Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2020-202-RC2, 2020

Geoscientific Model Development Discussions
Open Access

Interactive comment on "Comparison of ocean vertical mixing schemes in the Max Plank Institute Earth System Model (MPI-ESM1.2)" *by* Oliver Gutjahr et al.

Anonymous Referee #2

Received and published: 29 October 2020

In this manuscript the authors compared four different ocean vertical mixing schemes in MPI-ESM1.2 in 100-year long fully coupled simulations representing the conditions of 1950s. They focused their analysis on the mean state of important ocean variables, such as temperature, salinity, mixed layer depth and vertical eddy diffusivity and discussed the differences in these ocean vertical mixing schemes in explaining common biases in climate simulations, in both the global patterns and regional patterns at high latitudes. They found relatively small sensitivity of the simulated SST, SSS to the changes in the vertical mixing schemes but bigger sensitivity of the ocean interior. They conclude that 1) model resolution determines the global-scale bias pattern, and 2) vertical mixing schemes may play an important role for regional biases.

Printer-friendly version

Discussion paper

I think this is an interesting study. Comparing ocean vertical mixing schemes in a single modeling framework is also of practical use for the ocean and climate modeling communities. But I think there are some aspects this manuscript could be improved before being published.

General comments:

Throughout the manuscript, the authors are comparing the simulated results, which is the mean of the last 20 years of a 100-year long simulation under constant 1950s conditions, with the EN4 observation during the years 1945-1955, which is a state the simulations initialized from (or at least close to by nudging). I was wondering how sensible it is to use the word "biases" here to describe the differences, especially considering that the authors talks a lot about model biases reported in other studies in section 3 and 4 (which is good by the way). I just don't see how relevant the "biases" reported in this way are in terms of reflecting the "true" model biases in a historical climate simulation under transient greenhouse gas forcing. For example, do you expect the results of a "perfect" model to match the EN4 (1945-1955) observation in this experiment setup? I think it perhaps makes more sense to frame the discussion to focus more on the differences among the four simulations with different vertical mixing schemes and on which scheme, and in what ways, has the potential to fix the model biases reported in the literature, instead of targeting on a direct comparison with the observation, which I think will need more careful design of the simulations.

Related to the above comment, I think this manuscript could be improved by improving the clarity of the analyses in section 3 and 4. The thing I like about in these analyses is a summary of the relevant model biases reported in previous studies. However, I feel that the discussion of the simulation results itself is sometimes rather separated from these nice summary. I think the authors might want to be more specific in the reasoning and refer more frequently to the features in the figures in order to show what aspects of the different ocean vertical mixing schemes have the potential to fix the existing model biases reported in the literature. Sometimes I feel confused about which statement is

Printer-friendly version

Discussion paper

from the simulation results and which is from the literature.

Another thing I was hoping to see in this manuscript is some more insights of the differences among the four ocean vertical mixing schemes and more reasoning of how these differences in the schemes lead to differences in the simulation results. The authors discussed relatively more on the interior mixing below the surface mixed layer, which is quite simple especially for PP and KPP. But these scheme differ quite a lot in the mixed layer. For example, the implementation of KPP in this study used the same interior mixing as PP (according to Table 1), yet the results are often quite different between the KPP and PP simulations. It would be helpful if the authors could elaborate more on how the differences in the surface mixing contribute to the differences in the simulation results.

Specific comments:

L6: It is a bit unclear what you mean by "little sensitivity of the ocean surface", perhaps be more specific on what ocean surface variables and ocean interior variables, and be explicit on the sensitivity to changes in the ocean vertical mixing schemes.

L12: Are you comparing the effects of vertical mixing and the horizontal processes?

L13: How did you reach the first conclusion about the model resolution? Is the model resolution a focus of this study too?

L20: Temperature and salinity are active tracers

L20: "uptake" -> "ocean uptake"?

L23: Unclear statement. The complexity of a parameterization also depends on the physical and computational requirements in an ocean model. We could have a physically more favorable scheme based on our best understanding, but it could be too computationally expensive or not necessary for a simple model.

L26: I'm not sure if PP, perhaps even KPP, is "state-of-the-art". They are widely used

Printer-friendly version

Discussion paper

though.

L32: There are actually small modifications to the implementations of a certain scheme, such as KPP, happening throughout the time due to practical reasons, e.g., Appendix A of Danabasoglu et al., 2006

L33: Numerical implementation based on the same principles may also matter. See, e.g., the comparison of the CVMix version of KPP and ROMS version in Li et al., 2019.

L34-35: "schemes provide either direct vertical profiles" -> Perhaps something like "schemes diagnose vertical profiles of ... from surface forcing and background fields"

L36-37: I believe these schemes also only provides eddy diffusivity and viscosity when implemented in an ocean circulation model, not the fluxes. The key difference is that both PP and KPP are diagnostic which assume equilibrium with the current forcing and background state, whereas second-order schemes have memory of previous states.

L42: Briefly introducing ECHAM6.3 for those reader who are not familiar with this model? For example, "ECHAM6.3, the atmosphere model developed at ...,"

L42: What do you mean by unstable? Does the AMOC shut down?

L48: "it depends" -> "depending"?

L55: I think Olbers and Eden, 2013 is a more appropriate reference here.

L57: "not only represents" -> "represents not only"?

L63-64: Be more specific on "a minor effect on the climate state"?

L66: Confusing, please rephrase.

L96: Delete "control"?

L113: What do you mean by "unbiased effects"?

L114-115: If the coupling and feedbacks from the atmosphere are not discussed, why

Printer-friendly version

Discussion paper

not using the OMIP protocol to force the ocean model with atmospheric data? What is the advantages of using coupled runs here when comparing the four ocean vertical mixing schemes?

L119: "(section 4)" -> "in section 4"?

L121: Without being more specific focusing on your results, this statement is certainly not true, even for the ocean interior and excluding the effects of deep convection, which I assume you meant here. The vertical diffusivity is affected by many processes (such as internal waves, which depends on both the bathymetry as well as the surface forcing) and background state such as the stratification. A constant background diffusivity in PP and KPP is a simplification. You might want to rephrase, for example, to restrict it to the simple parameterizations of PP and KPP. You might also want to be specific that you are talking about the vertical diffusivity in the ocean interior away from the surface and bottom boundaries.

L151-153 and in Fig 3: The difference of SSS in the Arctic appears substantial (especially between panels a, b and c, d). You might want to elaborate more on the possible causes. For example, how the differences among the four schemes lead to the significantly different SSS. Does the simulated sea ice change a lot?

L153: I'm confused – Isn't the vertical mixing scheme the only difference among the four simulations? What do you mean by increased river inflow?

L168-170: It might just be due to the different vertical distribution of the salinity resulting from different vertical mixing. Does the horizontal flow change a lot among the four simulations, in order to support your hypothesis of stronger inflow of saline water from the Indian Ocean?

L181: What do you mean by "above named currents and water masses"?

L183-188: Again, you might want to support your hypotheses by more clear reasoning of how changing the vertical mixing results in the differences in, e.g., the inflow from the

Indian Ocean, and the overflow water at 60N and the MOW, thereby the temperature distribution.

L190: Perhaps "the next sections" -> "this section"?

L199: "background mixing value" -> "background eddy diffusivity and viscosity"?

L210-216: Comments on how these features are simulated in the four simulations? What are the conclusions from Fig. 7?

L222, 224: Fig 11 and Fig 12 are used before Fig 9 and Fig 10 without introduction?

L229-243: Again, how are these features simulated in the four simulations here? This review seems to be separated from the analysis of the simulation results.

L262: What do you mean by "close to the limit of measurement"?

L264-265: You mean the cell averaged wind stress? Is the ice stress accounted for here?

L285-286: Does this result in, say, more realistic sea ice results?

A question to most of the maps at a certain depth: how was the depth chosen for each map?

L317: Why not using the same density threshold?

L324-325: Do you mean comparing with HR_{tke} here? How is the internal waves affecting the surface mixed layer?? Is it due to more frequent convection as a result of a stronger interior mixing (due to internal waves) and thus weaker stratification?

L337: The disappearance of the bottom cell is pronounced...

L340-341: Would you be more specific about this statement? I can see quite some differences between HR_{tke} and HR_{kpp} in many of your figures, e.g., MLD and AMOC

L408-409: Rephrase? Do you mean the difference between the three are small?

L410: Delete "but"?

L486 and Eq(A6): Why a change of notation here (removing the prime)?

L498: 0.7 is significantly bigger than the suggestion of 0.3 in Large et al., 1994 and many implementations of KPP via CVMix in other models. Any comments on why this value is used?

Appendix A2: Since the KPP scheme is well documented in the literature, especially in the CVMix documentation, I would suggest the authors to consider the necessity of showing all the details here. I think it would be much cleaner for the reader if you only briefly introduce KPP and highlight the different configurations than the default in CVMix when implemented in MPI-ESM1.2.

L534-537: What are the values for these parameters that are used in this study?

L564: Combine with the previous sentence and delete "When TKE is used alone without being coupled to IDEMIX"?

References:

Danabasoglu, G., Large, W. G., Tribbia, J. J., Gent, P. R., Briegleb, B. P., & McWilliams, J. C. (2006). Diurnal coupling in the tropical oceans of CCSM3. Journal of Climate, 19(11), 2347–2365. https://doi.org/10.1175/JCLI3739.1

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-202, 2020.