

Responses to Reviewer2

We thank Reviewer 2 for his/her comments and provide our detailed responses below (in bold)

General Comment

This is a very good paper, well-written, easy to follow, and addressing key elements of physical nutrient supply to the biological community in the Middle Atlantic Bight particularly at the shelfbreak. Study of the enhanced biological activity at the shelfbreak front has spanned decades, with much learned but little real progress made in distinguishing competing mechanisms proposed as driving the observed elevation in biological activity. The model's value is that it allows separation of the various contributions to the continuity of nutrients and plankton, and this paper has done a very nice job of quantifying this, finding that the system is driven by wintertime breakdown of stratification and mixing of deep nutrients into surface waters, plus alongshore advection of nutrients originating from upstream features (e.g. Scotian Shelf and George's Bank), along with upwelling driven by bottom-boundary layer convergence at the foot of the front. This is a nice result, showing that several supposedly competing hypotheses are all active in setting the total function of the system. I recommend publication after some revision.

We thank the reviewer for his/her kind comments and encouragement.

Specific Comments

1) Why are 0-50m-average quantities chosen as diagnostics? My experience working here has been in the spring and summer, and 50m doesn't seem particularly relevant then, but maybe there's a reason that this is a better metric for evaluation an interannually and seasonally-resolved model. Please justify this choice.

The reason for choosing 0-50m average quantities as diagnostics is that 50m is generally the base of euphotic zone (Hales et al., 2009 JGR & JMS; Houghton et al., 2009 JMS) so that biological components in the upper 50m largely represent the major characteristics of the ecosystem. We have included this justification in the revision.

2) It seems that two terms are used to define 'surface' here; are they intended to be the same? Are 'surface' waters in Fig. 2 and 4 0-50m averages, or just the surface box, or some average over the (f(t,x)-variable) satellite optical depth?

Figure 2 and 4 show the ocean surface Chlorophyll concentration measured by MODIS satellite. The corresponding model results are also for ocean surface, not average over 0-50m.

3) Why use an isohaline as front ID (e.g. Fig. 12)? The front persists through seasons and interannually, when S structure variations are large. Is this a more reliable ID than a sigma surface, or an isobaths, or some modeled product, like the position of the alongshore jet?

Indeed, there are several ways to define the shelfbreak front. We chose 34.5 isohaline as a front ID in this study in order to be consistent with earlier observational and modeling studies by Linder and Gawarkiewicz (1998), and Chen and He (2010).

4) Units on continuity terms need m^{-3} , unless they have been corrected somehow. Were they multiplied by domain volume? The values don't seem consistent with this. Each term in the continuity equation in section 4.3 has dimensions of mass per volume per time; somehow the per volume got dropped in this section and Figure 13.

Yes, the unit for all nutrient diagnostic terms should be $mmol\ N/(m^3s)$. This has been corrected in the revision.

5) I'd like a little more than a 'note' that cross-shelf HADV much smaller than alongshelf. cross-shelf water transport is weaker, but property gradients are stronger. Also, HADV is exclusively defined as $-u\partial N/\partial x$, but the along-shelf dimension of the shelf-break shifts from mostly x to mostly y within the model domain. If the model domain has been defined to be isobaths-following, it's not clear.

The model domain is indeed not isobaths-following, and we agree with the reviewer that the shelfbreak along-shelf dimension changes within the domain. We have clarified in the revision that the model diagnosis (Figure 13) is for Nantucket transect only. On average, the cross-shelf HADV is about 20% of along-shelf HADV along the Nantucket transect.

6) Why use Chl-N as currency? There is lots of evidence of chl:C variability as $f(z,x,t)$, and using a standing stock doesn't really address the distinctions between convergent accumulation and net production. Please explain this choice.

We are not sure we understand the reviewer's question. We used the same biological model as Lehmann et al., (2009, Biogeosciences, 6, 1961-1974), and adopted the same convention for biological unit for consistency.

7) The self-shading argument (Section 4.1) is unlikely for the depths/densities of particle maxima shown here. This should be testable within the model framework, if the light dependence of Lima and Doney was truly used.

The self-shading effect is small in this modeling context.

8) The caption in Figure 10 is confusing. It refers to the N influx as a dashed line that the legend seems to show as a solid blue line, while the dashed line isn't called out by the legend at all.

We have revised the caption as follows:

“Monthly means of domain-averaged upper 50m nutrient concentration (black line), the upstream nutrient influx (blue line), and the surface mixed layer depth (red line) from 2004 to 2007. Also shown in the dashed red line is 50 m as a reference depth for MLD. Both N-influx and local N time series are normalized, whereas the domain- averaged, monthly mean MLD is not and has unit of meter.”

9) Figure 2 seems impressive, until realizing that it is a domain average that could have been reproduced with a box-model. This is reinforced by looking at Fig. 4, which shows clearly that the model doesn't reproduce the dynamic range in the spatial variability seen in the remote-sensing data. Figure 3, showing model-data agreement of surface chl-a, is a really nice figure, but, again, these are domain-wide annual averages of some definition of surface waters, and the model performance is quite variable between years. 2007 is an extreme-condition year that is evaluated extensively, and yet it has the worst model-satellite agreement. Are the 2004-2007 comparisons telling us about the real world differences, or problems with model performance between years?

Our coupled biophysical shelfbreak model is driven by realistic forcing. Both initial and boundary conditions are taken from an early realistic shelf-wide ecosystem model hindcast (Lehmann et al., 2009, Biogeosciences, 6, 1961-1974), which has been quantitatively validated during 2004-2007. Our model simulation is a continuous run from 2004 to 2007 with all model parameters kept the same, so we think the interannual variability simulated by this model is a reflection of the real processes, rather than model performance problem between years.

10) The cross-shelf Nantucket sections don't look right, either. The spring mean nutrient section (Fig. 7) looks like the concentrations are overall too low (5 mmol m⁻³ at 300m seaward of the shelf break in Spring?) and seem to be incompatible with the temporal average shown in Fig. 9. The P distribution is only consistent in that there is a subsurface maximum shoreward of the front, but even that is significantly weaker than in-water quasi-synoptic observations. There is no seaward P max in these results, as has been observed previously.

The right column of Figure 7 shows the spring mean <N> averaged over 2004-2007 and the corresponding nutrient anomaly N' in spring 2004 through 2007 relative to this 4-year mean, whereas the top panels of Figure 9 show the long-term mean N, P and Z over 2004-2007. Features in such seasonal or long-term means would indeed be weaker than those in synoptic observations.

11) The interannual variability looks suspicious. If I am interpreting the figures in the right column of Fig. 7 correctly, the deep offshore water nutrient concentration varies by 100% of its 4-year mean? This seems unlikely for deep slope waters to have this kind of

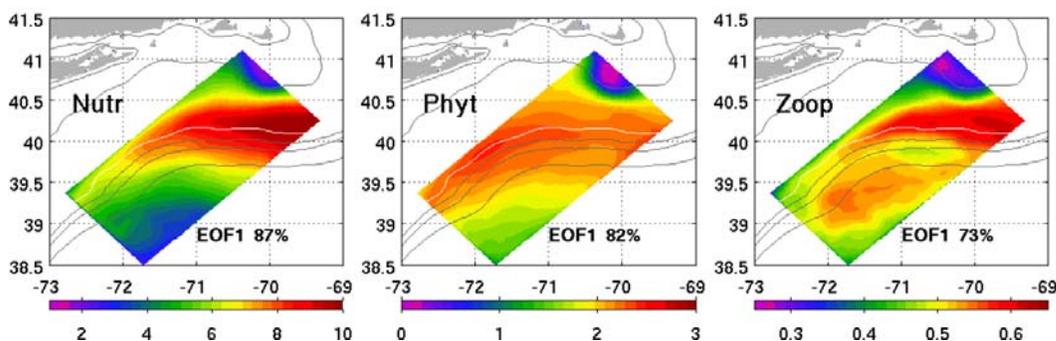
variability, and the mechanism driving it needs to be discussed in detail if the reader is to accept it.

Different colorbars were used in Figure 7 for spring mean $\langle N \rangle$ and anomaly N' , respectively. So the offshore nutrient concentration varies only by 10-20% of its 4-year spring mean. We have clarified the use of different colorbars in the figure caption

12) I'm having a hard time finding the 'shelf-break biomass enhancement', referred to in the abstract, in any of the figures, model or otherwise. The front doesn't show up as a noteworthy maximum in either model output or satellite data in Figure 4 or Figure 8; The subsurface bio-mass maxima in Figure 9 actually appear to be strongest shoreward of the front, and to diminish as the front is approached. This is consistent with Figs 4 and 8, where the strongest features seem to be well inshore of the front, and trapped at the upstream boundary of the model domain. Why is this? In some models, the boundaries are very tricky to deal with. Is it a concern that the strongest features are on the edge of model?

The existence of shelfbreak enhancement has been confirmed by synoptic observations (e.g., Marra et al., 1982; Ryan et al., 1999). We agree with the reviewer that such a shelfbreak biomass enhancement is difficult to detect in the modeled mean field (Figure 8, upper panel) because it is not a permanent feature in the MAB region. That's why we invoked EOF analysis to highlight this variability. The first mode (Figure 8, middle panel, also re-plotted below) show the shelfbreak enhancement in N, P and Z fields much more clearly.

As to the elevated nutrient and biomass concentrations near the Nantucket Shoals, they are associated with both strong tidal mixing in that area (He and Wilkin, 2006, JGR) and upstream boundary input from the Georges Bank and Gulf of Maine (modeled by Lehmann et al., 2009, Biogeosciences). Thus it is a ramification of both local and upstream dynamical influences rather than a model boundary condition issue.



13) After all this kvetching, I'll come back to saying that I don't believe that the objective of a modeling exercise is perfect reproduction of the real world, but rather study of the

mechanisms driving the system, as the authors have done here. The problem only comes when some unrealistic feature of the model becomes an important factor in the results. Can the authors acknowledge shortcomings in the model representation, but show that the ultimate findings are not likely to be affected by them? I think that is probably achievable.

2) There are several features of the quasi-synoptic sections we reported in Hales et al (JMS 2009, JGR 2009) and Bandstra et al (2006) that I hoped would be addressed by a good mechanistic coupled model. These are primarily related to the offshore biomass max and the physically/biogeochemically distinct characteristics of this feature from the onshore biomass max. The onshore feature was apparently more productive, was vertically separated from the base of the euphotic zone, and had distinct biooptical characteristics that coincided with distinct apparent nutrient and carbon uptake ratios. We never really had a good mechanism for explaining all those differences, but it seems like a good model that could resolve the secondary frontal circulation features (like this one can) and that included size-resolved phytoplankton assemblages (like this one does) might be able to resolve some of these features.

We appreciate the reviewer's suggestions and have implemented these points in our discussion section. We focused on the seasonal and interannual variability in this study, but indeed the next step of this research is to perform a detailed modeling study on the synoptic events as reported by Hales et al. (JMS 2009, JGR 2009) and Bandstra et al (2006). These observational studies have provided invaluable ground-truth data for future model calibration, validation and analysis.