Interactive comment on “Impact of open-ocean convection on particle fluxes and sediment dynamics in the deep margin of the Gulf of Lions” by M. Stabholz et al.

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Detailed reply to Dr. T. Tesi comments

1) **Reviewer #1:**

The current format is a little unsatisfactory and I have the feeling that the authors struggled with structuring the data outline because of the heterogeneity of dataset. Up until the discussion the paper is well written and easy to follow. However, in the discussion, the authors jumped straight to the comparison with other studies although their own dataset was entirely undiscussed. I didn’t find this approach very satisfactory as the reader should first know what kind of particulate fluxes the sediment traps have collected and what this comparison is about. Therefore, I think that a well structured discussion should at first focus on the physical dataset, starting from the time-series of temperature and current data. Once the physical part has been described, this should set the stage for the discussion on particulate fluxes at different depths bringing up the occurrence of lateral and vertical fluxes. In doing so, the dataset is well linked as a collected sequence of thoughts making sure that the reader understands the difference between seabed resuspension and sinking of marine phytodetritus. So at first, put more stress on when these fluxes occur, their intensity, and then subsequently link them to previous studies and the effect of the benthic communities.

Reply:

We agree with the reviewer. As suggested we now introduce the discussion with a new paragraph that resumes the main results of our dataset, making the link between all findings before putting our findings in context with others. The respective paragraph is “Measurements obtained between September 2007 and April 2009 in the convection zone of the Gulf of Lions allow observing two contrasting winters. Winter 2007-08 is characterized by a mild convection with a mixed layer depth limited to 1000 m-depth, low current speeds and low particle fluxes in the water column and near the bottom. In contrast, winter 2008-09 is characterized by a deep convection, with a mixed layer depth reaching to the bottom (2350 m-depth), stronger current speeds in the water column and near the bottom, and significant particle flux near the bottom. The extent of the convection zone, characterized by the minimum concentration of surface Chl-a, is smaller in winter 2007-08 than in winter 2008-09. The difference in the intensity of convection between the two winters primarily results from larger net heat losses during fall/winter 2008-09 compared to fall/winter 2007-08. At first, we consider the seasonal variability of currents and particle fluxes and interpret them in light of previous observations gathered in the Northwestern Mediterranean Sea. Second, we will discuss the impact of the interannual variability of convection events and interactions with dense shelf water cascading on deep particle fluxes and the sustainment of a bottom turbid layer in the deep basin. Finally, we will discuss the effect of convection events on sedimentary substrates and deep-sea ecosystems.”

2) **Reviewer #1:**

In addition, as the outcome of this study is well supported by the data, I would suggest speculating/discussing rather more about the potential effect of resuspension of sediment vs export of marine phytodetritus. Lateral and horizontal fluxes have such a contrasting OM composition and it is likely that the effect on the deep sea communities is not just a matter of flux magnitude. For example, in the Bari canyon (southern Adriatic sea) the composition of suspended material collected via sediment traps (∼35 m above the seabed, deployed at ∼600 m) alternates periods characterized by significant lateral advection of aged OC (thousands of years old!!) from outer-shelf deposits with periods influenced by the export of modern marine phytodetritus from the upper water column (Tesi et al., 2008, Deep-Sea Research I). Contrasting 14C ages of particulate material were also observed in the Gulf of Lions as a result of resuspension and primary production (Tesi et al 2010, Progress in Oceanography). Therefore, given the different reactivity of these contrasting pools of organic carbon, the presented results can be further discussed in terms of labile vs reactive carbon and relative implications for deep sea sediments (biogeochemistry, ecology, etc).

Reply:

We agree with the reviewer. The objectives of the present paper were to describe the open ocean convection impact primarily on the quantity of POC that is resuspended from sediment or advected from the surface to deep water. From our point of view, speculating about the impact of POC fluxes without presenting data on the composition of mass flux is not satisfactory. Actually, a second paper dealing with the sources, origin and quality of the TMF fluxes during open ocean convection (with opal, ^{13}C and N:C data together with MEB pictures) is currently in preparation and will provide more information on the flux composition allowing a better assessment of the impact of OOC. Moreover, the reviewer have to keep in mind that during both winters of this study no intense DSWC events occurred (LDC1000 data) meaning that no lateral fluxes coming from the shelf contributed to the high fluxes recorded in winter 2009. However, as suggested we changed the discussion part 5.3 on the effect of OOC on deep ecosystems and particle flux, the respective paragraph reads now: "Our observations reveal the effect of OOC on POC load in the deep water layer. Inputs of POC exported from the surface layer following the winter 2007-08 mild OOC or primarily released by resuspended sediments during the winter 2008-09 deep OOC event may fuel the deep-sea pelagic microorganisms and trigger enhanced biological activity (Boutrif et al., 2012). The POC derived from sediment resuspension is generally much older (up to several thousand years old as described by Tesi et al., 2008, 2010) and more refractory than the POC from the downward export of modern marine phytodetritus. The response of deep-sea ecosystems is thus likely dependant on the intensity of OOC that regulate the quantity and quality (i.e., labile vs. refractory) of the POC."

3) **Reviewer #1:**

Fig. 4. Sensors of temperature should be also presented in the methods were the authors describe the mooring setting. Also, the depth of each sensor is hard to see (need a thicker line).

Reply: We agree with the reviewer and we included in the revised manuscript descriptive information about the temperature sensors we used, within the part 3.2 "Hydrological time-series and profiles"; in Fig. 4 we added a thicker line for each sensor depth.

4) **Reviewer #1:**

Please revise the English. There are some recurring typos throughout the manuscript (e.g. replace "particles fluxes" with "particle fluxes" or "particulate fluxes").

Reply: The reviewer is right we replaced "particles fluxes" with "particle fluxes"

Detailed reply to Prof. L. Langone comments

1) **Reviewer #2:**

Then, authors spend more to compare previous papers/dataset with respect to discuss their data. The paragraph 5.1 Comparison with other NW Med oceanic sites carries no novel considerations. In summary, this manuscript could become a reference paper if the Discussion chapter will be improved (re-organized and re-written), looking for to better outline the new findings of this study and increasing the integration between different kind of data.

Reply:

The reviewer is right, see response to reviewer #1, comment 1)

2) **Reviewer #2:**

It was not explained why were chosen only 3 of 5 sites for sediment analyses.

Reply:

The cores where taken in April 2009, due to time limitation and bad weather conditions we only recovered three cores at 3 sites instead of 4 (SC2160 site was planned, but the multicorer was empty at the sea surface). The data from the HC2300 mooring site (consisting only of hydrological and hydrodynamical time series) was included later on during the writing up the manuscript to extend the region under study, and was thus not sampled.

3) **Reviewer #2:**

Fig. 1– CLD1000 is not explained in the caption, neither in the text. Is it part of your experiment or it is just used as reference? Cited also in Fig. 2, and Fig. 5. In the text, it is cited in a different way (LDC1000) in paragraph 4.2 (row 293), but it is not explained if data belong this manuscript or to other authors.

Reply:

The reviewer is right we added further explanation in the revised manuscript within the Material and Methods section 3.1 “mooring lines”, in this paper, we only use potential temperature data concomitant with our studied period for discriminating the dense shelf cascading events. We have homogenized the citation LDC1000 for the Lacaze-Duthiers mooring site in all the text and in Fig. 5.

4) **Reviewer #2:**

The sedimentation rates of table 3 are (too) high for slope sediments. I suspect that some mixing is responsible for this. It should be useful to show the Pb-210 profiles.

Reply:

We may give the original ^{210}Pb data that was used to calculate the sedimentation rates in a web appendix but this is not the main focus of this study. No mixing was observed in the cores, and a steady decrease of $^{210}Pb_{xs}$ was observed in the 10-12 cm upper part of the cores to reach the supported ^{210}Pb allowing a good assessment of the sedimentation rates following the CFCS model. Even if the result seems too high to the reviewer (especially for the SC2240 site, 0.22cm.yr^{-1}) we are certain that the ^{210}Pb was accurately measured.

5) **Reviewer #2:**

Fig. 4 - Too many instrumented levels with respect to what explained in the Materials and Methods, especially in the third deployment period! Temperature was measured at about 30 levels.

Reply:

We agree, see the response to reviewer #1, comment 3)

6) **Reviewer #2:**

Fig. 5 – a), b) and c) are not in the right order. In SW2060, there is an ADCP but no such data are shown. No salinity time-series is shown, although in the Methods, SBE37s are described.

Reply:

The reviewer is right; the Fig. 5 caption is modified in the revised manuscript. In Fig. 5. we wanted to compare potential temperature on the slope (LDC1000) and in the deep basin (SC2350 and HC2300) to show the absence of deep dense shelf water cascading during both winters and the bottom-reaching convection in winter 2009. Only these sites had temperature sensors with sufficient resolution and covered the entire study period (from September 2007 to March 2009). Salinity was only measured at SC2350 and HC2300, and description of salinity does not provide useful information for this part.

7) **Reviewer #2:**

For OOC, "low" is used as contrary of "deep", whereas "shallow" sounds better. You use the same for DSWC.

Reply:

The reviewer is right we replaced "low" by "shallow" to describe moderate DSWC or OOC events.

8) **Reviewer #2:**

12.9 is very similar to 13. Do you mean that the difference between the two years was the duration of low temperature? If yes you should write it in a clearer way. What about the salinity? Maybe the difference is there. (P12859, L8-10)

Reply:

A difference of 0.1°C in the open sea convection area in wintertime is important and directly acts on the depth of the mixed layer that is classically defined as a deviation of less than 0.1°C between the surface and the base of the mixed layer.

9) **Reviewer #2:**

For Palanques et al., 2009, the speed increased « during most of the deep cascading period ». Thus they did not refer the speed increase to OOC (P12863, L10-13)

Reply:

We agree with the reviewer, the authors did not refer the increase of near-bottom current velocity to OOC. However, we know that OOC and DSWC were exceptionally strong in winter 2004-05 (Puig et al., in press) we thus suggest that OOC could also have contributed to generate such current close to the bottom. The respective paragraph reads now "The authors associated the origin of the large TMF in winter 2004-05 primarily to exceptionally intense and persistent DSWC. However it was also observed close to the bottom exceptionally strong current velocities (up to $47 cm.s^{-1}$) and a temperature increase (approx. 0.1°C). Given that OOC was exceptionally strong during the winter 2004-05 (Puig et al., in press) the large TMF is rather originating from both deep DSWC and OOC events than from deep DSWC event only."

10) **Reviewer #2:**

Maybe it should be added that vertical mixing supplies nutrient to the surface from the deep waters. This triggers the algal bloom that follows the OOC. (P12865, L23)

Reply:

We agree with the reviewer and we added a comment about the supply of nutrients by vertical mixing into the Results, in section 4.1 "Spatial variability of Chl-a in the open-ocean convection zone": "(iv) a large increase in late winter/early spring associated to the large planktonic bloom that takes place when the surface layer restratifies and has been enriched in nutrients from deeper waters."

11) **Reviewer #2**

They did not say exactly this (P12866, L28 to P12867, L3)

Reply:

We agree with the reviewer: the authors did not hypothesize that the reduced availability of easily resuspendable sediment could have prevented the resuspension by strong currents. To be clearer we changed this part in the revised manuscript, the paragraph reads now "They considered that potential contribution of deep sediment resuspension by strong currents (between 20 and 47cm.s^{-1}) could have been also possible, but less intense due to the position of the trap farther from the seabed (250mab).Given the absence of turbid bottom layer in the years preceding this event (Puig et al., in press) the availability of easily resuspendable sediment may have been reduced."

12) **Reviewer #2:**

Need a citation. (P12868, L11-14)

Reply:

We agree with the reviewer we added two references (Marty and Chiavérini, 2010 and Martin et al., 2010).

13) **Reviewer #2:**

It does not seem what is written in the Discussion, where is pointed out that the recurrence of deep OOC could prevent present-day sedimentation inside giant scours where they undergo local acceleration, whereas the OOC currents, characterized by changing directions, cannot generate the observed bedforms. (P12871, L8-10)

Reply:

We agree with the reviewer; however sediment resuspension by recurrent deep OOC prevents sedimentation that can be considered as a "major driving force for deep sedimentary dynamics". The respective paragraph reads now: "The observations suggest that the recurrence of deep OOC in the area has a long-term effect on seabed morphology (i. e. by preventing particle sedimentation) and thus should be considered as a major driving force for deep sedimentary dynamics."

14) **Reviewer #2:**

Fig. 9. Refs to A, B, C and D part of figure are lacking. It is not explained which parameter is in color in the Pot temp vs S (I suspect the turbidity but it should be explicited).

Reply:

The reviewer is right we modified the Fig. 9. caption in the revised manuscript.

References

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