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Interactive comment on "Towards Multiscale Modeling of Ocean Surface Turbulent Mixing Using Coupled MPAS-Ocean v6.3 and PALM v5.0" *by* Qing Li and Luke Van Roekel

Anonymous Referee #1

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Review of "Towards Multiscale Modeling of Ocean Surface Turbulent Mixing Uxing Coupled MPAS-Ocean v6.3 and PALM v5.0" submitted to Geoscientific Model Development

This paper discusses and presents some preliminary results from efforts to couple a hydrostatic hydrodynamic ocean model (MPAS, the parent model) to a nonhydrostatic horizontally-periodic large eddy simulation (LES) ocean model (PALM, the child model). The PALM model is ported to GPU, which results in a ∼10x speedup relative to a single CPU. And, a couple simple test cases are presented.

I find that this paper is a solid and significant contribution to GMD; the approach is novel and has potential value for ocean and climate modeling. However, the test case

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results are of limited scientific value; they simply provide preliminary evidence that the implementation is likely correct or close to correct. The methods generally seem reasonable, and the results seem like they flow from the methods. However, the chosen test cases and the methods seem ad hoc and preliminary. In particular, it seems plausible that the approach chosen here is a suboptimal first step, rather than a polished model. I'm particularly concerned about the coupling via nudging and running in coupled mode with different vertical mixing schemes at adjacent grid points; these issues seem scientifically problematic if not technically problematic. The paper is also well written and there are only a few typos, and the software is evidently public, so it is likely to be reproducible, but I did not try to reproduce the results myself. It would be hard to reproduce from the manuscript alone (without the code), but that seems ok to me. Overall, I think this paper is a useful contribution to the literature and should be published. Some specific comments are below.

Specific comments: It is hard for me to interpret the practical implications of porting LES to GPU in the context of existing system architectures. The small test cases presented here don't really highlight the advantages of the approach at scale, i.e. the examples are all cheap/small calculations. The implementation is also not exactly clear. Does each LES grid point use 1 CPU and 1 GPU? Is the application limited by the number of GPUs on a typical node?

There seems to be too much scientific background material and the motivation is confusing. For example, there is much ado about "superparameterization," but the suggestion is that using this coupled model with LES in each grid cell as a superparameterization is likely to be infeasible (e.g. L70). I don't think that this general suggestion can be accepted or rejected based on the benchmarking and small number of examples and results in the paper. However, I agree that the practical value of such a model is its ability to provide LES dynamics in the context of larger-scale ocean dynamics, as the authors suggest. I think the paper should focus on this latter motivation, and on one-way MPAS->LES coupling. Reduce some of the discussion of superparameterization

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and two-way coupling to make the manuscript more focused.

The coupling between parent and child is ad hoc. For example, MPAS -> LES mean profile is a nudging with a timescale of 30 min for tracers and 5 hours for momentum. There is some discussion of sensitivity to the nudging timescale, but there are alternative approaches that are not explored. For example, it may better to apply the dynamical tendencies from MPAS to the LES mean profiles (excluding vertical mixing terms), rather than or in addition to nudging the mean profiles to the parent model. In addition, the omission of mean lateral gradients in the LES significantly modifies the turbulence energetics where gradients are strong (e.g. at fronts) in the second example (e.g. Bachman et al. 2017). These explicit lateral gradient effects are missed with the nudging (and the forcing by large scale tendencies).

Bachman, S. D., Fox-Kemper, B., Taylor, J. R., & Thomas, L. N. (2017). Parameterization of frontal symmetric instabilities. I: Theory for resolved fronts. Ocean Modelling, 109, 72-95

The inconsistency between adjacent grid points (some points with KPP and some points with LES) makes the two-way coupling too problematic to use without further exploration. I think it would be better to start by exploring science questions with only one-way parent-to-child coupling to avoid this problem as a starting point. That is, use KPP at all points to advance the parent model. See my first specific point above about the background on superparameterization.

Please clarify how surface fluxes calculated in the two models. Are there feedbacks between ocean state and atmosphere, as in bulk flux algorithms, that lead to inconsistencies in the fluxes between grid points with KPP and grid points with LES?

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