

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

Anonymous Referee #2

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General comments Lomas et al. present a compilation of ~17 years of data collected at the BATS site in the North Atlantic. This work shows the immense value of long term data sets in studying links between climate shifts and ocean processes. Overall I believe that the analyses of the data set are robust and that sound conclusions have been reached. There are a few instances where I do not agree completely with the authors' interpretation of the data. These are detailed in the section below. This data set includes the period of a shift in phase of the winter North Atlantic Oscillation and shows nicely the accompanying changes in physical and biological responses in the ocean. In light of this emphasis on effects of NAO forcing, I was somewhat surprised, therefore, to see such a focused title on this manuscript. I suggest that the authors

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broaden the title to reflect more accurately the breadth of information contained in the manuscript.

Specific comments - Science

1. p. 9554, line 7: What was the duration of the deployment of the sediment traps? Please add to this section. (The point of this addition will become clear in a later comment). Also, was the trap material preserved?
2. p. 9558, line 10: I had trouble reconciling how the increased cell abundance of *Synechococcus* shown in Fig. 3d) translated into an increase in TChl-a, since an increased contribution of *Synechococcus* to TChl-a is not apparent from Fig. 3c). Please clarify.
3. p. 9559, line 1: Figure 4d) is referred to, but I believe this should be 4a). Please clarify. Also, the increase in POC remineralization was indeed associated with an overall increase in AOU. However, while Teff decreased steadily from 1996 to 2007, the temporal pattern of AOU is quite different within that time frame (is high, declines, then rises again). Thus, to me, the suggestion that POC attenuation is due to increased mesopelagic activity has a rather shaky foundation. The authors do acknowledge that changes in trap collection efficiency can't be ruled out. The addition of further information to the Methods section (as requested in #7, above) might shed some light on whether loss of material from the traps could account for the decline in Teff, particularly if that material is not protected by minerals as diatom-based particles would be.
4. p. 9559, lines 23 to 25. The authors should include a back-of-the-envelope calculation to support their assertion that the observed increase in zooplankton biomass (Fig. 6) is sufficient to account for the observed increases in AOU. My read of Figure 6 is that the increase is driven strongly by the wide range of values in 2004. Would the same trend be observed if the data from 2004 were not included in the regression? Please clarify. This also relates to a later assertion that increases in mesozooplankton biomass/grazing results in the decline in diatom abundance (p. 9563, line 1).

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5. p. 9562, line 2: The MLD coefficient is negatively correlated with biological carbon pump parameters, but not significantly so. Does this weaken the argument for the proposed mechanism of increased primary productivity? Please comment.

Technical corrections

1. p. 9550, line 9: Change POD to PDO
2. p. 9551, line 1: Change “dominate” to “dominant”
3. p. 9552, line 15: three biomarker pigments and their corresponding groups are indicated as the “only groups that showed significant changes. . .”, yet dinoflagellates and pelagophytes are included in Table 2. Please fix so the table and text are consistent.
4. p. 9553, line 1: Change the order of Synechococcus and Prochlorococcus so that “the presence or absence of phycoerythrin, respectively” phrase will be correct.
5. p. 9553, line 7 and elsewhere in the text: please add “3” to the exponent (PO₄-3).
6. p. 9553, line 20: What volume of water was incubated for the primary production measurements?
7. p. 9555, line 25: Should “depth horizon” be “density horizon”?
8. p. 9557, line 1: Please put more emphasis on the fact that only winter/spring data are presented in Figure 1 and Table 1. I was somewhat confused as to what the word “period” referred to until I figured out that this meant the time period in years, not a seasonal time period (winter/spring vs. summer or fall).
9. p. 9557, starting at line 10: I believe that the Spearman Rank Order correlations are those presented in Table 3. If so, Table 3 should be referenced here. Table 3 would then become Table 2. Also, in Table 3, if $n=4$ for each variable for each year, then how does $N=15$ or 17 as is stated in the caption of Table 3? Please clarify. This assumes that n is the same as N . If not, please define N .

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10. p. 9558, line 20: I suggest removing Figure 5; it is not necessary. The authors have already convinced me (from data given on p. 9555, line 1) that the average depth of 1% PAR has not changed from 1992 to 2007.
11. p. 9558, line 25: Remove “Cross” from Spearman Rank Correlation. Also, in Table 3, the values I see for p -values are 0.08 and 0.07, which are not < 0.05 as stated. Please clarify.
12. p. 9559, end of line 18 to line 21: I found this sentence somewhat confusing. Perhaps shorten or split into two sentences?
13. p. 9561, line 9 to 12. The text that re-iterates (for the third time) the lack of change in 1% PAR depth can be removed.
14. p. 9561, line 28. The r value for the Spearman correlation in Table 3 says -0.48, not -0.58 as is in the text.
15. Figure 1: As mentioned in 9) above, please emphasize that these data are for January to April only. This could be done in the first line of the figure caption.
16. Figure 2: Please remove the lines depicting the trends as these are arbitrary. Also, please check the units (sQ?) in the y-axis label.
17. Figure 5. Delete (see above).
18. Figure 6. Suggest removing the white space (dates before 1993 or so), and add “at the BATS site” after “size class”.
19. Figure 8. If the data shown in Figure 8 are those from which the correlation in Table 3 was calculated, then I suggest removing Figure 8. As mentioned above, please check the r value (does $r=-0.48$ or -0.58 ?). In general, I like to see the r^2 of a correlation rather than r , but this is a personal preference.

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

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