

Interactive comment on “Mean circulation in the coastal ocean off northeastern North America from a regional-scale ocean model” by K. Chen and R. He

Anonymous Referee #2

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This paper describes a model implementation for the Mid-Atlantic Bight and Gulf of Maine for the period 2004–2013 with a partial skill assessment and some basic evaluation of the average conditions (transport, sea level slope, momentum balance). The authors present the results of a regional model simulation for the coastal ocean with approximately the same horizontal resolution as their large-scale forcing. Little justification of the benefit of the approach is given. The presented skill assessment is only partially useful and a more complete, extensive, and quantitative assessment is suggested. However, the paper provides a lot of interesting results that could help understand the dominant processes in the coastal system in the region.

Major points:

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What are the benefits of running the ROMS simulation if the HYCOM model results are of approximately the same horizontal resolution (8–10 km)? The authors talk of downscaling, when in reality both grids are of similar resolution. The authors show the salinity differences, but if the bias is known and corrected in a similar approach to the one followed to generate their boundary condition, why run a system at all? A regional simulation is usually set up at a finer horizontal resolution than the global/basin scale solution used as forcing. To the untrained eye, it might seem as a waste of computer time. Please provide adequate comparisons with observations also for the HYCOM fields that show the advantage of using the regional system. It might be that the HYCOM solution is sufficient to estimate the fluxes described in the study.

In the future, the modeling system will definitely benefit from a finer regional resolution to capture smaller scale processes that dominate the exchange in many parts of the domain (e.g., frontal dynamics).

Using Hydrobase as the ground-truth seems odd. Assuming that the long-term averages provided by Hydrobase that include data since the beginning of oceanographic data collection are true for the period 2003–2014 seems like a stretch. Both temperature and salinity conditions are likely rapidly changing (IPCC AR-5 provides a lot of information in this aspect) and therefore the dynamic height is likely different during recent years. At least HYCOM uses NCODA to assimilate recent available temperature and salinity information.

Why apply a thermal relaxation and not a similar salinity relaxation? It seems that this could lead to inconsistencies in the surface density field.

The choice of skill assessment stations is questionable. The way it is performed it seems like the authors pick stations that resulted in good agreements while avoiding other relevant stations. There are several other NERACOOS stations in the GoM, several NDBC buoys that include SST and in some cases subsurface information, many more water level stations along the coast. A full skill assessment is encouraged. While

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correlation coefficients and bias estimates are useful, the literature is full of better skill metrics (rms differences, skill scores, Taylor diagrams). The skill assessment needs to be more quantitative than what is presented. At the least, a table with the differences between model and observations at all available locations needs to be added. I also encourage the authors to include the HYCOM results to highlight the benefits of their approach. The comparison with water levels should include a tidal analysis.

While the authors claim that the subsurface comparison is good, the temperature and salinity time series exhibit significant differences especially during the summer. This result suggests the mixing dynamics and stratification are at least deficient. An example is the lack of a meaningful seasonal cycle in subsurface salinity in the model solutions. The complete skill assessment described above will highlight any other deficiencies.

The mixed layer depth discussion for the entire simulated period seems of little use over the shelf. I understand the usefulness for open-ocean dynamics, but the strong seasonal variability over the shelf makes the average value almost meaningless. I encourage the authors to refocus this discussion on the seasonal changes in MLD over the shelf as the focus of the study is the "coastal ocean".

The momentum balance in the cross-shelf as a depth-averaged estimate is not as useful as a two-layer estimate. The fluxes from the surface will often be at least partially compensated by bottom fluxes. Please use Lentz et al. (2001) approach for a more meaningful discussion of cross-shelf exchanges.

The mean transport results are quite useful and one of the main results of the paper. However, the discussion of the cross-shelf transport being dominated by eddies leading to enhanced variability should be revisited. How much of it is eddy activity and how much is the fact that the cross-shelf transport is inherently two-layer? The authors have all the pieces in place to answer this question.

Isn't the mean sea level slope basically the result of the average temperature and salinity conditions? If this is the case, then your results are by definition the same

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as the slope in dynamic height from Hydrobase. What does the model add to the climatological estimate? How different is it from other estimates that are not forced to match the climatology? Is the mean sea level slope (and as a consequence the transport) changing in time?

Minor comments: References in the abstract need to be completely spelled out, as the abstract needs to be understood even without the rest of the paper. Modify the reference Lentz (2008a) accordingly or remove it, as it does not seem to add anything to the abstract.

Pg 2756, Line 9: "Good agreement with observations", please quantify.

Pg 2756, Line 14: "at Scotian Shelf", should read "over the Scotian Shelf".

Pg 2771, Line 5: "MABGOM model model simulations", should read "MABGOM model simulations".

Please include the chosen model configuration formulations. The vertical mixing scheme is Mellor-Yamada, but what are the horizontal mixing scheme and the values of the constants used? What are the chosen parameter values for the bottom friction formulation?

Most authors these days prefer the term "skill assessment" rather than "validation" to avoid confusion.

Spell out the names of the momentum terms in Figure 12 in the Caption.

Interactive comment on Ocean Sci. Discuss., 11, 2755, 2014.

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