Biogeosciences Discuss., 12, C10437–C10445, 2016
www.biogeosciences-discuss.net/12/C10437/2016/

Biogeosciences Discussions

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.

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Received and published: 4 April 2016

We first thank the anonymous reviewer #2 for the comments and suggestions, which will improve the ms.

RC 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 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.

AC We understand the argumentation of reviewer #2 to make the manuscript easier to digest. Our major motivation was to investigate the variability of deep ocean mass fluxes in the NE Atlantic and link them to major climate phenomena, such as the NAO. From a statistical point of view, we see no impact of the NAO on the deep ocean mass fluxes (BSi and Corg). This is due to local episodic perturbations, e.g. by dust deposition, which is assumed to weaken the relationship between fluxes and the NAO (as pointed out in the abstract). We will rephrase the introduction and focus now more on the flux correlations and the NAO index (reviewer #2: 'the working hypotheses that is best supported by the data'). Later, in the discussion section, we will discuss other possible drivers (e.g. AMO, ENSO) as suggested by reviewer #2 (see also below). There is no single and straightforward hypotheses. As several factors together impact on this region in different and complex (non-linear) ways, there are no alternative hypotheses. We believe that we carefully discussed these points, as also mentioned by reviewer #1 ('It is therefore consistent to investigate the relationship between fluxes and indices of climate oscillations of multiannual or decadal scale (AMO, NAO, ENSO). The authors manage to present this complex matter in a comprehensive and concise way. The paper is well written and well organized and the evaluation of the results is critical and refrains from over-interpretation.').

Compiling a large matrix of correlation coefficients between all kinds of climate statistics and fluxes of all parameters will mostly show statistically insignificant values (r^2 <

0.1), except for the NAO and BSi/Corg. Additionally, we believe that it makes little sense to compare i.e. lithogenic fluxes (=dust: dependent on aridity, winds on land, etc.) with i.e. SST in the ocean. Instead, it makes more sense to try to relate the NAO (influence on coastal upwelling and biomass) to BSi (=diatom, primary producers) fluxes or organic carbon fluxes, which is presented in the respective figures. Another problem is the potential effect of time lags (e.g. from ENSO, say we use a 3.4. index) from the Pacific Ocean to the NE Atlantic coastal upwelling. Potential time lags of zero months up to 1-2 years are discussed in the literature. It would be difficult to capture all these timescales without enlarging the discussion and the paper significantly.

Nevertheless, we focus on the flux relationships with NAO in the revised abstract and introduction section and, as suggested by reviewer #2, move other influences more to the discussion section. We will add a few sentences in the revised introduction section, which will make it more clear why we focused on specific parameters. Discussing all possible relationships is well beyond the scope of this paper. We therefore selected those parameters, which may be interpreted in the light of earlier findings in the literature and that are related, e.g. to upwelling of nutrient rich deep water and the production of biogenic silica.

RC 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.

AC We will briefly mention the other EBUEs in the revised manuscript. We did not intend to make an inter-comparison study of fluxes of the Canary Current with other Eastern Boundary Upwelling Systems such as the California Current System. An intercomparison between both systems would further enlarge the extension of our paper.

Both systems are dominated by different climate forcings on the longer-term (different relative importance of NAO, ENSO etc.), making a comparison rather complicated and clear trends less obvious. Furthermore, a high and continuous dust supply is less pronounced in the California Current System compared to the CC system. An intercomparison needs to include the Benguela and the Humboldt Current Systems and not only the California Current) and can be the topic of a separate study.

RC 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?

AC There seems to be some misunderstanding. Dust particles of 10-20 μm may easily pass through a filter of 1 mm. The point is that these fine-grained dust particles can only sink at higher rates (10-100 meters per day) when being incorporated into larger organic-rich particles such as marine snow aggregates (in our case off Cape Blanc around 1mm or less in size, Nowald et al., 2015) or fecal pellets (several 100 μm large). Organic carbon and dust together with other particles sink to depth and form the deep ocean flux. However, within the sampling cups, those particles mostly disintegrate due to their fragile nature. Consequently, we do not exclude any particles except some large carcasses from crustaceans, larger pteropods and rare 'swimmers' which are in the size fraction > 1mm. We will add a sentence in the method section.

RC 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.

AC See AC comment above. It will be deleted in the abstract and is referred to only in the revised Discussion section. In the revised abstract, we will mainly refer to the NAO.

RC 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, 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.

AC Indeed, there is no a single coherent message in the literature concerning the influence of larger scale climatic oscillations and reactions within the NW African coastal upwelling system (see ACs above). As mentioned above, we have changed the focus in the revised Introduction on the potentially primary driver, i.e. the NAO. We will rephrase and slightly restructure the Introduction in order to make the different motivations and hypotheses available in the literature and the main points addressed in our study more clear.

RC 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.

AC The temporal correlations shown in Fig. 3 indicate a linear relationship of sea level pressure variations at each gridpoint with the respective climate index (NAO, ENSO, AMO). Sea level pressure variations are a direct indicator for wind variations and therefore also for wind-driven (coastal) upwelling. The patterns in Fig. 3 clearly show that wind changes in our study area are part of large-scale teleconnection patterns (also supported by the literature given in the Introduction) and that consequently, wind-driven

upwelling changes may be linked to the respective phase of the climate oscillations.

However, the sediment trap fluxes will always record the superimposed influence of the teleconnection patterns. At this stage, it is difficult to disentangle the relative importance of NAO, ENSO and decadal fluctuations for the fluxes based on the very limited number of realizations of the respective combinations (e.g. El Niño coinciding with a NAO-phase etc.). Longer sediment trap records will potentially help in clarifying this issue. In this sense, our sentence on p.17650 l.25 should be taken as a cautionary note which gives a perspective for future work in this respect.

RC 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.

AC For further information, the nitrogen data are now included in Table 2. However, we will not discuss them in detail.

RC 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.

AC To estimate organic matter composed mainly of C, N, H and O, we used a conversion factor of two as about 50-60% of marine organic matter is constituted by organic carbon (e.g. Henson et al., 2002). Fig. 4 does not show significant differences between seasons, therefore our focus is on the longer-term and interannual changes of fluxes (see also comments of reviewer #1)

RC The Results section would benefit from stating the ranges of the total, Corg, BSi,CaCO3 fluxes.

AC We present the ranges in the revised Results section.

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

would be valuable.

AC We present the slopes and show them in Table 3.

RC p. 17658: Please discuss how the analysis of Corg fluxes in trap samples collected >1000 m affects interpretations relative to fluxes of BSi, CaCO3, 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

AC Biological/bacterial degradation of Corg is much faster than dissolution of CaCO3, BSi and mineral/lithogenic components. Surface chlorophyll should be better related to Corg than to lithogenic and biogenic minerals – the phytoplankton pigments will also be degraded as the total Corg. Therefore, we did not compare CaCO3 and lithogenic fluxes to surface chlorophyll from satellite imagery. If the Corg decay over time is constant (which is not precisely known), we may relate it to surface chlorophyll produced by phytoplankton.

RC 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?

AC No, there is no contradiction. We provide a description of a single ENSO event 1997-1999 and the resulting consequences for the NW upwelling as known from literature. We did not argue that fluxes are not correlated with the strength of upwelling. We speculate that the reviewer possibly had in mind part of the introduction (p. 17647 ll 14-16), where we cautiously note that there does not necessarily have to be a simple link between changes in upwelling and deep ocean fluxes.

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

AC No, it does not mean "statistically significant". We deleted this sentence. We will add the number of data points and indicate the statistical significance (r^2, 99%/95% confidence level).

RC Table 4: Similarly, what is meant by "important"?

AC We meant 'important' for our study. We deleted 'important'.

RC 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?

AC We removed this sentence in the caption.

RC 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.

AC More data from the lower trap than from the upper one are available and the former is better suited to record coastal upwelling. We will change Fig 5. and the caption: 'Total mass fluxes of the lower traps are gray shaded. White bars denote sampling gaps where upper trap data have been used to fill the gaps of the lower trap record'.

RC 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

AC This will be better described in the revised figure captions.

RC Technical Comments Comma usage and grammar are problematic throughout the manuscript

AC A native speaker will check grammar in the revised version.

RC Page 17653, line 14: incomplete sentence

AC We delete this sentence.

RC 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", "CaCO3 flux", and "lithogenic flux" and note in

the figure caption that all are mean seasonal values.

AC Will be done

RC Figure 5 caption: polynome should be polynomial?

AC Will be done

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