

## ***Interactive comment on “Deep ocean mass fluxes in the coastal upwelling off Mauritania from 1988 to 2012: variability on seasonal to decadal timescales” by G. Fischer et al.***

### **Anonymous Referee #2**

Received and published: 9 March 2016

**General Comments** The authors present a >20 yr record of mass fluxes in the North Atlantic – this is a rare and valuable data set. The authors are trying to write a synthesis of a huge data set, and this is a worthwhile, if very challenging task. Understandably, the authors look for correlations between flux data and climate metrics. However, the manuscript suffers from an apparent listing of potential hypotheses that are at best modestly supported by the data in sections where there is not space to fully develop these ideas, especially the Introduction. Given the restrictions on space, the manuscript would be better served by articulating the working hypothesis that is best supported by the data in the beginning, i.e., that the organic carbon and BSi fluxes are mostly highly correlated with dust deposition, and discuss the alternative hypotheses

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in the discussion section. A table of flux (including total, lithogenic, Corg, and BSi) correlations with different climate statistics, i.e., NAO, ENSO Index, sea surface temperature, sea surface pressure, might be a more coherent and easily digestible way to present this data.

I also expect that the study would benefit by comparisons of their data with data collected in similar locations, i.e., the long-term study sites off the California coast, e.g., CalCOFI data sets and data sets from the San Pedro and Santa Monica Basins. There have been a number of studies of those California sites looking at relationships between productivity, water column oxygenation, winds, and upwelling. It would be interesting to see if there are similar trends observed in the Eastern Pacific and Eastern Atlantic basins.

p. 17660: How does the fact that particles analyzed in this study were < 1 mm (because of sample filtration) relate to the predominant grain size of dust particles being between 10 and 20 nm? Are those dust grains broken down in size between the surface ocean and deep traps? If the authors are invoking mineral dust as the primary driver of the sinking flux in this manuscript, but the samples exclude particles > 1 mm, is that consistent with the dominant grain size of dust being much larger than the filter size?

Specific Comments In abstract, the discussion of AMO is confusing – since there is no or only a weak correlation, I recommend dropping the discussion of AMO and focus on the positive relationship with dust deposition. It may worth mentioning that you looked for a relationship between BSi and AMO in the discussion, but it is distracting in the abstract. Abstract would benefit from not discussing the hypotheses that were not supported by the data – only focus on the hypothesis supported by the data, and the data that supported the hypothesis.

P. 3 is confusing – a list of hypotheses that are sometimes supported by the data and sometimes not – unsure what message we are to take away from this other than that there is no statistically significant correlation w/ anything? If this is not the message,

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the Intro needs to be restructured around a single, coherent message. I appreciate that the authors are trying to look for correlations between their data and climate indices, but this information is better suited to the discussion.

Section 2.3, line 20-25: How do the authors evaluate and quantify the relative strength/magnitude of correlation between climate variables and flux metrics? Which statistics are used? Section 2.3 did not convince me that there were meaningful correlations between climate indices and upwelling and/or flux metrics at the trap location.

p. 17652 line 1: The text says total nitrogen was measured, but it is not reported in the tables or figures. This would be a very valuable set of data to include. If the authors chose not to include the total nitrogen flux data they should not report that it was measured.

p. 17652, line 13: Please describe the factor of 2 that the Corg is multiplied by

p. 17654, Results: Please specify whether differences in the bulk fluxes are statistically significant higher in winter and summer than fall and spring.

The Results section would benefit from stating the ranges of the total, Corg, BSi, CaCO<sub>3</sub> fluxes.

p. 17655, Results, lines 12-19: reporting the slopes together with the correlations would be valuable.

p. 17658: Please discuss how the analysis of Corg fluxes in trap samples collected >1000 m affects interpretations relative to fluxes of BSi, CaCO<sub>3</sub>, and lithogenic fluxes, that do not experience flux attenuation with depth the way Corg fluxes do, and whether this is expected to affect a correlation with remote sensing data of sea surface chl

p. 17661: Doesn't an increased mass flux with La Nina conditions contradict other text where the authors state that fluxes are not correlated with the strength of upwelling?

Table 3: Does important mean statistically significant? If so, how significant?

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Table 4: Similarly, what is meant by “important”?

Figure 2: What is implied by “strong changes”? It is not clear what the reader should note happening over the past four decades. Is there something unusual? If so, unusual relative to what?

Figure 5: Not clear to mean if the gray shaded area is the data from the shallower trap? If so, it appears that there is more data from the upper than lower trap, and so the figure should be about data from the upper, and not lower trap.

Figures 5 and 6: Not clear what the shading of El Nino/La Nina represents, since those colors don't appear elsewhere in the figures nor are they described in the figure captions

Technical Comments Comma usage and grammar are problematic throughout the manuscript

Page 17653, line 14: incomplete sentence

Figure 4: The font size of the y-axis labels is too small to read – you could replace with “Mass flux”, “Corg flux”, “BSi flux”, “CaCO<sub>3</sub> flux”, and “lithogenic flux” and note in the figure caption that all are mean seasonal values.

Figure 5 caption: polynome should be polynomial?

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Interactive comment on Biogeosciences Discuss., 12, 17643, 2015.

**BGD**

12, C10138–C10141,  
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