

Interactive comment on “3D dynamics of the Southeastern North Sea, effects of variable resolution” by Ivan Kuznetsov et al.

Anonymous Referee #2

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Review of “3D dynamics of the Southeastern North Sea, effects of variable resolution” by Kuznetsov et al.

The authors describe in their manuscript the application of an unstructured grid model (FESOM-C) to reproduce the baroclinic dynamics in the southern North Sea. For the base setup, they use a resolution varying from 4-1 km. The authors do a tough validation with gauge data, cruise data, fixed stations, glider and ferry box data. After they concluded that the model is able to reproduce the baroclinic fields, they tried to explore the effects of variable resolution. The testcase consist of a batrotropic tide. To check for solution convergence, they use 2 additional refined grids.

I do like the basic idea of the paper and the promise hidden in the title. Anyhow, I recommend major revisions, although rejection would also fit.

At present, the paper is out of balance. Roughly 90

An interesting extension of the paper would be to plot for individual stations (offshore, onshore, estuaries) the runtime (or local grid resolution) vs. the error/rmse. This should immediately show how sensitive different regions are to changes in the grid size. This would also provide some clues on the needed grid resolution in the estuaries/inlets and offshore. This would also give a hint on the efficiency for different grids.

Throughout the manuscript, the authors blame the coarse grid resolution in m8 for lacking performance in the validation of the baroclinic fields. In the conclusion they state that the model is pretty fast and scales well. Why not than simply repeat the baroclinic runs with m5 and m3, and do a true converge analysis for the 3D fields. To repeat the computation for m5 and m8 with two additional tracer fields (T/S) should not be that expensive! This would also help to answer the question, if it is really the grid resolution or is the model still “too diffusive”. And please don’t blame the limitations in disk space for having trouble with the data analysis.

If I look on the glider data, stratification is clearly to weak in the model. Is it turbulence closure issue (I even do not know which closure the authors use), is it a boundary issue, or is it an interplay between lacking horizontal and vertical performance?

You have such a nice validation data set, especially the ferry box data! Why not explore these data in detail? Why not do a convergence analysis for the region Helgoland-Büsum/Cuxhaven? Here one could study the effect of grid resolution on frontal dynamics in the Elbe plume. Moreover, one could do a similar analysis for offshore waters on the Immingham track. In short: do some science (and cut the lengthy validation, even it is a tough one).

Some technical remarks: Section 2.3. You state that you used a spinup of 1 year. Thus, you started the model run in 2009, throw away 2009 and used than only 2010-2014. Right? Section 2.4 You explain lengthy that a 5 day mean for boundary conditions is a good trade off between available data and accuracy. Two sentences later, you

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state that you used monthly mean data (from a reconstruction) . Based on your above statement, that is a critical issue in that highly dynamic region!

As final remark: the authors state that the computations were done on 24 cores and this proves that the model is fast. I strongly believe that this is a poor measure. More interesting and more valuable are the needed cpu-hours per simulation year. That would help others to compare their needed resources (and model errors), to your results.

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