Biogeosciences Discuss., 6, C3514–C3520, 2009
www.biogeosciences-discuss.net/6/C3514/2009/

Biogeosciences Discussions

Interactive comment on "Increased ocean carbon export in the Sargasso Sea is countered by its enhanced mesopelagic attenuation" *by* M. W. Lomas et al.

M. W. Lomas et al.

michael.lomas@bios.edu

Received and published: 14 December 2009

This reviewer makes an important point, the role of eddies, but I would argue that the heavily referenced paper (Mourino-Carballido 2009) makes the case for me that eddies don't contribute to the multi-year trend that is presented in our manuscript, for the following reasons. BATS is commonly impacted by eddies, and it is perhaps because of this that they are the "mean" and therefore don't contribute to the observed trend. During the 9-year period from 1993-2002 (which the reviewer is most familiar with), there are 8 cyclones, 6 Cyclone/anticlone interactions, 1 MWE and 2 anticyclones occurring during the January to April period. The MWE and anticyclones, due to low

frequency of occurrence at BATS during the winter/spring, likely don't have a significant impact on the observed trends in PP and other carbon pump variables. The C and CA's might be common enough to have an impact (ie., ~1 event per winter/spring period). In looking at the anomaly plots compiled by Mourino-Carballido, CA's enhance PP to the rate of ~3mmol/m2/day and C's depress PP to the rate of ~4mmol/m2/day. Given the roughly equal occurrence of these two features and their opposite effect, it seems that these features would 'average out' when it comes to any year-over-year trend. It is also worth noting that the observed increase over time in PP substantially exceeds the enhancement due to CA's. There is the possibility that CA's and C's both favor the growth of Synechococcus, but none of the eddy features identified have any impact on POC flux measured at BATS on the monthly cruises (Figure 4, Mourino-Carballido 2009), that is not to say they don't impact POC flux at all, it just isn't happening at BATS. I agree with the reviewer there are times where eddies have profound impacts on PP (e.g., spring bloom 1994), but these appear to be rare. Moreover, there doesn't appear to be a trend (over the 1993-2002 period) in the frequency of eddy features at BATS, with the one possible exception that CA's might have a higher prevalence between 1999-2002. So to me, based upon discussions with researchers who actively study Sargasso Sea eddies and reading of the published literature, eddies contribute to variability at BATS but not to year-over-year trends, at least over the length of our current data record. The reviewer's specific points.

1. changes in BCD over time in the mesopelagic. There are only 3 data points for the mesopelagic prior to 1996 coming from Craig Carlson's PhD thesis. I agree with the reviewer that the data point in 1993 is likely impacted by an eddy feature. If that is removed, as suggested by the reviewer, that only leaves two data points prior to 1996 to compare with the data post 1996. Statistical comparisons of this type are weak at best. I feel that the decrease in free living bacterial productivity over time (see figure below) is a very interesting observation in its own right, and highlight that. That said if I were to average the BCD values to compare with AOU and POC attenuation (with the caveat that there are only 2 data points prior to 1996) there is no significant

difference (before 1996, BCD = 2.5 ± 0.5; after 1996, BCD = 3.0 ± 1.4). Despite the lack of change in BCD, POC attenuation and AOU increase suggesting another source of heterotrophic metabolism. Regardless of whether or not I exclude the high point in 1993 and compare averages for BCD before/after 1996, the main conclusion, increases in heterotrophy by organisms other than free-living bacteria, remains valid. While I agree that eddies contribute to variability in the Sargasso Sea, the published data don't support a long-term trend in BCD associated with the occurrence of eddies and therefore have no impact on our interpretation of the observed trends.

2. Absolutely, there will be interactions between convective mixing and eddies during this time of year, where the type of eddy might impact the 'mixing pattern'. In the revised manuscript we have added specific sentences on convection/eddy interactions and how this might impact mixing frequencies during the winter/spring period. It is interesting that 1996 had equally high variance in MLD-CV and that seemed to have less eddy activity than 1994 or at least impacted by a very different eddy feature, cyclone vs. anticyclone (Mourino-Carballido 2009). The 1994 anticyclone was clearly a very strong eddy feature that was unique, none of the other 5 anticyclones in the record presented had anywhere near the deepening of the 20o isotherm. As discussed above, without temporal changes in one type of eddy over another (or their overall frequency of occurrence), it isn't clear how interactions between convection and eddies would result in the observed year-over-year trends, and therefore how not specifically discussing eddies influences our interpretation of the observed trends (as implied by the reviewer). In the end, the change in mixing frequency linked to NAO is just a hypothesis that may be proven incorrect.

Minor/technical comments: 1. Shorten sentence and modify. We agree this is a long sentence and have broken it up as below. However, we have not modified it as the reviewer suggested, because both the euphotic zone and mesopelagic zones see changes in plankton community and metabolic activity. The sentence as written is accurate, albeit long. "The increased mesopelagic POC attenuation appears me-

diated by changes in plankton community composition and metabolic activity in both the euphotic and mesopelagic zones. These changes are counter to extant hypotheses regarding inter-relationships between phytoplankton community composition, productivity and carbon export, and have significant impacts on how the Sargasso Sea ecosystem, at least, is modeled."

2. Grammar for section heading. 'rates' and 'stocks' are both measurements. As well, the revised text provided by the reviewer contains a number agreement problem (rates and stock). Therefore this section heading has been left unchanged, with the exception that the words 'rate' and 'stock' are switched to reflect the new order of this section.

3. Methods 2.1 order. Statement on data used in this analysis has been changed as follows. "This sampling scheme results in up to 4-6 data points during each annual winter/spring bloom period, the time period considered in this retrospective analysis.". The order of this methods section has been changed as suggested.

4. Van Heukelem and Thomas has been corrected, thank you.

5. Knap et al. 1997 has been added.

6. statement on 1% light depths has been moved.

7. Data were not interpolated. The BATS dataset was queried for nutrient concentrations that fell within the range of isopycnals listed. It was very rare that more than one measured datapoint fell within this isopycnal range so there is no range of nutrient data to report. In the rare instance where two data points were available they were averaged.

8. Not all variables have been collected for the entirety of the analysis period. For example, HPLC pigments didn't start routinely until 1990. Flow cytometric counts only started again in 2002 when I arrived at BIOS. Because we didn't wish to exclude any data by choosing the timeframe that had all data, there is variability in the lengths of data records. When it comes to statements on the duration of the record, we have

chosen the longest (ie., 17-years). For any given variable the duration listed in tables and presented in figures is consistent.

9. Percent change over 17-years. This has been corrected.

10. Reference to Corno et al. – data not shown. The correlations between TChl-a, PP and POC are shown in Table 2. While there is no plot of Assimilation Number over time, it is inferred from the table so it seems inappropriate to us to add 'data not shown' in this instance.

11. Changes in Synechococcus pigment biomass. This has been corrected, the ~45% given in the text was the correct value.

12. P-value for correlations between Teff and POCflux. The reviewer is correct, $P \leq 0.07$ for those comparisons, and this has been corrected.

13. BCD changes over time. Please see the beginning of this response regarding the comparison between BCD, AOU, and POC attenuation. In answer to the question why was AOU calculated for 200-300m only – it was to attempt to get an estimate of AOU below the mixed layer for the longest period of time each winter/spring season. If we integrated from 150-300m, and therefore had to wait until the MLD was <150m, there would be a larger temporal disconnect between euphotic zone and mesopelagic processes. Oxygen data prior to 1992 was not included because those data were generated using a different measurement system (see new methods). This information has been added to the data processing section.

14. winter time stratification. We didn't calculate a stratification index, rather just presented surface and 200 m $\sigma\theta$ values. We agree with the reviewer that talking about stratification during a period of convective mixing is odd. However, as other studies have suggested that increases in primary production are tied to changes in stratification which are tied to climate variables, we wanted to point out that there have been no long term changes in the density difference in the upper ocean at BATS.

15. Underwater irradiance changes. As suggested by another reviewer, because irradiance doesn't change significantly, this has downplayed. It is only stated once in the manuscript and the figure related to irradiance has been removed entirely.

16. Please see response to interactions between NAO and eddies with regard to mixing at the beginning of this response.

17. This statement has been clarified.

18. This statement has been clarified.

19. Karl et al. 2001 do not present seasonal data but rather annual data, likely because there is a small seasonal cycle at that location. There are many possible explanations for the different responses, this is just one that we feel is correct.

20. Table 1 has been corrected.

21. Table 2 (formerly Table 3) has been corrected as the Reviewer suggested, Table 3 (formerly Table2) has respectfully been left as is.

22. This has been changed to be "... $P \leq 0.05$."

23. Figure 2. Because we only chose nutrient concentrations that were within the stated isopycnal range, there was not always nutrient data for every cruise and therefore different numbers of data points in each series.

24. Figure 4 & 6 corrected.

Interactive comment on Biogeosciences Discuss., 6, 9547, 2009.

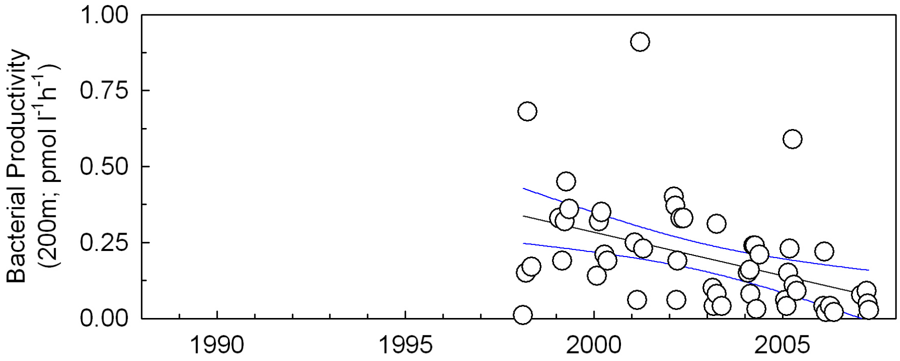

Fig. 1. Figure 1. Volumetric bacterial productivity at 200m at BATS. Data are downloaded from the BATS site and do not include the specific data of Craig Carlson's thesis. Data highlight the decline over