

## ***Interactive comment on “Settling particle fluxes across the continental margin of the Gulf of Lion: the role of dense shelf water cascading” by C. Pasqual et al.***

**C. Pasqual et al.**

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Received and published: 22 October 2009

We thank the effort of both reviewers in understanding our data value and encourage us to discuss it more accurately. We also thank specific comments that sure improve the presentation of the paper.

Answer on **Referee #1 comments**:

**Referee #1 is concerned on how to distinguish the flux values of organic matter, opal, CaCO<sub>3</sub> and siliciclasts contributed by pelagic biological production from those influenced by the dense shelf water cascading event. The reviewer also suggests the use of algorithms to estimate export flux from biological produc-**

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**tion.**

Heussner et al. (2006) reviewed this issue in the GoL area and differentiated the resuspended/laterally advected and the vertical settling components of the organic carbon fluxes using the general equation:

$$F_{TCorg} = F_{PCorg} + F_{RCorg}$$

where  $F_{TCorg}$  is the total flux of POC,  $F_{PCorg}$  is the pelagic POC flux, and  $F_{RCorg}$  is the resuspended/laterally advected POC flux. Heussner et al. (2006) calculated  $F_{PCorg}$  from the power function that relates POC export from surface waters and subsequent exponential decrease (Suess, 1980) using mean POC export estimates by Monaco et al. (1990, 1999). They found that lateral advected POC represents on an annual basis around 90% of the total flux of POC in the Lacaze-Duthiers canyon. However, with this kind of approach the seasonal and intra-annual variability of primary production, which is quite high in the area (Marty and Chiavérini, 2002), is not well reflected.

Seasonal variability could be assessed using algorithms developed to predict POC fluxes. Martin et al. (1987) developed an algorithm that is widely used for to calculate the OC flux:

$$F_{POC}(z) = EP(z/100)^b$$

where  $F_{POC}(z)$  is the POC flux at depth  $z$ ,  $EP$  is the export production ratio (the POC flux at the base of photic layer, at 100m) and  $b$  is a negative constant based on an open ocean sediment trap studies. Posterior models include the ecosystem functioning as variables in order to better estimate the link between primary production (estimated from satellite data) and  $EP$ . Indeed, Dunne et al. (2005) developed an empirical algorithm for particle export ratio using parameters observable by satellite (temperature and chlorophyll). The use of satellite data chl  $a$  in the Mediterranean Sea is limited because during a large part of the year there is a well-developed deep chlorophyll maximum (Estrada, 1993). Moreover, other factors triggering the primary production

besides the ones that are often used in the models must be considered in the Western Mediterranean such as Saharan dust inputs. This phenomenon is suggested to enhance primary production during oligotrophic conditions (Ridame et al., 2002) and particle fluxes from the euphotic zone (Zúñiga et al., 2007; Lee et al., 2009).

Therefore, we think that the separation of pelagic and resuspended (even from marine or terrestrial origin) organic carbon (or organic matter) fluxes using actual algorithms based on export flux or satellite data (chl *a* and water temperature) is poor accurate in our study site. In fact the use empirical approaches would not be adequate due both the high intra-annual variability of the primary production in the study area, and the limited use of satellite data chl *a*. That question should be approached by using specific indicators of resuspension. However, the combination of the different variables presented in this paper and the actual knowledge of the particle flux in the Western Mediterranean allows discern the signal of the pelagic flux in the lower canyon, canyon mouth and open slope station during “calmed” summer conditions.

[References not included in the manuscript:

Dunne, J. P., Armstrong, R. A., Gnanadesikan, A., and Sarmiento, J. L.: Empirical and mechanistic models for the particle export ratio, *Biogeochem. Cycles*, 19, GB4026, doi:10.1029/2004GB002390, 2005.

François, R., Honjo, S., Krishfield, R., and Manganini, S.: Factors controlling the flux of organic carbon to the bathypelagic zone of the ocean, *Global Biogeochem. Cy.*, 16(4), 1087, doi:10.1029/2001GB001722, 2002.

Monaco A., Durrieu de Madron X., Radakovitch O., Heussner S., Carbonne J. Origin and variability of downward biogeochemical fluxes on the Rhone continental margin (NW Mediterranean), *Deep Sea Res. Pt I*, 46(9), 1483-1511, 10.1016/S0967-0637(99)00014-X, 1999.

Suess, E.: Particulate organic carbon flux in the ocean-surface productivity and oxygen

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utilization, *Nature*, 288, 260-263, doi: 10.1038/288260a0, 1980.]

**The method applied in the trap deployment should be presented and evaluated in more detailed information, such as trapping efficiency and addition of preservative in trap bottles.**

We have added detailed information on the effectiveness of the sediment trap in trapping material, the problem of the swimmers and the conservation of settled material in the bottles filled with the poisoning solution. We also assess the problems that may affect our sediment traps in particular.

**Specific comments are listed below**

**Experimental design and data recovery. The authors should give an evaluation on the trapping efficiency for the readers to understand the uncertainty of their study. They did not estimate the trapping efficiency, but a paper (Yu et al., 2001) on the trapping efficiency of deep sediment trap should be included in the material and methods. Yu et al. 2001. Trapping efficiency of bottom-tethered sediment traps estimated from the intercepted fluxes of <sup>230</sup>Th and <sup>231</sup>Pa. *Deep-Sea Res. I*, 48, 865-889.**

We have added detailed information on the effectiveness of the sediment trap (new section 3.2)

**Sinking particles were collected by 12 collecting cups. Did the authors add any preservatives into the cup solutions prior to the deployment. Recent research has shown that solubilization of “settling particles” is a serious problem for under-estimating organic matter and other elements (N, P, silica, etc. samples were from 600-4000m) (Antia, 2005). If this is the case (without addition of any preservatives), the flux values will be significantly under-estimated because the traps have been deployed for more than several months.**

We referred to Heussner et al. (1990), where a complete explanation of the sediment

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traps deployments and samples treatment is explained. Explicit comments concerning the preservatives used has been added in the “experimental design and data recovery” part as the referee suggests. Effectiveness of that poison is explained in the new section 3.2: Sediment trap efficiency.

**Results p. 7904, line 15, “was high form” should read “was high from” line 24, “up to 80m s-1” should read “up to 80 cm s-1”**

Corrected

**Look at the data showing at Table 1, some of organic matter minimum values (OM, opal) are zero. As I mentioned early, the mooring sediment traps had been deployed for over five to six months each time. I am wondering that the “solubilization” phenomenon of organic matter might be very significant, particularly for cups collected at shallow traps (300 m) during early periods (i.e. Oct. to spring). Because particles mainly contain OM, opal, CaCO<sub>3</sub> and siliciclastic, one may wonder how one of the components is equal to zero. If the authors keep the solution of cups, they may measure dissolved organic carbon and compare to their original DOC concentration in the cups prior to deployment. The authors need to address this issue.**

0 values are due to units used, we have changed units from g to mg. In addition, we have added in the methods section the preservative used in the sediment trap cups (formalin).

**Discussion p.7911, The authors just described the impact of DSWC in the open slope and these similar reports have been published by Bethoux et al., 2002, Lopez-Jurado et al., 2005, Font et al., 2007 and Palanques et al. 2009. They should quantitatively estimate the POC flux caused by primary production in the open slope and compare the calculated data to their observed field data. That will give the readers a new insight for the influence of DSWC on organic matter flux in the open ocean.**

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All papers cited by the reviewer relate to the hydrological characteristics of DSWC. None of these papers give evidence on the impact of DSWC on particle fluxes and organic carbon transfer. Following the suggestion for the reviewer, we have improved the discussion section on pelagic vs. laterally advected POC.

**Conclusions I am not sure if this the first time to record particle fluxes during a DSWC event. The authors should check it carefully because the authors have mentioned several papers talking about DSWC events in similar region (Palanques et al., 2009; Sanchez-Vidal et al., 2009).**

We eliminate the words “for first time” in order not confuse lectors.

**Reference P7919, line 10, “dense shelf-water” should read “dense shelf-water”**

Corrected

**Table 1, list all of the TM and main components data (every 15-day), not only the Max, Min, and Mean values. This is important information and needs to be explained. Also, why there are so many “o” values in Table 1.**

We present all values in graphs. A table with all TM and components data has more than 250 rows x 8 columns. If the editor considers that this is necessary, we can add this it as a supplement. Concerning the 0 values and as said in previous comments we have changed the units from g to mg to avoid this loss of information.

**Fig. 2, the y axis “(x10<sup>3</sup> m-3 s-1)” should read ““(x10<sup>3</sup> m<sup>3</sup> s-1)”**

Corrected.

**Fig. 3, “Current speed (m s-1)” should read “Current speed (cm s-1)”**

Corrected.

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Answer on **Referee #2 comments:**

**I reviewed the manuscript by Pasqual et al on cascading in the Gulf of Lions. I believe the paper merits publication in Biogeosciences after relatively easy issues are revised. This work adds to a number of recently published works by some of the authors, who are building an extensive, very informative history in this area of the Mediterranean. This may be a reference for other studies elsewhere, on top of actually providing a number of clues for understanding this region of the Mediterranean. I list here a number of aspects the authors may want to revise for the final version of the paper.**

**1. Title: Not entirely sure it explains what the authors discuss in the manuscript, especially in terms of composition of the particles.**

We have changed the title following the suggestion of the reviewer

“Flux and composition of settling particles across the southwestern Gulf of Lion’s continental margin: The role of dense shelf water cascading”

**2. Introduction: I find it too generic and, having a long list of previous works on the subject in the same region, could be more focused. For instance, P7899L23-26: "important role" for what?; P7900L04: "quantitative and qualitative impact...": should precise the objectives, explain about the composition of what, which parameters, in terms of?**

We agree that introduction may seem too generic and unconnected due to the large amount of studies done in the study area. In this revised version we have focused the introduction on more specific ideas. In addition, the objectives have been concretized.

**3. P7900L22: export from where to where. I do realize this can be understood from the context, but here and elsewhere the language is too vague and the reader could be guided a little more.**

More specific sentences and words in order to guide the reader have been added.

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**4. Results: Suggestion; All the information is in the manuscript, either in Figs or Tables. However, the complexity of the system for those not familiar with the area of study may point to the need of trying to add a paragraph synthesizing the main issues/results**

Main issues and results are described in the abstract. We do think that adding a paragraph synthesizing the main issues/results would be redundant information. Moreover, following comment 6, listed below, we have simplified the description of the data.

**5. P7905L27: Presumably?**

We take out “and presumably CCC300” from the results part.

**6. P7906L10-20: The description of the data is fine enough, but this is an example of a passage a little difficult to follow. Any effort to simplify it would be appreciated.**

We have removed non discussed data in order to simplify the manuscript.

**7. P7908L12-25: several comments here: "material sedimented"? Please precise. Next the authors refer to "material settling": would it be "transported"?. Then the authors refer to "mass accumulated on the seabed by DSWC" twice in the paragraph. I am not sure I follow this and it is known as explained it here. Could data on this respect be provided here?**

We have unified the terms and re-wrote this paragraph in order not to confuse the reader.

**8. P7908L25-30: the sentence "the impact of..." is too vague, and the authors should justify how, in what terms and what type of ecosystems.**

We have changed the text following the comments of the reviewer.

**9. P7909L15-20: I am not sure I follow the argument here**

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We agree that the way that was presented was confusing. In the previous version we were comparing data from different sources (turbidity and sediment traps) without a clear discussion. But, as discussing between both kind of measurements is not the aim of this paper, we have restructured the paragraphs and deleted not well developed ideas.

**10. P7911 after 5.1.1: I miss commenting on the source; if large cascading occurred in the previous year, would that affect the amount and composition/quality of what is available the following year? Can the authors compare the data and tell?**

We agree that data is needed for to reinforce that idea but, by now, we do not dispose of that data, for that reason we have withdrawn that idea.

**11. P7912L2: which year?;**

Corrected.

**L15: “vertical settling”? Isn’t this too simple?**

We have improved the explanation of the pelagic fluxes in the section 5.2 of the manuscript.

**12. P7913. I don’t see that the statement on sedimentation is supported by the data (see above for previous comment on this). It does make sense, but data should be provided. Also, the discussion about OM export from the photic zone is somewhat based on pre-conceived schemes in that export from the PZ is transferred to depth by vertically settling particles. The authors are well aware this is not this simple, less so in this area. The way the subject is addressed here is not solid enough to allow the discussion in section 5.2, which could also be expanded. Also, it is not clear what is meant in L18 by “visual knowledge”.**

We absolutely agree with this comment, which has also suggested by the Referee#1. We have addressed the discussion of the data in a different way.

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**13. P7915, last sentence in the conclusions: isn’t this a little too generic?**

We have concretized it after the improvement of the 5.2 section.

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Interactive comment on Biogeosciences Discuss., 6, 7897, 2009.

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