(Reviewer 1 without emphasis, Author response to comments with bold emphasis)

General Comments
This manuscript describes the development and evaluation of a regional modeling system (Doppio) for simulating the circulation and hydrography of the shelf/slope region off the northeast United States. As the authors note, regional oceanographic models like this are useful in that their results can provide the physical underpinning for studies of coastal ocean biogeochemistry and ecosystem function, and can themselves be used to investigate interannual and long-term variability in shelf circulation and hydrography.

The Doppio modeling system is based on the Regional Ocean Modeling System (ROMS), which is well described in the oceanographic literature, thus allowing the authors to dispense with detailed descriptions of model parameterizations and numerical schemes. Their focus is thus on the description of the external forcing (tides, river inflows, meteorological forcing, and open boundary forcing) and on the evaluation of the model results in comparison with numerous observational datasets (water properties, currents, sea surface elevation). The authors do a good job in describing the development and testing of the external forcing and the tradeoffs that are needed. The comparisons of model output and observational data are wide-ranging and give a clear picture of the model fidelity.
Overall, I consider this manuscript to be well written and worthy of publication. However, I have a number of specific comments, noted below, that I believe should be addressed.

I found the description of the river inflow forcing to be somewhat unclear. As best I can tell, the authors use a statistical approach whereby the integrated discharge over fairly large regions are predicted using gauged discharge at 27 of the largest rivers. The integrated discharge dataset comes from the Stewart et al (2013) reference. Examination of the list of the 27 rivers indicates that almost all are large rivers, with the exception of the Quashnet (which I note is NOT located in New Hampshire as indicated in the figure 1 legend) and Carmans. From the USGS website, the Quashnet (located on Cape Cod) has a tiny mean discharge of about 20 ftˆ3/s and a drainage area of only 2.6 square miles. It seems questionable to use such a small stream to predict the discharge of rivers over a wide area (it is not clear from the manuscript how large an area is represented by this river source). I would suggest the addition of a bit more detail in this section of the manuscript in order to flesh out some of these details. I also note that I was unable to locate the USGS page for the Carmans River, so it is not clear to me where the authors are obtaining its discharge. I know that this is a very small stream as well. My guess is that it is used to predict the discharge of all rivers on Long Island, none of which are particularly large, so this may not be a bad choice.

- **Quashnet location corrected in Fig. 1 legend**
- **USGS site 01306460 is used for Carmans. At the time of model development, the gauge was labeled "CONNETQUOT (Carmans R) BK NR CENTRAL ISLIP NY."**
- **The USGS discharge for the Carmans is scaled by the watershed discharge for all points on Long Island, NY, as such it serves as a collective for all of the relatively small rivers on Long Island and sources them to the model at one location.**

Specific Comments lines 79-81: The characterization of the GOM's bathymetry and currents as "uncommon" seems a bit strange. What exactly is uncommon? Granted that the bathymetry is rougher than that of the MAB shelf and the tidal currents are stronger, but uncommon is not a useful descriptor in my view.

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- **Word choice has been changed. Author's intent was only to note GOM bathymetry was directly shaped by glaciers and thus a more varied profile compared to the MAB.**

last paragraph of section 1: This may seem like a minor issue, but is it necessary to use words like "hibernal" and "estival" instead of wintertime and summertime? While it is nice to learn new words, having to look them up does interrupt the flow of reading.

- **Word choice has been changed.**

Figure 1: the legend for the moorings indicates (bold type face) that moorings B, I, and N are used for later correlation analysis, but the text (sect. 3.4) says that moorings B, N, and M are used.

- **Figure 1 legend has been corrected.**

Figure 2b: It looks like the estimates of PAR from corrected NARR shortwave radiation are biased low relative to MODIS PAR (many points below the 1-1 line). At least by eye, it looks like a regression line would lie well below the 1-1 line. So, I don't understand why the authors state that the mean ratio is close to unity.

- **Author acknowledges the corrected NARR shortwave radiation to be biased low relative to MODIS PAR when PAR is above 45, however this is only for two-thirds of the observations. The remaining third is the opposite, resulting in the mean ratio to be near 1.**

Lines 215-220: The description of the open boundary bias correction is not clear to me. I think that they adjust the mean of the boundary variables to match the mean values from their local analysis, however the writing is vague.

- **This interpretation is correct; we will rephrase the writing to better clarify the method.**

Figure 7c and 7d: The symbols in these figures are so numerous and overlapping that it is hard to decipher them. It is difficult to see the increase in correlation using the bias-corrected boundary conditions that the authors describe in lines 295-300 of the text.

- **Figure has been reworked so it is more legible.**

Figure 8: It looks like the coherence (blue) is only plotted for cases where the lower bound of the 90% confidence interval is greater than zero. This should be stated in the caption. Alternatively, the figure could be revised to include only the coherence and the level of significant coherence. This would make the figure less busy and allow more detail to be seen.

- **Further explanation will be added to the caption.**

line 310: Is it correct to say that at site N, the model captures high frequency and seasonal timescales? The coherences are 0.3 and lower, so the correspondence is not very high.

- **While the coherences 0.3 are low, they are still statistically significantly not zero. We will add text clarifying and acknowledging this.**

Figure 15: I think that some discussion of the differences between Doppio and the 4Dvar climatology over the outer shelf on Georges Bank and the MAB is needed. Doppio shows a separation between the coastal flow and the shelfbreak flow whereas the 4Dvar field indicates

equatorward flow over the outer shelf as well. Why is this so?

- **The 4D-Var climatology solution is computed from a model without tides and some other high frequency dynamics that may lead to rectified flows. Without further analysis we cannot easily explain the cause of these differences. We have noted in the revised text the reviewer's observation of these differences for completeness. Of note is that the latest CNES-CLES18 MDT (included below) has a similar feature to that which reviewer 1 describes, which indicates further that the true pattern is uncertain.**

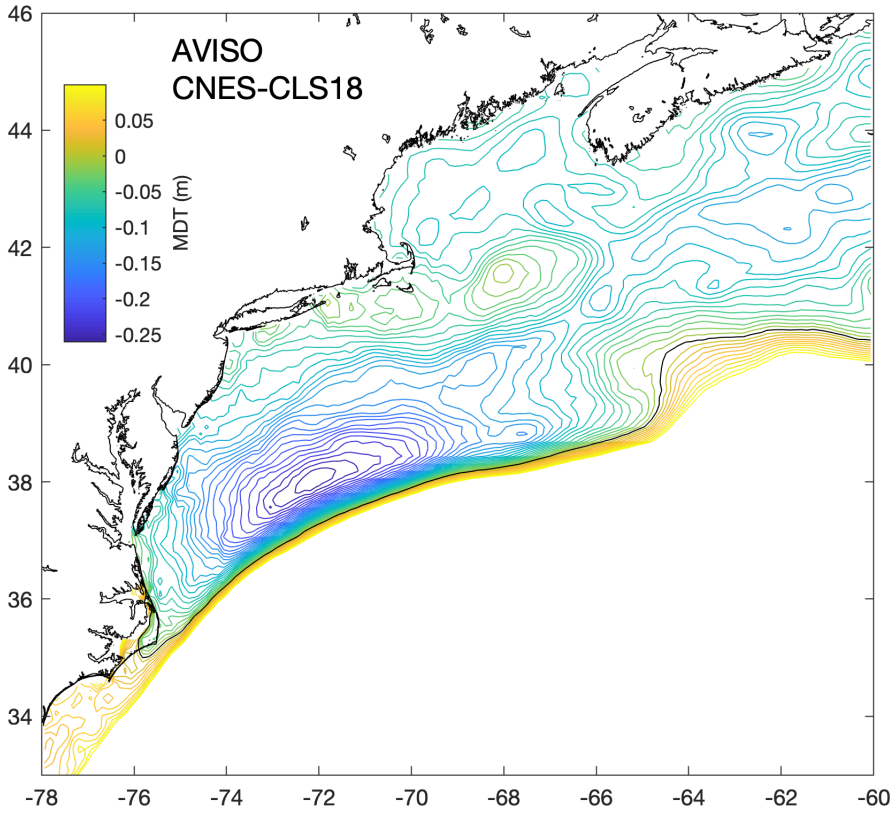


Figure 16: The bottom figure is supposed to represent the flow in the region from 100m depth to the sea floor. Why are there arrows plotted on the MAB shelf where water depth is less than 100 m?

- **This description has been corrected to model surface and bottom layers, hence the arrows plotted in the less than 100 m waters.**