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Interactive comment on "Max Planck Institute Earth System Model (MPI-ESM1.2) for High-Resolution Model Intercomparison Project (HighResMIP)" *by* Oliver Gutjahr et al.

Anonymous Referee #1

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The paper compares climate simulations with MPI-ESM1.2 under HighResMIP protocol. The roles of different resolutions in both, ocean and atmosphere as well as the effects from using two vertical mixing schemes in the ocean, namely PP and KPP, are addressed. The work presents a big value for the climate modelling community and contains material which is worth publishing in GMD. I recommend this paper for publishing after minor revisions. Please see below my comments and concerns.

First of all I was positively surprised seeing this paper submitted to GMD because to my knowledge the developments of both, ocean and atmosphere components reported here were announced to be discontinued. I still believe that a lot of things can be done

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using this system.

Regarding MPIOM in the middle of the chapter 2.1 describing the setups I was surprised by the choice of the the GM coefficient which is 250 m2/s for 400km cell width. I find this value too small as compared to what is practiced by other models. For instance, in paper by Marshall, J et at. 2017 which is cited by the authors the value of 850 m2/s is reported for 1° resolution in the control run. It is about 13 times larger than what is used in this paper. The same, although not that extend, is true for the Redi diffusion. All this implies that the mesoscale eddies are basically neither parameterized nor resolved in simulations denoted with HR and XR. Going through other papers using MPIOM I found that the choice made by the authors is indeed canonical for this model. From this one may conclude that the baroclinic instability in low resolution setups is "indirectly parameterized" by some diabatic processes taking place in the model. These can be for instance the explicitly specified or numerical diffusion. According to Marshall, J et al. 2017 the MOC/AMOC are far too large when the small values are used although in the ocean only configurations.

XR_pp is characterized by a very small AMOC due to very low winds in ECHAM/T255 and associated collapse of the deep convection in the Labrador Sea. A similar behaviour regarding Labrador Sea MLD collapse has already been reported for several completely different climate models. In line 21, page 8 the authors discard the effect from the Southern Hemisphere saying that the collapse happens on a fast timescale. Although I tend to believe that the authors are right one still could speculate that the time range from 50 to 100 years is already not that fast. The colorbar range in Figure 5 is huge but one could see that the cold bias over ACC is coincidently the largest in XR_pp. Alternatively one may look at the difference between subplots in Figure 8 to see whether the slope of the halocline in the SO changes between simulations. Otherwise it is hard to guess it by eye. How do the interranual timeseries of the MLD in the Labrador Sea look like? After which year does the first Labrador Sea MLD collapse happen?

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In Figure 9 I would expect to see the sea ice in the Labrador Sea in XR_pp. Is it masked by the choice of colorbar. Honestly, I would add even more material regarding the Labrador Sea collapse as it has been often discussed in literature. See for instance the paper by Moriaki Yasuharaet al. 2008 in PNAS titled as “Abrupt climate change and collapse of deep-sea ecosystems”.

The authors solved the problem with too cold North Atlantic in XR_pp by changing the mixing scheme from PP to KPP. I speculate that it would be sufficient to increase the upper mixing coefficient in PP to parameterize for the wind induced turbulence but still encourage using KPP as there is more physics in it.

Even though XR_kpp is a reasonable simulation, the NADW is still not as well simulated as in ER_pp where the bottom cell in AMOC is nicely visible (Fig. 12). Most probably, playing with GM is still required in order to further improve the quality of XR_kpp. So far if all I wrote above regarding GM is correct, ER_pp is the only simulation which physically consistent deals with the baroclinic instability. Surprisingly, ER_pp looks fine even with PP.

In addition to showing the differences to climatology at the surface I would also suggest to plot these differences at other depths (1000 or 2000 meters). The drawback of using KPP might be that it propagates the model error further to the deeper ocean than it was with PP.

I would suggest the authors to elaborate a bit more on the text considering what I have written above. It will be not that descriptive then. The text is easy to read but I would recommend that some native speaker will read it. I found sentences containing things like “cold bias in the North Atlantic improves” in the text.

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