Here an NPZD-type ecosystem model is used to examine seasonal and interannual variability of the MAB shelfbreak front. This is a nice example of using a model to better understand observed processes. This manuscript appears to be appropriate for publications in Biogeosciences, but would be improved by addressing the comments below.

Abstract: It would be helpful to have some more detailed (perhaps more quantitative) results/conclusions included in the abstract. The abstract states that there are three processes that control nutrient supply, but the existence of these three processes is well known already. Can the authors provide some quantitative information, or at least comment on the relative importance of these processes in the abstract? The last sentence also states that there is significant interannual variability and vertical structure. Again, this is something I knew before reading the manuscript. Can the abstract provide more detailed information about how the vertical structure varies or what aspects of interannual variability are largest/smallest?

The one specific thing I learned from reading the abstract is that the phytoplankton bloom starts 1-2 months after the nutrient peak. I'm not sure that this has been quantified before. My question is – how sensitive is this result to the parameter choices made in the model? It would be nice to see some sensitivity results of some of the growth/grazing parameters on this result.

The authors call this model a "size-structured" ecosystem model, but in fact this model is not structured according to size. I realize that the original Lima paper called this model 'size-structured', but it's really not. There are other models that have allometric size relationships that are truly size-structured (see Poulin and Franks 2010, JPR, vol 32, #8, pp 1121-1130). The model used here is simply and NPZD model with two size classes, which is now probably the most common type of NPZD model. The authors should take 'size-structured' out of the model description (and out of the title).

I don't really see the "shelfbreak biomass enhancement" in the satellite data, which seems to me to be a problem since a large portion of the manuscript discusses this feature.

End of section 1: the names of the sections here don't really match the names of the sections in the paper. (Discussion is Section 4, not 5.) The model results are presented in Section 4, so this should probably be Results and Discussion, though it might be slightly easier to read if Results and Discussion were separated into different sections.

The text makes it sounds like the Lehmann model differs from Lima and Doney. The differences between these models should be provided. (Are there simply parameter differences, or structural differences? Line 25, p. 1559 – are the biological equations and parameterizations different in Lehmann? If not, the reader should simply be referred here to Lima and Doney.)

The authors should talk a little more about spin-up issues, and why they think their short spin-up time is adequate. In Figure 6, it looks to me like a lot of the differences between

2004 and 2007 (a major aspect of the discussion) could be due to inadequate spin-up time.

The model is compared almost exclusively to remotely sensed surface observations. Some comparison to in situ and subsurface data is required before using the model to analyze sub-surface processes.

Other issues:

"Shelfbreak Front" should not be capitalized in the title.

Is the MABGOM model nested within a North Atlantic simulation? More detail here would be appreciated.

Units should be double-checked in many figures. In figures 5 and 6 it looks like phytoplankton is shown in mmoles of phytoplankton! Units aren't given in many figures, such as Figure 7... Also, why is N in log units in Figure 7? I would much prefer to see N without logs. This would be much more meaningful to me.

Fig. 2 – can the standard deviations be included in the model results as well, for comparison?

Fig. 4 – the comparison in winter is really not very good, which is hidden in the color log scale. The model shows chlorophyll from about 0.8-1.2 throughout the whole domain, whereas MODIS shows 0.3 to 3. The authors should be more honest about the agreement here. Also, a Taylor diagram showing spatial variability for the four seasons would make a nice complement to the Taylor diagram shown in Figure 3. This would help the reader understand how well the model is getting the spatial patterns correct – which is a main point of the paper (i.e. the enhancement of biomass associated with the shelf break front.) It is customary and easier to read if the actual units on the color bar (0.5, 1.0, 3, etc... as opposed to -.4, 0, .4...)

Figure 10 – the normalization factors need to be explained a bit more thoroughly. Why are these needed? Why is the dashed line constant? The text talks about not being able to see the time lag in figure 10, which makes me wonder why the authors didn't simply zoom in on a certain time period and show us the time lag, if it exists. (Maybe it doesn't?)

Figure 13 – Why is VADV blue in the deepest waters in 2004, and red in 2007? Is this a spin-up issue? This seems like a big difference between these two years. Also, the authors should comment on the fact that it is the physics that is varying between years – the biology (SmS) is almost identical in both years. I think this is an interesting point that should be discussed.