Author responses

Review of the revised manuscript "Comparison of ocean vertical mixing schemes in the Max Plank Institute Earth System Model (MPI-ESM1.2)"

I think the revised manuscript is greatly improved as compared to the previous version in terms of clarity. But I still have a few concerns.

I may misunderstand the main purpose of this manuscript. But based on the title and abstract I was expecting to see a more in depth discussion of the differences among the four vertical mixing schemes and how these differences lead to the differences in the simulated mean climate states in MPI-ESM1.2. However, it looks like that the authors would like to present the differences in the simulated mean climate states with the four vertical mixing schemes on a high level without going too deep into the causes of these differences. This is fair as I understand that it is hard to disentangle the climate responses to the changes in parameterizations in a coupled system. But I would suggest the authors to pay more attention on the differences between the four simulations with different ocean vertical mixing schemes, and perhaps a bit less on the common "biases" of these simulations. Otherwise I don't see much added value of this manuscript to the authors' previous publication Gutjahr et al. (2019), which, if I understand it correctly, is an overview of similar MPI-ESM1.2 simulations with a set of different configurations.

We thank the reviewer for his thorough reading and constructive comments. In the following, we answer all raised questions. Before doing so, however, we would like to mention that we are investigating coupled global simulations. The reduction of model errors is a main justification for incorporating new parameterisations into models. We agree that the detailed analysis of new parameterisations is important, but first we are interested in a general overview. In other words, we want to know whether incorporating a more advanced parameterisation is worth the effort, in terms of reduced model errors.

In particular I think there are a few pieces of information missing in the introduction section:
1) What is the main purpose of this study?
We thank the reviewer for raising these questions. We agree that the motivation was not as precisely as it should have been. It is correct that we compare climatic mean states on a high level, because we are interested in the overall effect a change of the vertical ocean mixing has in global coupled simulations. Therefore, we do not (and probably cannot) deduce the effects down to the process level. Such a study would require stand-alone (idealised) simulations, which we think were already published by the developers of the mixing schemes. We underline that investigating such differences in a coupled set-up is challenging because the ocean is coupled to the atmosphere in a complex way so that additional feedbacks need to be considered.

As outlined below, another “added value” is the introduction to MPI_ESM of a new class of mixing schemes (IDEMIX) that includes an improved representation of mixing in the ocean interior.

2) Why are these four schemes chosen, given the many other choices? Or why do you think a comparison among these particular four schemes in MPI-ESM1.2 is helpful?
The choice of comparing PP, KPP and TKE are motivated because these are the most widely used or traditional schemes in CMIP-type coupled models. We are not aware of a more exotic choice in any of the main modelling centres. An unwanted effect all these scheme share is that they rely on an artificial background diffusivity. From the perspective of energy conservation, introducing such an artificial mixing requires energy that is not taken from a reservoir but is spuriously created. This is an unwanted effect in a model. This is where IDEMIX comes in, because it replaces this artificial background diffusivity by a prognostic equation for internal wave energy, whose dissipation then acts as a source for mixing. Although IDEMIX can in principle be connected with PP or KPP, it is natural to combine it with TKE, because both are formulated in terms of energy budgets, which is

necessary to achieve a more consistent mixing scheme. The basic scheme can be extended by other processes such as Langmuir turbulence, lee waves, etc. but the only step forward to a more energetically consistent scheme is by implementing IDEMIX.

3) What do you want to get out of this comparison of the four vertical mixing schemes in MPI-ESM1.2?
There are at least two motivations. First, there was no study before that compares the traditional vertical mixing schemes in MPI-ESM1.2 because only PP has been used until 2019, where also KPP was implemented. Second, since the energy conservation in the ocean is of great importance it was necessary to also implement the TKE scheme and IDEMIX. As with all new implementations it is of interest what effect a different parameterisation has and whether model biases can be reduced. A special focus is laid on the combined TKE+IDEMIX scheme, because from theory it is the most energy conserving scheme of the scheme we compare. Moreover, there is only one other study that studies IDEMIX in a coupled model (Nielsen et al., 2018).

Then, in the presentation and discussions of the simulation results, I would suggest the authors to focus more on differences among these simulations. The present discussions appear a bit unfocused to me.
We think that we focus on comparing the differences between the simulations; however, not at a detailed process level, but at a higher level, because of the intention of the study. We present results of the mean states averaged over the last 20 simulation years (model year 80-100) and focus on model biases. This focus is justified since it is hoped that these biases are reduced or at least it is of interest to see how they are affected by choosing a mixing scheme different from the default option. For some areas, such as the Arctic Ocean/Fram Strait we can link changes in the model bias to the mixing, whereas in other areas, e.g. in Agulhas region, the change in bias results from changes in the circulation. Whenever possible, we try to discuss possible mechanisms, but for a more detailed analysis specific case studies are needed, which is not the aim of our study.

Specific comments:

L41-50: A sentence or two on why the four schemes were chosen given these many more may be helpful. Perhaps moving the paragraph of L29-35 here after this paragraph?
We explain in the text that KPP and TKE are the most widely used vertical mixing schemes in ocean models. In fact, there is not really an alternative to these two. We also explain that PP is still the default option in MPI-ESM1.2 and we therefore include it in our analysis. Of course, there are several extensions and modifications to KPP and TKE (such as Langmuir turbulence), but we consider IDEMIX for two reasons: we want to get rid of artificial background diffusivity and establish an energetically more consistent scheme. These two points are mutually dependent. We have inserted a paragraph at the suggested location to sharpen our motivation and goal we want to achieve with our study.

L64: "ist" -> "is"?
Corrected.

L69: "modified version PP" -> "modified version of PP"?
Corrected.

L82: What is the main difference from the CVMix version? Is the Marsland et al extension the only difference?
Indeed there is another difference we did not mention explicitly, but have added now to the suggested line: "… and in which the diffusivity is independent of the viscosity."

L110: "first" -> "upper"?
Corrected.

L115: Based on Table 1 it looks like you are using PP below the mixed layer for HR_kpp. Does that

mean the only difference between HR_pp and HR_kpp is the treatment in the mixed layer? How is the mixing in the mixed layer treated in HR_pp?
That is correct. The KPP scheme is a pure mixed layer scheme. Therefore, below the mixed layer the parameterisation of the viscosity and diffusivity is independent from KPP. Large et al. (1994) proposed a different formulation to Pacanowski and Philander (1981). However, to be consistent with the PP scheme and with Gutjahr et al. (2019), we use the PP scheme below the mixed layer, or more precise the ocean boundary layer. In the original formulation of Pacanowski and Philander (1981) there is no discrimination between the mixed layer and the interior ocean. That is one reason why the additional mixing induced by wind stress was added to the PP scheme at MPI. So the viscosity and diffusivity in the mixed layer are calculated by equation (A4) and (A5), with the additional term from the wind stress, Kw(z), being active (the term decays exponentially with depth).

L149-150: But it looks like the deep convection in the Labrador Sea is the strongest in HR_ide? What might be the reason?
That is true. It is not straightforward to pinpoint a process that is causing this. It is more that several processes act together to destabilize the water column. A possible reason could be that the inflow of salt from the Mediterranean and the Indian Ocean is strongest in HR_ide. This more saline water in the Atlantic reaches the subpolar gyre, which is reducing the effect of warming on the density with the result of higher density than in the other simulations. An indication for this reasoning is that the mixed layer depths are almost everywhere deeper in the subpolar North Atlantic and Nordic Seas with IDEMIX.

L171: I can see quite some differences in, e.g., the Eastern and Western Equatorial Pacific and the subtropical regions in the Atlantic?

That is correct. We revised the sentence in the manuscript as follows:
"The sea surface salinity is mostly unaffected by the chosen vertical mixing scheme, except in the Arctic Ocean (Fig.3) and in western and eastern equatorial Pacific, which could be related to differences in the feedback with the atmosphere."

L182: The simulations are forced by constant 1950s conditions whereas the PIOMAS represent the average of 1979-2005. Do you expect they will match each other if the model is perfect?
No, we do not expect to match PIOMAS with 1950s conditions. We have added the PIOMAS ice thickness to illustrate how the thickness distribution looks like in the (more) recent past. Since there is no other comparable data set for sea ice thickness, we think it is a justified comparison. At least from the perspective of climate warming, we would not expect thinner ice than in PIOMAS for 1950s conditions.

L184-186: Are you comparing with HR_pp? HR_ide seems to have an ice edge to the further north.
Yes, we compare with HRpp.

L188: By TKE here are you referring to both HR_tke and HR_ide?
Yes we do, but we have reformulated the sentence to make that clear: "The more northerly location of the ice edge in the KPP and TKE(+IDEMIX) simulations … ." We also made this clear in the sentence before and after this one.

L204-205: Comparing which vertical mixing schemes?
We refer here to TKE+IDEMIX: "…, for instance with TKE+IDEMIX the warm bias is reduced in the Arctic Ocean but enhanced for the MOW (Fig.6)."

L206: Fig 8 introduced before Fig 7?
Thank you for spotting this error. We have switched the position of Fig.7 and Fig.8. Now the salinity bias map is referred to before the inflow from the Indian Ocean.

L211-212: What about the salinity bias in the South Pacific?
That is correct; we did not mention this bias here. We have added a sentence to the paragraph: "We further notice a large salinity bias in the South Pacific that is linked to the South Pacific gyre.

We speculate that the bias is due to unresolved eddies in the East Australian Current and processes at the border zone of the South Pacific Current and the Subantarctic Front. A more detailed analysis is needed to identify the cause of this model bias, which we suspect to be related to model resolution but will not explore here."

L213: Is the streamfunction in Fig 7 integrated in the vertical over the full depth, or from the surface to some depth?

It is integrated over the full depth. We have added "vertically integrated" to the caption of now Fig. 8.

L219-220: You can probably see this by looking at vertical sections of the salinity in this region.

We have plotted a vertical cross-section along 36°N in the Atlantic showing the distribution of salinity in HRpp and the differences of the KPP and TKE(+IDEMIX) simulations. This figure shows higher salinity already in the Mediterranean Sea with KPP and TKE+IDEMIX (Fig.1b and d). At a depth of about 1000 m, where the overflows are in equilibrium with the ambient water, a clear imprint of the higher salinity can be seen in all three sensitivity simulations, but it is most pronounced in TKE+IDEMIX (Fig.1d). This result indicates that IDEMIX influences the watermass properties in the Mediterranean Sea and the mixing of the overflow plume.

We revised the relevant paragraph in the manuscript to: "However, the MOW is already saltier by about 0.4psu and 0.5°C warmer before it flows into the Atlantic. It remains a subject of further investigation what causes this warmer and saltier MOW in TKE+IDEMIX. Possibly, it modifies the variability of the near-surface wind field or the net evaporation over the Mediterranean Sea (Aldama-Campino and Döös, 2020)."

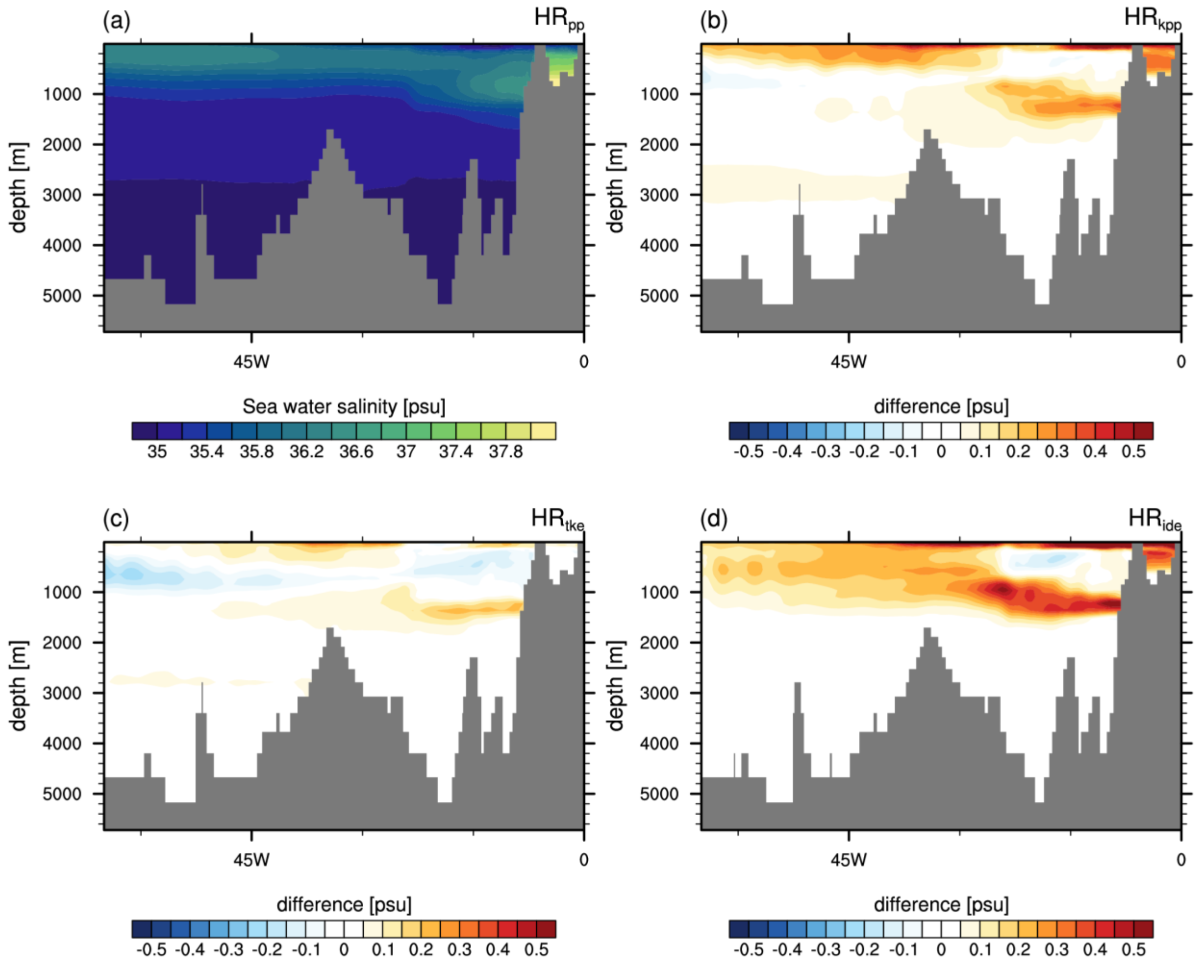

Figure 1: Vertical cross-section along 36°N in the Atlantic Ocean (averaged over last 20 model years) showing (a) absolute potential temperature in HRpp, and (b-d) the differences "experiment" minus HRpp.

L229-231: This reflects one of the issues in this set of simulations. If you run the simulations (forced by constant 1950s conditions) for longer, you might see different patterns of the "biases", right? So how sensible is the magnitude of the "biases" shown here in these figures?
Yes, it is possible that the bias pattern changes over longer integration periods. However, because we follow the HighResMIP protocol, we only conducted 100-year long simulations with these model configurations. Therefore we wrote at the end of section 3: "The simulation length of 100 years is too short to see pronounced effects in the deep ocean, but it could be expected that over longer periods (several centuries) the additional mixing from internal waves might affect the diapycnal diffusion of the upwelling deep water, e.g. in the Pacific." Thus, the biases and their differences between the simulations stem from quick (100years) responses. On longer time scales, also the slower reacting deep ocean becomes involved.

L233: Delete "of"?
Corrected.

L234-237: I'm confused, are you discussing Fig 7 in this paragraph?
Yes, we refer to Fig.7 (now Fig.8)

L245-248: This paragraph is confusing.
We have revised the paragraph to: "In the Nordic Seas the water temperature is higher in all simulations with KPP and TKE(+IDEMIX) than in HRpp. Although the higher temperature partly compensates for the increase in salinity, the overflows across the Greenland-Scotland Ridge are dense enough and reach depths of about 3000m. Their warmer temperature can be seen at and south of 60°N. Similarly, also the deep water formed in the Labrador Sea is warmer than in HRpp and together these water masses cause a warm bias when exported as the Deep Western Boundary Current."

L258-260: I don't understand this sentence.
We have revised the sentence to: "To compensate for the increased overturning in the Nordic Seas, the water in the Atlantic must be replaced by a stronger inflow from the Indian Ocean, which is the case in the simulations with KPP and TKE(+IDEMIX), as seen in Fig.8".

L361: Do you mean "warm halocline layer"?
In addition, brine rejection in the interior Arctic is less effective as a mixing mechanism because of the strong stabilising vertical salinity gradient.

L436: Delete "but"?
We revised the sentence to "Although not observed by Argo floats, all simulations show deeper mixed layers north of 50°S in the Pacific Ocean east of New Zealand and in the South Atlantic."

L438-439: What do you mean by "in addition, we compare them with control simulations"?
What we meant is that we compare simulations forced by constant 1950s GHG with ARGO observations of the recent past. We revised the sentence to: "It should be kept in mind, however, that comparing model simulations with Argo float data is always difficult because the floats do not measure continuously in time and space. Moreover, we compare the Argo float data of the recent past with simulations driven by a constant greenhouse gas concentration from 1950."

Fig 1: This figure shows the differences between the four vertical mixing schemes in the ocean interior very nicely. I wonder if it is worth to show their differences in the mixed layer as well, which may be more relevant to the ocean surface states such as SST, SSS and MLD.
Probably yes. However, we tried to find a trade-off between the number of figures and the information we want to show. We decided to have 2d maps of SST, SSS and MLD in the manuscript because these are important fields for the coupling, and furthermore readers are interested in these fields. We agree that a more detailed analysis of the mixed layer would be of interest in a future study.

References

Gutjahr, O., Putrasahan, D., Lohmann, K., Jungclaus, J. H., von Storch, J.-S., Brüggemann, N., Haak, H., and Stössel, A.: Max Planck Institute Earth System Model (MPI-ESM1.2) for the High-Resolution Model Intercomparison Project (HighResMIP), Geosci. Model Dev., 12, 3241–3281, https://doi.org/10.5194/gmd-12-3241-2019, 2019.

Review #2 of:" Comparison of ocean vertical mixing schemes in the Max Planck Institute Earth System Model (MPI-ESM1.2)." by Oliver Gutjahr et al.

The paper compares the effects of four different ocean vertical mixing schemes provided by the Community Vertical Mixing (CVMIX) library on the mean state simulated by the coupled model MPI-ESM1.2. Used are the Pacanowski and Philander (PP), K-profile (KPP), turbulent kinetic energy (TKE) vertical mixing schemes as well as a prognostic schemes for internal wave energy and its dissipation (IDEMIX) which is combined with TKE. The author addresses temperature and salinity biases in a global and regional context with sufficient analysis of there causes and impacts. Compared to the first revision round the paper has significantly improved in scientific impact but also in its phrasing and expression. All comments of the reviewer have been considered. The paper was restructured for a better understanding and new chapters for the impact of the mixing schemes on the sea ice but also on the coupled atmosphere have been added. These chapters are of particular value for coupled modeling community.

We thank reviewer 2 for his time and positive feedback.

From my point their are only minor things to change (linenumber with respect to file gmd-2020-202-ATC1.pdf):

line 67: "...submodel JSBACH3.2, and of the ice-ocean...", delete of
Corrected.

line 73: "...River runoff ist calculated...", exchange to "is"
Corrected.

line 79: "...is a modified version PP scheme..." exchange with "...is a modified version of the PP scheme..."
Corrected.

line 110: "...wave-wave interaction is accounted for that might...", please reformulate
We rephrased the sentence to: “When internal waves propagate, they can be damped by wave-wave interaction, which is taken into account in IDEMIX”.

line 153: "...approximately homogeneously distributed to leading order...", not fully clear what the author want to say here
We rephrased this sentence to: “Figure 1 shows the spatial distribution of the vertical diffusivity K for the model layer at 1020m depth. Apart from boundary flows, deep convection regions and the surface mixed layer, K is approximately homogeneous in the simulations with PP and KPP. Both simulations use a simple constant background diffusivity of $K=1.05*10^{-5}$ (m^2 s^{-2}) to parameterise internal wave breaking.”

line 203: "...Ice thickness is lower in all simulations than in PIOMAS..." exchange with "...Ice thickness is lower in all simulations compared to PIOMAS..."
Corrected.

line 226: "...where they affect to local...", exchange with "...where they affect the local..."
Corrected.

line 278: "...across the Greenland-Scotland Ridge at and ...", delete at
We revised the sentence due to a comment from reviewer 1 to: “In the Nordic Seas the water temperature is higher in all simulations with KPP and TKE(+IDEMIX) than in HRpp. Although the higher temperature partly compensates for the increase in salinity, the overflows across the Greenland-Scotland Ridge are dense enough and reach depths of about 3000m. Their warmer temperature can be seen at and south of 60°N. Similarly, also the deep water formed in the

Labrador Sea is warmer than in HRpp and together these water masses cause a warm bias when exported as the Deep Western Boundary Current."