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Biogeosciences Discussions

Interactive comment on "Variability of phyto- and zooplankton communities in the Mauritanian coastal upwelling between 2003 and 2008" *by* Oscar E. Romero et al.

Anonymous Referee #2

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Dr Romero and colleagues reports on the phytoplankton and zooplankton communities collected during five years by a sediment trap deployed in the Eastern Boundary Upwelling Ecosystem of the Canary Current (site CBeu in the Mauritania coastal upwelling). Authors provide detailed taxonomic information of a variety of microplankton groups including diatoms, coccolithophores, calcareous and organic dinoflagellate cysts, planktonic foraminifera and pteropods. Interestingly, authors find no evidence of succession from primary to secondary producers in their analysis. Between 2004 and 2006 authors report a change in the "normal" seasonal pattern of microplankton composition sinking into the traps mainly revealed by an increase in benthic diatom species and other changes in coccolithophore and pteropod assemblages. This signal

is interpreted by the authors as incursions of southern waters into the study region. Overall the manuscript is well-written, figures are appropriate and provides new and important insights into the functioning of upwelling ecosystems (that sustain an important fraction of global fisheries). As authors state in their manuscript, such long sediment term records of phytoplankton and zooplankton fluxes are rare but of critical importance to understand the functioning of open ocean ecosystems. This is probably the first multiannual sediment trap study in the world's ocean documenting such a large variety of microplankton groups (including both primary producers and some zooplankton groups). This type of approach is needed to improve our understanding of marine ecosystems and to establish baseline data of microplankton community composition in key regions of the global ocean. Given the value of this data set, I recommend the publication of this manuscript after some minor-moderate changes are implemented. Please find my comments below.

Title. The current title is somewhat misleading. As it reads now it seems that authors documented phyto- and zooplankton communities from the upper water column. Since phytoplankton and zooplankton assemblages can be severely altered before reaching the sediment traps I would suggest to find alternatives for the current title. Mentioning the terms sediment trap or fluxes would help to give the reader a better idea of the content of the article.

Lines 20-21. Do authors mean calcareous and organic dinoflagellate cysts? Please revise.

Lines 55-60. Authors provide a short summary of previous long-term monitoring experiments in the global ocean. As stated in the text, these type of studies are scarce, however, there are several sites in the global ocean where multiannual records of microplankton and biogeochemical fluxes have been analysed. The IOC-UNESCO report only covers very few of these sediment trap experiments. Since the current work is based on sediment trap samples, it is important to include in the introduction some of these studies in order to provide the reader with better picture of previous similar work.

Section “3.1 Moorings, sediment traps and fluxes”. Can authors provide the depth of the water column at the study region? It is important to know the distance from the sea floor in order to assess the possible influence of resuspended sediments in the trap record.

Line 184: “Uncertainties with the trapping efficiency due to strong currents (e.g. undersampling, Buesseler et al., 2007) and/or due to the migration and activity of zooplankton migrators (‘swimmer problem’) are assumed to be minimal in this depth range.” Is this assumption based on Buesseler et al. (2007) paper? Or is this an assumption made by the authors? I would suggest to include the reference at the end of the sentence to support the whole statement and/or explain better.

Line 246-248: Authors refer to a taxonomy key that they used for dinocyst identification. However, no names of dinocyst taxa are provided in the manuscript. Why do authors provide species names for all microplankton groups but not in the case of the dinocysts? Please clarify.

Line 333: “On average, the carbonate fraction (CaCO3) dominates the mass flux (41% to the total mass flux)” Is this average a mean of all samples without considering the magnitude of the flux? Or is it an annual integrated average? I would recommend that authors provide annual values of main biogeochemical components of the flux, microplankton groups and major species in an additional figure. This information would greatly facilitate the comparison of the results of the current study with other investigations conducted in other regions of the world’s ocean.

Lines 336-337. I do agree that the main contributors to CaCO3 and BSI must be the ones listed in the text. But how do authors know that the bulk of the organic carbon is delivered by diatoms coccolithophores and organic dinoflagellate cysts? Although this possibility is likely, the data provided in the current manuscript h is insufficient to reach such conclusion. In particular, in the case of diatoms, they were treated with chemicals that removed their organic content, a process that impedes the estimation of

the number of cells that reached the trap with their cellular content intact. An important fraction of the organic matter could correspond to other phytoplankton or zooplankton groups, faecal matter or other components of the marine snow. Is this statement based on previous research in the study region or is it just an interpretation by the authors? I would suggest to either provide more evidence or be more cautious with this statement. Moreover, could authors provide some insights into the contribution of the different components of the CaCO3 flux to the total CaCO3? It would be really interesting to see which microplankton groups are the most important in CaCO3 export to the deep sea.

Line 404. Why not dinocyst species are presented? Authors should provide a list of species, not only groups.

Line 416. Defining Globigerina bulloides as an upwelling species is an oversimplification of the environmental preferences of this species. The contribution of this species is often higher at times and in regions of high primary productivity, but such conditions are not necessarily linked to upwelling. Please explain better and provide references to support the affinity of this species for certain environmental conditions. Please also note that planktonic foraminifera species distribution is also influenced by changes in primary productivity not only SST as suggested in the discussion (line 582).

Line 425. “Heliconoides inflatus (formerly known as Limacina inflata)” Why two names for the same species are provided? Please clarify and provide references supporting this statement.

Lines 437-448. Authors do not mention the relationship between the different phytoplankton groups and Chlorophyll-a in Figure 5. Why? This is an important parameter that should be discussed.

Line 479. Many other studies (including sediment trap studies) have documented a simultaneous increase in the abundance of different microplankton groups during favourable conditions for phytoplankton growth. Perhaps authors could discuss their

results in light of Barber and Hiscock (2006, GBC). Please consider this suggestion and incorporate if appropriate. I found interesting that dinocysts increase their fluxes together with those of diatoms and coccolithophores. I would expect that the cysts are developed at the end of the productive period. Could authors briefly summarize/mention previous studies that describe the environmental parameters that trigger dinocyst formation?

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