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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

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Response to Reviewer 2:

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

Anonymous Referee #2 Received and published: 8 July 2020

This paper represents important material for further development of global climate model. The authors do a decent job in getting an overview of the different models and their performance. My comments are mainy regarding the formulations and figures, which could be improved.

Response: Thank you for your time and your useful comments. Below, please find point-to-point responses. All changes we suggest to include in a revised manuscript (including changes in figures and tables) are highlighted in the supplement file. 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.

Main comments: Introduction: in many places, the style is a fast shift between presenting 'settled knowledge' and presenting the moerations. This leads to very long sentences, which are difficult to read. For instance: L 72-74 (and how is it questioned?) and l. 75-79. The whole section must be revised according to this to become more clearly structured. The introduction, or may be the models and simulations section, must discuss 'high resolution' in relation to the Rossby radius and resolving eddies.

Response: We rewrote almost the entire introduction to better structure it. We now 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 describe the aim of the study, discuss high resolution (setting it also in relation to the Rossby radius) and potential effects of high resolution.

Power spectra calculated in Figs 6 and 9 and referred to in the text. As far as I can see, these 'spectra' are raw periodograms. They will have 100 independent points (for a 100 year simulation), and therefore, five points will exceed the 95% significance curve just by chance. So periodogram spectra don't tell much. There are ways to overcome this, see e.g. von Storch and Zwiers (1999). The spectral analysis must be improved along these lines. Also, it must be specified, how the red significance level is calculated. The spectra with red lines does not seem to describe the background spectra of the model data very well; this looks odd, and must be explained. Conclusions in text must be changed according to revised spectral analysis.

Response: The power spectra were calculated using a standard python program (https://pycwt.readthedocs.io/en/latest/index.html#) following the suggestions by Torrence and Compo (1998), which is widely used. The power spectrums were calculated with a Fourier transform, and red noise was used as background spectrum. To determine significance levels for Fourier spectra the method of Torrence and Compo (1998) assumes that different realizations of the geophysical process will be randomly distributed about this mean or expected background, and the actual spectrum can be compared against this random distribution. Afterwards Torrence and Compo derive the theoretical red noise wavelet power spectra and compared them to Monte Carlo results. These spectra are used to establish a null hypothesis for the significance of a peak in the power spectrum. The method by Torrence and Compo (1998) agrees with the one suggested by von Storch (see page 223 of the book). The theory for the Fourier transform, and for red noise (and its spectrum) are exactly the same (compare equation 11.23 of von Storch with equation 16 of Torrence and Compo (1998)). We added information on the calculation of the power spectra to the method section in the manuscript. We also, following suggestions from reviewer 1, changed the scale of the x, and y-axis.

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Minor comments: L 1-2 (title): The paper is comparing what could be called (present-day) standard resolution with higher resolution. The title should reflect this.

Response: We changed the title to " The effect of increasing the horizontal resolution on the deep water formation in the North Atlantic Ocean in HighResMIP models" to reflect this.

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L. 82: '..future model simulations ..', write e.g: '.. model simulations of future climate. . .'

Response: changed

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L. 87: 'The question whether . . .'. Why not write: 'It is still discussed . . .'

Response: changed

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L. 107: '..important role.' For what?

Response: For the AMOC. We added this to the sentence.

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L. 110: 'Climate-related processes' Be more specific, please.

Response: We replaced "…climate-related processes…" by "…and local and remote climate processes such as local sea ice cover or the large scale oceanic circulation"

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L. 113: 'increasinng the hjorizontal resolution' .. of GCMs.

Response: Corrected

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L. 117: Here you refer to a study with an eddy-resolving model. It should be made clear, that the present paper is not about eddy-resolving models.

Response: We rewrote large parts of the introduction and clarified this now, please see also our answer to main comment one.

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L. 134: An outline of the HighResMip-protocol and a reference are needed here.

Response: We added more details from the HighResMIP-protocol in section 2.1, being more specific about forcings of the coupled simulations that have been used in this study. We added also a short description of the spin-up. For further details we refer to Haarsma et al. 2016.

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L. 161: I cannot find a detailed description on how MLD is calculated in observations and in models. This must be added.

Response: The de Boyer Montégut et al. (2004) (0.03 kg m−3) variable density threshold is used to calculate the mixed layer depth in ARGO-data – see section 2.2. The ocean mixed layer thickness in the models is defined by the sigma t-criterion (Levitus, 1982) (variable mlotst following CMIP6-conventions). The sigma-t (density) criterion used in Levitus uses the depth at which a change from the surface sigma-t of 0.125 has occurred. We added this to section 2.3.

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L. 220: Where do you see that?

Response: You can see this if you compare Figure 2 to differences between 1° and 0.25° simulations in Figure 1 and 2 e to Figure 3. We clarified this in the text. Difference between DMV in ECMWF-LR (1°) (zero almost all the time) and ECMWF-MR (0.25°) is much larger than differences between different members. Same is true for the differences between 1 and 0.25° versions in the other NEMO-models compared to the spread across the ECMWF-ensemble.

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L. 231: Well , I se an around 50/50 split.

Response: All models with NEMO as ocean component where the ocean resolution

is increased (ECMWF-IFS, HadGEM3-GC31, CNRM-CM6-1, EC-Earth3P, see Table 2.1) show an increase in the DMV of the Labrador Sea. Note that MPI (not using NEMO) and CMCC (using NEMO) do not change the ocean resolution and AWI (as mentioned in the text; not using NEMO) show a reduction of the DMV with increased ocean resolution. We now specified the models and point to Table 2 .1 to clarify the statement.

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L. 259-261: I don't understand this sentence.

Response: The density profile in late winter is dominated by the convection itself; if we have convection, the density profile will strongly reflect if convection takes place or not. Thus, showing the late winter or March density profile does not really provide new information. However, if we analyse early winter/ late autumn density profiles (e.g. in November), we can see if models already before the period with strongest surface heat loss show large biases in the stratification, which can explain the strength of the convection in late winter, or if processes during the convection period itself explain potential biases in the convections. We reformulated the sentence to: "Naturally, the models with more frequent and deeper convection show a much weaker vertical stratification than the models that do not exhibit deep convection. We therefore analyse density profiles in the Labrador Sea in November that are not influenced by convection to explain why the mixed layer depth is overestimated in the following winter.

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l. 274: What is the spinup period?

Response: It is 50 years using 1950-forcing. We added this to section 2.1.

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L. 315: 'melting heat water fluxes'. What is that?

Response: We rephrased the sentence: This discrepancy could be due to the competing effects from global warming, which are represented differently in each model: on one hand, reduced sea ice extent enables a larger surface for deep convection, while on the other hand melt water and warmer surface water enhance the stratification and thus impede convection.

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L. 349 ’high-resolution models’. In ocean or in atmosphere?

Response: In the ocean. We changed the sentence to: “Increased ocean resolution improves the representation of the observed SHF pattern.”

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L. 419: What does this sentence mean?

Response: “Thus, the observations show a stronger AMOC with a lower DMV compared to the models, indicating other shortcomings in the representation of processes that govern the AMOC in the models.“: It means that observations can have a stronger AMOC despite a lower DMV compared to the models, or that the models with an AMOC, which is similar to the observed one, have a higher DMV than observed. The conclusion from this is that there have to be other shortcomings in the models, which might partly compensate the bad representation of the DMV. We reformulate the sentence to: “Thus, the linkage between mean values of AMOC and DMV in the models is not consistent with the observations. This indicates that other shortcomings in the representation of processes that govern the AMOC in the models exist. “

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L. 443: add ’. . . non eddy-permitting. . .’

Response: Some of the high-resolution models are eddy-permitting (maybe not eddy-resolving, although HadGEMGC31-3-HH might be called eddy-resolving), thus we

think it is misleading to add “non eddy permitting” here. However, we now clarified in the introduction that most of the models in HighResMIP and used in this study are not eddy-resolving.

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Table 3. Add columns with histroical trend – control trend.

Response: We added columns with the difference between historical and control trend to Table 3 and we show data in 1012 m3/s now for better readability.

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Fig. 2: Dont you yellow lines. They are really difficult to see.

Response: We replaced the yellow line with orange in Figure 2.

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Add a Fig. 2 showing MLD for models + ARGO (analogous to Fig. 3)

Response: We are not entirely sure if we correctly understood what you suggest. However, as described in the text, the ARGO mixed layer data consist of two data sets: the climatological mean MLD in each grid point in March in the years 2000-2015; second, the maximum (mean over the two largest observed values in the period 2000-2015) MLD in each grid-point. In many grid-points only a few ARGO-profiles exist and in some no profiles at all. Thus, it is unfortunately not possible to show any DMV-time-series from the ARGO data and we only calculated the mean DMV over the 2000-2015 period.

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Fig. 3: Labels like 5e+14 a really not nice to look at, please change them. Why is there only two lines in panel a). And please add a panel based on ARGO data.

Response: We now changed the y-axis in figure 3 and the other time-series figures to avoid such kind of labels. With respect to ARGO data, please see response to the comment before. The DMV in ECMWF-LR is 0 almost all the time, thus it is hard to see the third line in panel a because it is almost the entire time placed on the 0-line. However, following suggestions from reviewer 1, we changed the colors in all time-series figures and now this line is blue and you should see it a bit better.

Fig. 4: Instead of having a separate panel (a) for ARGO, put the argo stratification in the model-panels.

Response: We added the ARGO profile to the model panels and deleted panel (a) as suggested.

Fig. 10: Colors for HadGEM3 and CNRM-CM6 are indistinguishable, please change. Also the figure is hard to understand. May be points referring to the same model in different resolution can be connected with thin lines. You must experiment, and improve the figure.

Response: We connected different resolutions of the same model with lines now to make the effect of resolution clearer, and we chose a different color for CNRM-CM6.

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2020-41/os-2020-41-AC2-supplement.pdf

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