

Interactive comment on “Impact of horizontal resolution on global ocean-sea-ice model simulations based on the experimental protocols of the Ocean Model Intercomparison Project phase 2 (OMIP-2)” by Eric P. Chassignet et al.

Anonymous Referee #2

Received and published: 20 April 2020

General Comments: This is an important paper for the ocean and climate modelling community feeding into wider decisions on the benefits of high resolution ocean components to address climate science questions. The conclusion that the impact of resolution is consistent across model families in terms of circulation but model dependent in terms of temperature and salinity biases is an important one. My main comment is that the high and low resolution versions of the models are not always comparable due to differences in vertical as well as horizontal resolution. It would have been preferable to have changed only the horizontal resolution and associated physics so that the effects

C1

of horizontal resolution can be isolated as stated in the abstract. I would suggest that at very least, this issue should be addressed in the model description and discussion in the context of Stewart et al. (Ocean Modelling, 2017) and included as a future recommendation. I would recommend this paper for publication assuming that this and other specific issues below are addressed.

Specific comments:

L21: See discussion above on isolating the effects of horizontal resolution

L86: Title would be better as Model Descriptions

L93: A summary table of the four models at both resolutions would be useful for the reader

L117: Why is FSU model 41 layers in low res and 36 layers in high res? (see above)

L157: What is the impact in the NCAR model of two additional levels as well as partial step formulation?

L207: I couldn't see any mention in the description of the lack of salt flux normalisation in IAP?

L213: What is the impact of changing from 30 to 55 levels in the high resolution? It seems to me that this is more consistent with resolving baroclinic modes at high resolution (Stewart et al., 2017) but 55 levels is probably insufficient to resolve the first baroclinic mode.

L229: Title would be better as Temporal evolution and model drift

L249: Is there any comment on why the global average KE is so different between NCAR and IAP (which should be fairly comparable in terms of resolution)?

L258: Add this discussion of the salt flux normalization to mode description and specifically in description of IAP

C2

L275: Please correct 'all high resolution models warm faster over the upper 2000m and global temperature than their lower-resolution partners'. This doesn't appear to be true for FSU.

L284: I understand that including this is beyond the scope of this paper, but the authors could recommend and pass to the discussion, that the vertical heat and salt budgets are compared in future as in Griffies et al. (J. Climate, 2015) and Von Storch et al. (Ocean Modelling, 2016)

L335: Is it possible to hypothesise what leads to reductions or increases in the biases and the different model behaviour?

L417: is the similarity of the high resolution model and RAPID AMOC evolutions statistically significant or can it be shown to link with reduced deep convection? Are the convection patterns different across resolutions? I think without further discussion, this point is rather speculative.

L445: Should figure 14c be labelled as AABW cell?

L502: Since the temperature and salinity fields at depth are still evolving at the end of these experiments, it seems more likely that the ACC changes are related to buoyancy drifts. Can you comment on this?

L528: Also comment here about the choice of vertical levels affecting the representation of baroclinic modes (Stewart et al., 2017)

L539: I would argue from figure 16, that there is at least marginal improvement in the representation of the NW corner across the models but it is difficult to see in the multi panle figure

L555: Please comment on Agulhas eddies in other models

L710: Can you comment on the cause of the increased northward heat transport in the Atlantic by looking at the components? Presumably, it is related to a stronger Gulf

C3

Stream transport

L834: Reference should be Griffies et al. (2015) see below. Include here recommendation on diagnosing vertical heat and salt budgets in future experiments?

L848: I think specific comment is merited here on the aim to maintain traceability across resolution hierarchies in terms of numerics and parameterisations as well as vertical resolution

Technical corrections:

L106 (and similar throughout): present as 3×10^{-5}

L414: extra 'only' in the sentence

L505: The sentence doesn't make sense as currently written

References:

Griffies, S. M., Winton, M., Anderson, W. G., Benson, R., Delworth, T. L., Dufour, C. O., Dunne, J. P., Goddard, P., Morrison, A. K., Rosati, A., Wittenberg, A. T., Yin, J. J. and Zhang, R., 2015. Impacts on Ocean Heat from Transient Mesoscale Eddies in a Hierarchy of Climate Models, *J. Clim.*, 28, 952-977.

Stewart, K.D., Hogg, A. McC., Griffies, S. M., Heerdegen, A. P., Ward, M. L., Spence, P., England, M. H., 2017. Vertical resolution of baroclinic modes in global ocean models, *Ocean Modell.*, 113, 50-65, doi:10.1016/j.ocemod.2017.03.012.

von Storch, J.-S., Haak, H., Hertwig, E., Fast, I., 2016. Vertical heat and salt fluxes due to resolved and parameterised meso-scale eddies, *Ocean Modell.*, 108, 1-19, doi:10.1016/j.ocemod.2016.10.001.