Response to Reviewers

https://egusphere.copernicus.org/preprints/2023/egusphere-2023-1657/egusphere-2023-1657.pdf

We sincerely thank the reviewers for making helpful suggestions. The changes we will make in response will clearly improve the paper. We have considered all suggestions, and in most cases will make any suggested changes. The only exceptions are changes that we could not implement due to some of the simulation data being no longer accessible or changes that would make the paper less amenable to a particular audience, but none of these affect fundamental aspects of the work. You have our sincere appreciation for your invaluable contribution as a reviewer for the paper titled "Comparison of the Coastal and Regional Ocean Community Model (CROCO) and NCAR-LES in Non-hydrostatic Simulations" submitted to *Geoscientific Model Development*.

Your thoughtful and thorough review played a crucial role in enhancing the overall quality of the manuscript. Your constructive feedback and insightful comments significantly contributed to the refinement of the research, ultimately ensuring the paper's academic rigor and relevance. The time and expertise you devoted to evaluating the paper are deeply appreciated. Your commitment to maintaining the high standards of scholarly publishing is instrumental in advancing the field and fostering a culture of excellence.

Red comments below indicate places where the text is to be altered in a revised version of the paper. Other improvements in writing style were made at the same time. A tracked changes version of the paper will be made available on request. Discussion and commentary in response to reviewer comments is in black. Reviewer comments themselves are in blue.

RC2: ['Comment on egusphere-2023-1657'](), Anonymous Referee #2, 15 Jan 2024

Review: "Comparison of the Coastal and Regional Ocean Community Model (CROCO) and NCAR-LES in Non-hydrostatic Simulations"

Thank you for a very interesting manuscript.

Summary

The paper is mainly technical, comparing different LES codes and CROCO from a result and computational performance perspective. It can be seen that the results are quite similar but also that the models are tunable. Are the authors using a standard tuning or have they tuned the models during the study and then present the results for the runs with the closest results and the settings during these runs? The paper would improve if it was easier to see what is a standard setup and what has been tuned in order to get similar results.

The study would also have gained confidence if the results were compared with observations if available or chosen a test case where observational data is available.

The study uses different scaling in the comparison's plots. There are, however, different ways of scaling these runs and the scaling as presented make the analysis rather more difficult than easier to interpret.

Reply:

We use standard tunings for all models, as we explain on Section 2.1.1. In fact, many of the difficulties in arriving at a clean comparison, especially of the relative computational costs, involved retuning parameters to see if they made a significant difference in cost without degrading accuracy.

Unfortunately, as these are idealized forcing cases, there are not meaningful observations to compare against. Instead, we try to restrict our simulations to where the Monin-Obukhov

Similarity Theory (MOST) applies, as it has been found successful in explaining both simulations and observations.

The scalings used to remove the dimensions from the axes in some plots (e.g Figures 2 and 3) are standard approaches for the boundary layer turbulence literature (i.e., MOST). As those figures refer to topics discussed in this literature, we prefer to keep that scaling and to reach that audience of readers. Since the forcing is constant (u*, w*), most of these scaling factors are also just constants except for the boundary layer depth and buoyancy frequency which sometimes vary importantly among the models.

Minor issues

Line 1-15. The abstract would gain of being more precise and using numbers of acceptable, reasonable, or negligible deviation and additional cost of running CROCO compared to NCAR-LES.

Reply: We have added some quantification of cost and accuracy to the abstract. It now reads:

Largely due to the compressible fluid equations it solves, this version of CROCO is found to require six to fourteen times shorter timesteps than NCAR-LES, depending on forcing, and between ½ and 2x the cost per timestep depending on how many barotropic subcycles per baroclinic timestep are used.

Line 5 I assume that "code base" is used instead of model since a model can also include a specific set-up, which is good. However, it is easier to rather change "code base" to just "code".

Reply: We have removed this phrase.

Reply:

The discussion on Line 188 and near Eq. 7 & Line 155-167 address the fact that entrainment rate is very different between these models, which explains most of the differences in this comparison. The discussion of the different numerical schemes in the vertical advection equation is intended to address the reasons why this entrainment differs.

Reply: Figures 2 and 3 show the mean and variance of the velocities.

Reply: This has been corrected to "closest".

Reply: Unfortunately, these numerical simulations are for idealized forcing settings for which there is no observational or experimental validation. The use of simulations where MOST applies is the closest we can get to having objective "truth" to compare against. Mainly, we just have to be content with measuring the difference between the models without knowing which is most accurate.

Reply: We added the following text to the discussion of these figures: The deviations at low wavenumber are due to the integral constraints of <w>=0 and buoyancy anomaly over the whole domain being linked to vertical fluxes. Thus, the small-scale deviations and large scale deviations are linked. In u' and v', there are not meaningful large-scale deviations.

Reply: We have redrawn these figures to look more similar to other figures.

Reply: We ended up rerunning the Oceananigans calculation to regenerate figure 14. That model is under intensive development, and now the gradient of v'^2 seems to be consistent among the three LESs near the surface. We were using an older version of Oceananigans in the previous version of these figures. The new figures are from Oceanaingans v0.83.0. We now include the version of Oceananigans in the paper as it has been under intensive development. We suspect that the boundary condition implementation has been revised during the review of our paper. Note that Oceananigans is not the focus of this paper, CROCO is, so we do not feel the need to go deeper into these questions in the text.

We now add the following: In reproducing Li and Fox-Kemper (2017) with both NCAR LES and CROCO, there were notable differences. However, in that comparison many parameters differed between the models (e.g., stretched vertical grid, subgrid model) in addition to the numerics. Hence, a more detailed comparison where gridding was more tightly matched and subgrid schemes were explored was carried out (preceding subsections in Section 2). In this final subsection, a comparison between CROCO and NCAR LES in more typical configurations (where they are not matched in gridding and subgrid schemes) are shown to illustrate discrepancies under more realistic configurations.

We have rephrased:

When fewer processors per node are used, most systems still typically charge for the unused processors on each node so this is not more efficient overall, just more efficient per processor in use.

Reply:

We now use "NCAR-LES" consistently throughout the paper in most sections including Section 4, and "LES" for NCAR, Palm or Oceananigans in that section.

Reply:

Fixed. We replace the second sentence with: "The study begins with a comparison of several different LES versions and then because of their close agreement only NCAR-LES is used elsewhere."

Reply: We altered this sentence to be clearer:

Once these parameters are considered, the NCAR-LES results and the CROCO results are overall within expected variations.

Reply: We corrected this sentence to:

A rough comparison between CROCO on a stretched vertical grid and NCAR-LES on a uniform grid finds that the stretched grid does not significantly magnify the model discrepancies in this setting.

Reply: Changed "–easily by a factor of 4 or more" to "by a factor of 4 or more, but it ranged from 2 times to half as expensive as NCAR-LES per time step using a sound speed with accurate results depending on the amount of barotropic subcycling.". It is not easy to express this as a function of fast cycle time step as the model cost is a combination of baroclinic time steps taken and barotropic time steps taken with a very different weighting factor for each. In general, due to

the comparative complexity of the CROCO numerics vs. any of the LES, it is hard to be precise about the costs in a meaningful and fixed way.

Line 454-458 "Optimizations continue:…" Although interesting this comes abrupt in the summary since it has not really been discussed earlier in the paper.

Reply:

Yes, the authors in our group who are working on this effort at present were eager to include this phrase. We now add "which will be documented in future publications" to that sentence to clarify, and rewrote the sentence to lay out these plans and ongoing work more clearly. As this is future work, we think it should only be mentioned in the conclusions section and not elsewhere in the paper as is standard practice.