Please find our responses (in blue) below to the reviewers' comments (in black). Corresponding changes in the manuscript are also included with page/line numbers (in the revised manuscript) shown.

Responses to Reviewer 1:

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

J. Manning (Referee)
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I was very happy to read about this work. As an oceanographer at the Northeast Fisheries Science Center, I am glad that there are now multiple modelers generating multi-year hindcasts on the entire NE shelf. While the work described in this paper is fairly basic (primarily a look at climatologic means and validation), it is their first step in addressing the more important aspects of inter-annual variability. Now that they have described the basic workings of the nested models, the authors will need to take it a step further in order to make the hindcast useful. It is obvious they will need to reduce the grid sizes in the next round of runs and perhaps they will use an alternative parent model of their own (other than HYCOM).

Since most of the action on the NE shelf occurs at the shelfedge and the bulk of the transport occurs in a relative narrow jet near steep topography, the authors I'm sure are aware that the 6-10 km grid cells are probably not adequate to resolve the fronts in these areas. This paper speaks broadly about the shelf system but, in the future, it would be nice if the modelers could focus in on some of the canyons and be able to quantify their contribution to the overall import/export of shelf waters. This will require, I imagine, some higher resolution model grids.

The biologists at our lab are interested in describing and attributing the year-to-year changes in larval recruitment and survival to the variability in transport. Can the number of young cod and haddock on Georges Bank, for example, in any one year be explained by the degree of retention or loss from the bank due to pure physical processes? We certainly do not have enough observations anywhere on the shelf to be able to quantify these processes. We do need observations however for both assimilation and validation purposes. We need these models and it is best to have multiple models.

In some places, there were detailed validations but much more will be needed in the future.

For example, the time series comparison of temperature and salinity were plotted in Figures 5 and 6 for a single location that is certainly not representative of the entire shelf. The authors note that "the model generally tracks the subsurface temperature series" but, from what I can see from Figure 5, there is a 2 to 3 degC discrepancy during the stratified season. It is not clear to me what, if any, hydrographic data was assimilated in the child model. I imagine some (or much more) will need to be included in the next set of hindcast runs. It is clear that the parent HYCOM assimilates some T/S.

I was happy to read that there is an archive of state variables but it would be nice, in the future, if the authors could point to these archives (assuming these fields are accessible). In the spirit of open access for all, it would benefit the authors if other investigators could query the data, do some validations of their own, and they could share their results.

We thank the reviewer for the positive comments.

Indeed, the work described in this paper is the first step in addressing more important processes including inter-annual variability of the circulation and ecosystem dynamics in the Northeast U.S. coastal ocean. As the reviewer pointed out, the spatial resolution of our MABGOM model is not much higher than that of the global HYCOM. However, our goal for MABGOM is to produce a more accurate model representation of regional circulation by adding more complete coastal dynamics (i.e., river discharge and tidal forcing, which are absent in the global model) to the regional model and correcting biases in the global model. Such a regional model can then provide dynamically consistent open boundary conditions (OBCs) for higher resolution nested models to investigate more specific research questions in each sub-regions. For example, the MABGOM model had provided OBCs for a Gulf of Maine coupled biophysical model (He et al., 2008; Li et al., 2009) and a high-resolution circulation model focusing on the shelfbreak processes in the Middle Atlantic Bight (Chen and He, 2010).

Corresponding revisions in the text are made:

Page 16, line 485-493: "Our goal for MABGOM is to produce a more accurate model representation of regional circulation by adding more complete coastal dynamics (i.e., river discharge and tidal forcing, which are absent in the global model) to the regional model and correcting biases in the global model. Such a regional model can then provide dynamically consistent open boundary conditions (OBCs) for higher resolution nested models to investigate more specific research questions in each sub-regions. For example, the MABGOM model had provided OBCs for a Gulf of Maine coupled biophysical model (He et al., 2008; Li et al., 2009) and a high-resolution circulation model focusing on the shelfbreak processes in the Middle Atlantic Bight (Chen and He, 2010)."

We had compared the model hindcast solutions against other NERACOOS moorings (http://neracoos.org/realtime_map) in the Gulf of Maine, and found the comparisons are comparable to those shown in Figure 5 and 6. The following figures (Fig A1, A2) show the comparison made at NERACOOS mooring E.

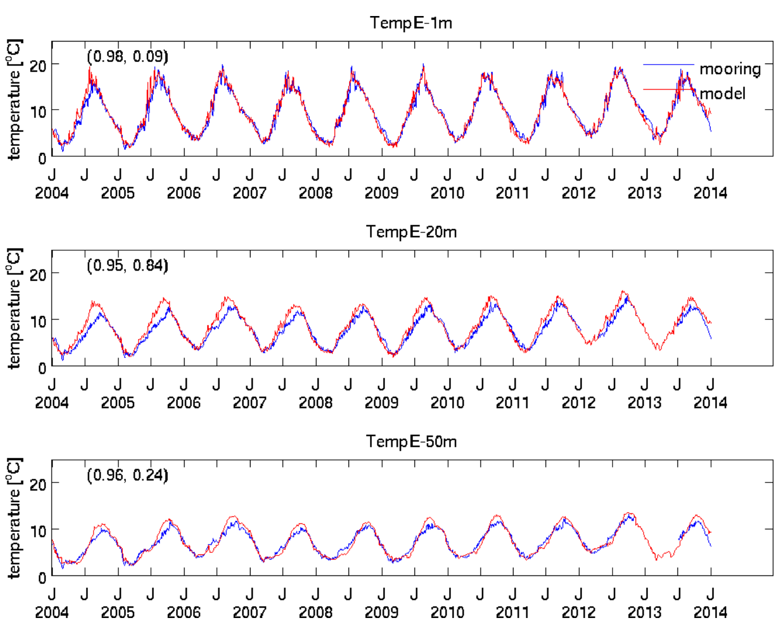


Figure A1. Comparison of observed (mooring) and simulated temperature time series at NERACOOS mooring E. Linear correlation coefficients (r) and mean bias (b) are shown in parentheses (r, b) respectively in each panel.

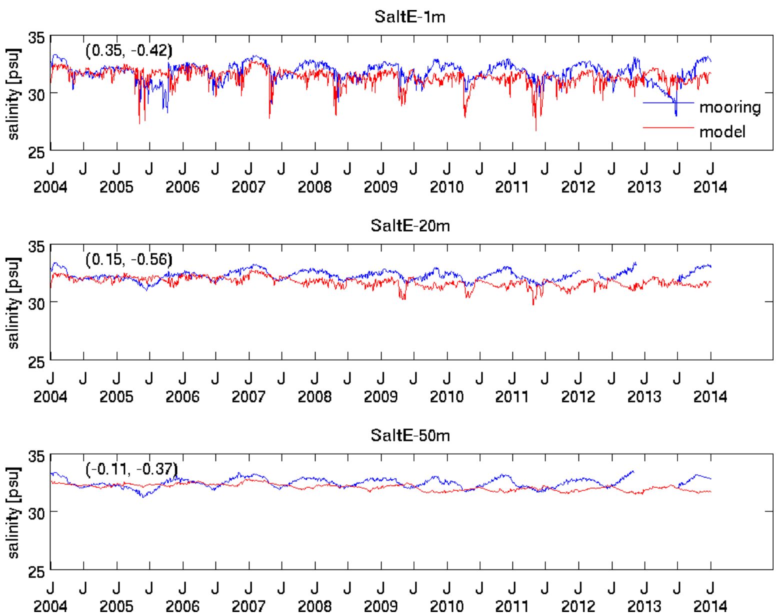


Figure A2. Comparison of observed (mooring) and simulated salinity time series at NERACOOS mooring E. Linear correlation coefficients (r) and mean bias (b) are shown in parentheses (r, b) respectively in each panel.

Corresponding changes in the text are made:
Page 9, line 253-255: "We have compared the model solutions against temperature and salinity recorded at multiple buoys. Figure 5 and 6 for example show the results at NERACOOS buoy B, which is located in the western GoM. Temperature comparisons between …"

Several sources may contribute to the larger misfit in model simulated subsurface temperature field. These include the model's spatial resolution, the sensitivity of turbulence closure schemes (we used Mellow-Yamada scheme in this study), and water mass biases inherited from the global model. It is our intention to further improve this model and make the model output available to the community at large.

Corresponding changes in the text are made:

Page 9, line 262-267: "Several sources may contribute to the larger misfit in model simulated subsurface temperature field. These include the model's spatial resolution, the sensitivity of turbulence closure schemes (we used Mellow-Yamada scheme in this study), and water mass biases inherited from the global model. It is our intention to further improve this model and make the model output available to the community at large."

I had only a few minor technical corrections/comments:

1) It was mentioned in the abstract that data from "glider transects in the MAB" were compared to the model but I didn't notice where.

We thank the reviewer for pointing this out. The text has been revised.
Page 1, Line 16-18: "… which included coastal sea levels, satellite altimetry sea surface height, in situ temperature and salinity measurements in the GOM, and observed mean depth-averaged velocities …"

2) p. 2758 line 5: Would the phrase "understand and quantify" be "understanding and quantifying"? I'm not sure.

Changed to "understanding and quantifying".

3) p 2759 line 4: "Gulf stream" should be capitalized?

Changed to "Gulf Stream".

4) Figure 7. Is there no arrow for the model at Martha's Vineyard Coastal Observatory or is it hidden under the observed vector?

Thanks for pointing out. We have added the model vector at MVCO.

5) p. 2767 line 8: It says of the Gulf Stream that "mesoscale eddy fields strongly perturb its mean velocity state". Does that mean that there is not enough years in the climatology? Is this not true for the shelfshope front jet as well?

The standard deviation of model SSH is based on subtidal output averaged over the M2 tidal cycle (~12.42 hour) over the 10 years, and the standard deviation of AVISO SSH is based on daily snapshots over the same period. So both results are based on a large number of realizations. The SSH variability associated with the meanders and eddy activities of the Gulf Stream is larger than that caused by the meandering of the partially density-compensated shelfbreak front.

6)Figure 9. It is mentioned somewhere in the text that there is a shoreward transport in the deep at the shelfedge but, based on the figure, that is really only occurring at the Long Island transect.

As stated in the text:
Page 12, Line 360-361 "For example, it was found that the cross-shelf current along the Long Island transect is moving shoreward (seaward) at depths shallower (deeper) than 50 m."
The cross-shelf flow is more complex, involving both onshore and offshore flow near the bottom. The divergence pattern at the Long Island is consistent with Lentz (2008). Similar pattern can be also found along New Jersey and Maryland transects.

7) Figure 10. I really like this figure. It should be replicated by anyone attempting to model the NE Shelf in the future.

We appreciate the reviewer's comment on this. We agree this figure shows the complexity of the along- and cross-shelf transport and exchange processes in the region, which should be examined by other models as well.

Again, we thank the reviewer for your constructive comments that helped us to improve this manuscript.

Responses to Reviewer 2:

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

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:
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).

Indeed, the work described in this paper is the first step to address more important processes including inter-annual variability of the circulation and ecosystem dynamics in the Northeast U.S. coastal ocean. As the reviewer pointed out, the spatial resolution of our MABGOM model is not much higher than that of the global HYCOM. Our goal for MABGOM is to produce a better representation of regional circulation by correcting biases in the global model and adding more complete coastal dynamics (i.e., river discharge and tidal forcing, which are absent in the global model) into our regional MABGOM model. Such a regional model can then provide dynamically consistent open boundary conditions (OBCs) for higher resolution nested models to investigate more specific research questions in each sub-regions. For example, the MABGOM model had provided OBCs for a Gulf of Maine coupled biophysical model (He et al., 2008; Li et al., 2009) and a high-resolution circulation model focusing on the shelfbreak processes in the Middle Atlantic Bight (Chen and He, 2010).

Corresponding revisions in the text are made:

Page 16, line 485-493: “Our goal for MABGOM is to produce a more accurate model representation of regional circulation by adding more complete coastal dynamics (i.e., river discharge and tidal forcing, which are absent in the global model) to the regional model and correcting biases in the global model. Such a regional model can then provide dynamically consistent open boundary conditions (OBCs) for higher resolution nested models to investigate more specific research questions in each sub-regions. For example, the MABGOM model had provided OBCs for a Gulf of Maine coupled biophysical model (He et al., 2008; Li et al., 2009) and a high-resolution circulation model focusing on the shelfbreak processes in the Middle Atlantic Bight (Chen and He, 2010)."

In this study, the hydrographic biases in the global HYCOM were corrected by replacing their mean values using climatology. Such a correction needs to be prognostically vetted in a regional model in order to generate dynamically consistent circulation fields. In other words, our downscaling effort here is intended to construct a better representation of regional ocean state variables, which can be used for other nested high resolution sub-regional modeling efforts.

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 temperate 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.

We did not use "Hydrobase as the ground-truth". The bias correction scheme we adopted only used Hydrobase T, S climatology to replace the T, S means of HYCOM solutions, so all the high frequency temperature and salinity variations (introduced through NCODA data assimilations) were preserved to depict variability occurred during our study period.

We agree with the reviewer that IPCC models have predicted various changes of ocean states into the future. But these model based estimations require extensive validations down the road, so we didn't use them in this study.

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

We applied the thermal relaxation in order to improve the fidelity of NCEP NARR surface heat flux. As in earlier studies (Chu and Edmons, 1999; Ezer and Mellor, 1992; He and Weisberg, 2002), our SST relaxation was done through the net surface heat flux correction, which is utilized as follows as the surface boundary condition in predicting ocean temperature:

$$K_h \frac{\partial T}{\partial z} = \left(\frac{Q_h}{\rho C_p}\right) + c(T_{obs} - T_{model}) \text{ at z=0}$$

where Q_h is the net heat flux, T_{obs} is satellite observed sea surface temperature, C_p is the specific heat, and c the relaxation coefficient (which was set as 0.5 day in this study). In other words, we are not directly changing 3-d temperature itself, rather letting the forward model to compute it as a state variable in a dynamically consistent fashion. A similar salinity relaxation would provide improvements towards better fresh water flux. However, we are not aware of any high resolution coastal surface salinity datasets that can serve such a purpose.

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 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 that 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.

We intended to compare the model solutions against different type of observations, including sea level data from tidal gauge stations over the Middle Atlantic Bight and South Atlantic Bight, satellite observed regional sea surface height fields, as well as time series measurements from NERACOOS moorings in the Gulf of Maine. The comparison of between observed and simulated depth-averaged current covers the entire Middle Atlantic Bight was also provided. The mixture of time series and spatial data comparisons make it difficult to summarize the comparisons in the table format. As we noted in our response to reviewer #1, comparisons were made for temperature and salinity at other NERACOOS buoys, showing similar results as Figure 5 and 6.

Following the reviewer's suggestion, we also examined comparisons between observations and HYCOM results to highlight the benefits of our own modeling approach. While surface temperature comparisons are very reasonable, we found clear mismatch in the subsurface thermal structures generated by HYCOM/NCODA's surface performance. HYCOM/NCODA also failed to resolve the surface variability due to the missing of fresh water discharge from the coast (Fig A3, and a4). The comparison of depth-averaged current further reveals the problem of this global model (Fig A5). HYCOM/NCODA systematically underestimates the mean velocity vectors, and at many places fails to reproduce the equatorward along-shelf flow.

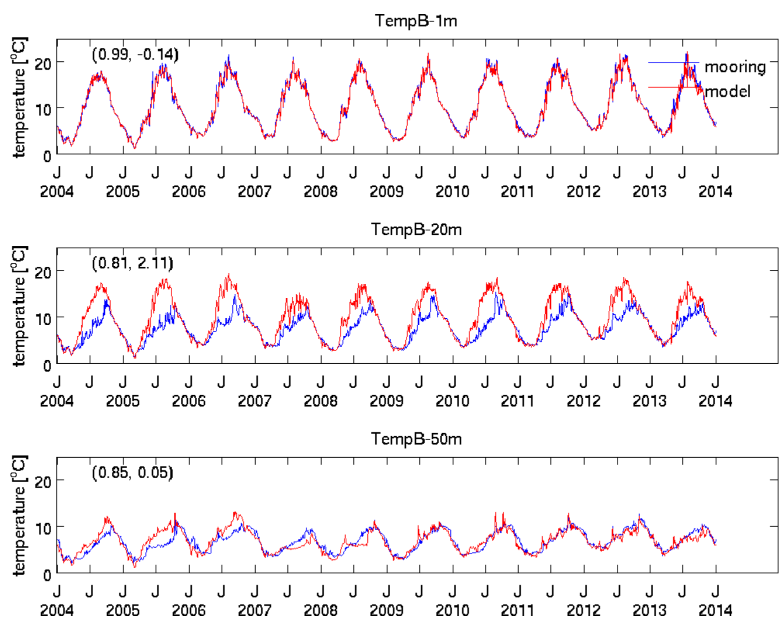


Figure A3. Comparison of observed (mooring) and HYCOM temperature time series at NERACOOS mooring B. Linear correlation coefficients (r) and mean bias (b) are shown in parentheses (r , b) respectively in each panel.

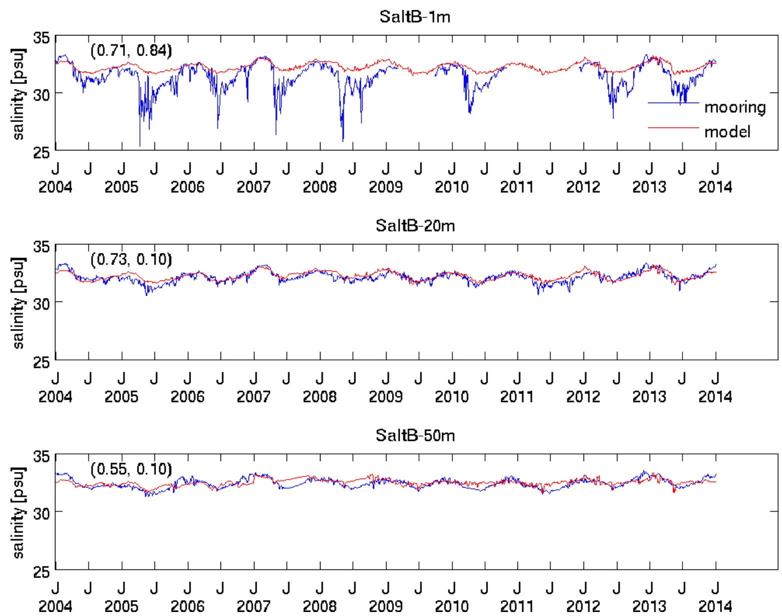


Figure A4. Comparison of observed (mooring) and HYCOM salinity time series at NERACOOS mooring B. Linear correlation coefficients (r) and mean bias (b) are shown in parentheses (r , b) respectively in each panel.

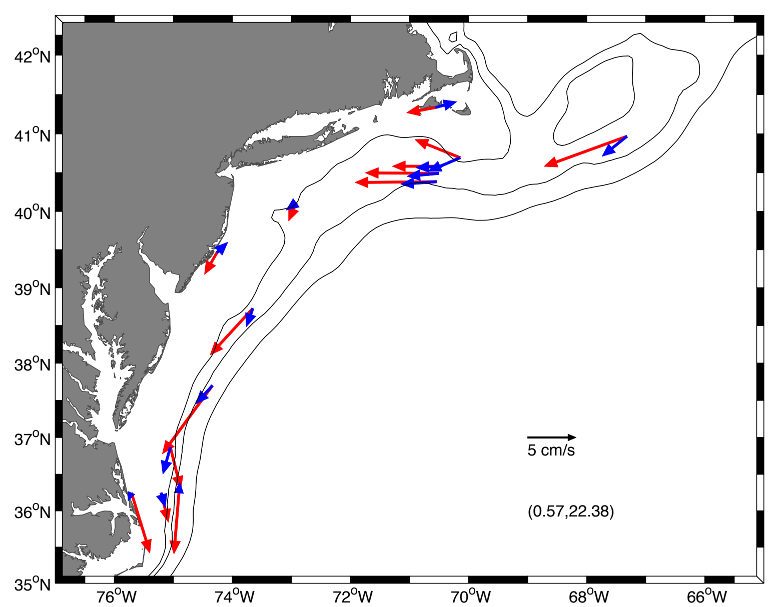


Figure A5. The comparison between observed (red, Lentz, 2008a) and HYCOM (blue) mean depth–averaged currents. Amplitude and angle of complex correlation between the model and observation are shown in the parenthesis (amplitude, angle).

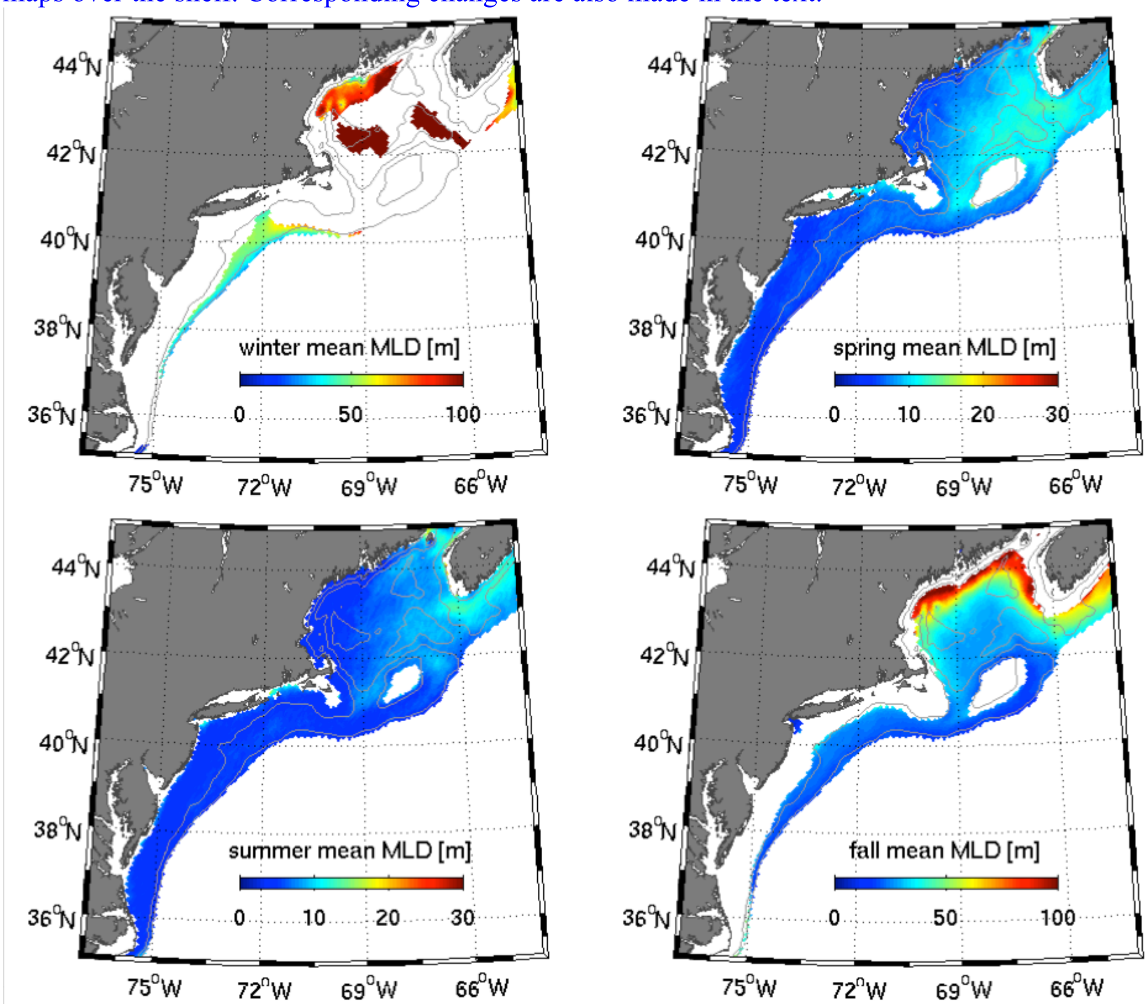
Corresponding changes in the text are made:
Page 16, line 495-503: "We also examined comparisons between observations and HYCOM results to highlight the benefits of our own modeling approach. While surface temperature comparisons are very reasonable, we found clear mismatch in the subsurface thermal structures generated by HYCOM/NCODA's surface performance. HYCOM/NCODA also failed to resolve the surface variability due to the missing of fresh water discharge from the coast (not shown).

The comparison of depth-averaged current further reveals the problem of this global model (not shown). HYCOM/NCODA systematically underestimates the mean velocity vectors, and at many places fails to reproduce the equatorward along-shelf flow."

Following the reviewer's suggestion, we added more quantifications of model-data comparison to make each comparison quantified. These changes include adding spatial correlation in figure 4, and complex correlation in figure 7. We focus on the mean circulation in the effort. More systematic model skill assessment will be provided in future correspondence on specific case studies.

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".

Following the reviewer's suggestion, we replaced the mean MLD map using seasonal mean MLD maps over the shelf. Corresponding changes are also made in the text.



Page 13, Line 377-388: "It is noted that MLD cannot be identified in regions with strong tidal mixing, such as Georges Bank, Nantucket Shoals, and some shallow estuaries. For most coastal areas, the long-term mean MLDs are 10-15 m. Embedded in this long-term mean are strong seasonal variations in the MLD fields. In winter, water column over most coastal region is well mixed except some deep basins in the GoM and shelf edge in the MAB. The intrusion of warm slope water onto the MAB shelf may restratify the shelf water in winter. During spring and summer, seasonal stratification develops, and the MLDs are 5-15m over the entire shelf region. In fall, increased storm events break down the seasonal thermocline and the MLDs start deepening. Strong tidal mixing in the GoM in combination of atmospheric forcing can deepen the MLD up to 100m. We note that the MLD depends on the method of calculation, and it is anticipated that these values will change using different definition of MLD(Kara et al., 2000)."

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.

As the reviewer pointed out, the 2-layer dynamics work well for the inner and mid-shelf. Further offshore, the presence of a separation of surface and bottom friction layers, the partially density-compensated shelfbreak front and baroclinic shelfbreak jet adds more complexity to the picture and the cross-shelf exchanges become highly variable. The meandering of the shelfbreak front and resultant shelfbreak eddies, and the impingement of Gulf Stream Warm Core Rings (WCRs) all contribute to the highly variable shelf-slope exchange process. Using a high-resolution nested model based on current model, we have focused on the shelfbreak frontal system and discussed the cross-shelf process (Chen and He, 2010). The variability appeared in the second mode of EOF analysis of cross-shelf velocity is more likely related to eddy activities. In another study on a large WCR in 2006, we found that over a time scale of one week, the WCR can significantly change the cross-shelf exchange of water mass, heat and salt (Chen et al., 2014). Other work also reported the significant role of eddy activities in the cross-shelf exchange (e.g., Gawarkiewicz et al., 2001; Gawarkiewicz et al., 2004; Joyce et al., 1992).

Corresponding changes in the text are made:
Page 15, line 441-450: "The meandering of the shelfbreak front and resultant shelfbreak eddies, and the impingement of Gulf Stream Warm Core Rings (WCRs) all contribute to the highly variable shelf-slope exchange process. Using a high-resolution nested model based on current model, we have focused on the shelfbreak frontal system and discussed the cross-shelf process (Chen and He, 2010). The variability appeared in the second mode of EOF analysis of cross-shelf velocity is more likely related to eddy activities. In another study on a large WCR in 2006, we found that over a time scale of one week, the WCR can significantly change the cross-shelf exchange of water mass, heat and salt (Chen et al., 2014). Other work also reported the significant role of eddy activities in the cross-shelf exchange (e.g., Gawarkiewicz et al., 2001; Gawarkiewicz et al., 2004; Joyce et al., 1992)."

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 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?

Yes, the temperature and salinity conditions largely contribute to the sea level slope. The Hydrobase climatology only serves for the correction of the long-term mean before the simulation. The model is driven by realistic oceanic and atmospheric forcings including river discharge, tide, and surface wind stress, freshwater and heat fluxes. So the modeled mean sea level slope represents more complete shelf circulation dynamics than just the geostrophic component.

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.

Removed from the abstract.

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

Quantifications are shown in the text.

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

Changed.

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

Changed.

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?

Changed as:
“We applied the method of Mellor and Yamada (1982) to compute vertical turbulent mixing. Harmonic horizontal diffusion/viscosity for tracer/momentum with a constant value of 20/100 m^2 s^{-1}, and the quadratic drag formulation for the bottom friction specification with a drag coefficient of 3×10^{-3} were also adopted ”

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

Changed.

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

Added as “Momentum terms in the figure represent time rate of change (accel), ageostrophic circulation (ageo), advection (adv), surface and bottom stress (str), and horizontal viscosity (vis).”

Again, we thank the reviewer for your constructive comments that helped us to improve this manuscript.

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