Interactive comment on "AMM15: A new high resolution NEMO configuration for operational simulation of the European North West Shelf" by Jennifer A. Graham et al.

Summary:

The paper presents results from an old and new setup of NEMO, covering the Northwest shelf region (NWS). It includes a model inter-comparison and validation against observations. The new model configuration will replace the previous setup in the operational model setup at the UK MetOffice. The paper focuses on results based on seasonal and yearly calculations. The results are well presented and it clearly illustrates that the new high resolution setup delivers better salinity and temperature results on seasonal to climatic time scales. Furthermore, tidal signal is also better described in the new setup. Model developments and validation are too rarely presented in journals, and it is very interesting to read model development at operational centers. The manuscript discusses processes near the shelf edge, which could be benefitted by the scientific community. Throughout the manuscript results from three regions are presented: 1) Outer shelf, shelf and Norwegian trench. It would be nice if the results were presented in this order for all sections. Sometimes the Norwegian section comes second and sometimes it comes last.

Central issues

The authors should present which operational products are produced based on results from the operational model. If products are related to storm surge events, search and rescue or other weather related issues, then this manuscript lacks validation on these phenomenons. It could be analyzing peak error on sea level or presenting results from a storm surge event.

For an operational model, the results on time scales from hours to weeks are also important, especially on sea level. Excluding sea level variations from the papers makes it impossible to know if the barotropic transports are sufficiently well simulated, especially if (as the author states) S/T-climate are mainly governed by vertical processes. Still, the (non-tidal) barotropic signal has effect on the state of the North Sea on both short and longer time scales.

One main conclusion presented in Section 4 is:
Abstract: "Since there has been no change to the vertical resolution or parameterization schemes, performance improvements are not expected in regions where stratification is dominated by vertical processes, rather than advection."

It seems like that this conclusion is based on pp13, line 2-6, thus rather short for being such a central parts in conclusions/abstract and I believe it deserves more attention in Section 3

Minor comments

pp 4, line 28. Why is the minimum depth spec. to 10m? Would it not be better to set minimum depth to much less, maybe 3m, and locally increase the depth to at least 10m in Bristol Channel and Gulf of St. Malo? Considor to include a comment on this in the manuscript.

pp5, line 18. Method for Tidal forcing is only valid for AMM15. Method for Tidal forcing for AMM7 is described on pp6 line 16. Merge these two sections into one, preferably at pp.5.

pp 6, line 22-23. There are shifts in the position of two amphidromes. Please comment if that is good or bad.

pp. 6, line 32. Add English to "Channel". Not all may come to think of English Channel when just writing Channel.

Caption to Figure 2 and Table 1. Refer to data source in text also (only in fig caption is not enough). To my knowledge, non-British tide gauge data (e.g. Danish, Norwegian, and German) is not available at BODC.

Figure 2. Label and add units to the vertical bars to the right of panels. I do not see the meaning of the phase-bar. For example, it is not possible to differ between phase=80 and phase=240.

Comment to pp 8, line 6. Tidal signal downstream of shallow regions will also be affected by too deep minimum depths. Please comment on downstream consequences.

pp9, line 12, Are OSTIA observations skin or bulk temperature? Are there any problems validating this observed temperature to a 10m thick model surface layer level? Please comment on that, especially for regions that have seasons with expected shallow surface layers. Maybe some regions do have thicker than 10m for all seasons, others not. I think addressing this will help the reader to interpret the validation better (also related to next three comments).

pp 10, line 28-31. Thermal inertia in model during summer that causes a delay in summer heating, and a delay in cooling during early fall (when warm surface layer is being developed and maintained) is , to at least some extent, explained by a too thick surface layer (=10m).

pp10, line 25-35. Hypothesis that too warm SST during spring due to too shallow mixed layer depth could be verified by producing a MAM and SON figure similar to Fig6a. Consider to include that analysis.

pp12, line 33. Too cold surface water may be explained by the 10 m thick surface layer. This could be investigated by computing the evolution of a single column model with different vertical resolutions, but with same forcing and initial conditions, but this is just a suggestion.

Fig. 8 and text pp. 17, line: 5-15: These two paragraphs do not add much information to the study; more than that AMM7 has larger interannual variability. But it cannot be verified which one of these setups does the best job. The observations referred to, cover twice the simulation period and are presented rather vaguely with only sign of trend (positive) but no rate of change or mean value for the period. If there is a trend, then stability in AMM15 is not reassuring. It should drift. Furthermore, if AMM15 simulates the long term mean value rather good, the decrease in AMM17 until early 2000s may be a good thing, and maybe the increase after 2003 could just as well agree very well with the observed long term trend. My conclusions based on your results may be considered rather speculative, but that is exactly my point. From the interannual salinity data presented in this paper, the authors' results are speculative. More results are needed, or the paragraph should be rewritten or deleted.

Title of section 4 is "Discussion and Conclusions": I do not detect so many conclusions, more discussion and future work. It would be nice to highlight conclusions further in text or add some conclusions drawn from the paper; alternatively rename Section 4 to "Disccusions and Future work". The only obvious conclusion to me is:

Section 4: pp 18, line 9-11 "Given the fact that climate on the shelf can be predominantly driven by a balance of vertical forces (surface buoyancy fluxes and vertical mixing) rather than horizontal advection, it is not surprising that the two models are similar. Both have the same atmospheric forcing, vertical mixing schemes and vertical resolution."
This is very interesting and I think it should be more clearly presented in Section 3. Vertical processes may govern the salinity/temperature climate in the North Sea, but advection may very well a dominating factor during storm surge events.

Section 4: There are no conclusions and/or discussions from tidal section. In fact, it took me a while to realize that it is only baroclinic features are discussed. Please add a paragraph of the tidal-results presented in 3.1. It could be something like: Tidal signal within a model setup covering the North Sea is to a large extent determined by the boundary conditions and bathymetry. AMM15 and AMM7 have different bathymetry and tidal forcing at the open boundary. It seems like this step is larger than comparing two different models (e.g. NEMO and ROMS) with same bathymetry and tidal forcing (and advections schemes). To me, the tidal part is not considered to be an update (from AMM15 to AMM7), but a replacement to a new setup. Feel free to add something like this, or choosing some other angle of the tidal-results presented in Section 3.1.