used the density of standard sea water (i.e. at 1 atm), as given by Millero and Poisson (1981): Frank J.Millero, AlainPoisson 1981: International one-atmosphere equation of state of seawater. Deep Sea Research Part A. Oceanographic Research Papers 28 (6), 625-629, https://doi.org/10.1016/0198-0149(81)90122-9 We added this information to the text.

———————————-

Section 4.1, clarify whether SHF > 0 means heat lost or gained by the ocean (from the figures and results, I assume it's lost by the ocean).

Response: It is a loss for the ocean, we clarified this in the text and in Figure 11.

————————————

Lines 377-378: spell out what the relationship means in practice, i.e. that larger DMV is associated with larger heat fluxes out of the ocean. Furthermore, comment on lagged relationship (calculate it if needed) to see which comes first.

Response: Large ocean heat losses in the winter are linked to strong DMVs in the following March, indicating that large upward surface heat fluxes lead the DMV. We clarified this in the text and commented on lag/ lead relationship.

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2020-41/os-2020-41-AC1-supplement.pdf

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-41, 2020.