

Interactive comment on “Seasonal and interannual variability of physical and biological dynamics at the Shelfbreak Front of the Middle Atlantic Bight: nutrient supply mechanisms” by R. He et al.

Anonymous Referee #2

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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

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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. I'll give the minor points of clarification first, and then offer some longer discussion items afterwards.

Clarification:

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 a better metric for evaluation an interannually and seasonally-resolved model. Please justify this choice.

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?

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?

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.

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

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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

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.

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 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.

Discussion points:

The most important conceptual concerns I have focus on overstating the model's performance, and on not addressing some (to me) interesting observations that should be explainable with this model.

1) The model doesn't really reproduce key features of the observations or the perceptions of the biogeochemical functioning of the region very well at all. 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?

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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 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 $\pm 100\%$ 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.

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?

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.

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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 bio-optical 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.

These features may be beyond the scope of the body of the paper, but they should be acknowledged and might provide some context for the discussion.

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