

***Interactive comment on “Short-term changes in particulate fluxes measured by drifting sediment traps during end summer oligotrophic regime in the NW Mediterranean Sea” by J. C. Marty et al.***

**J. C. Marty et al.**

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Response to referees comments

Referee #1

1 The referee points out the fact that additional sources of variability of fluxes might have influenced the variability measured in our experiment. To assess this variability the referee suggests that it would have been ideally convenient to use two or more different traps to get a replication of fluxes. We certainly agree with this comment but, as the referee writes, "obvious technical and economical reasons" made it impossible during our cruise. Nevertheless, as suggested by the referee, a comment will be added in the text at the end of paragraph 2.1 acknowledging the lack of true replicates.

2 The referee indicates that a possible bias on the variability of fluxes is the splitting of samples. We use for splitting a well-trying procedure based on automatic splitter (Heussner et al., 1990). Unfortunately, the low quantity of material in traps has not made possible the replication of the analysis of the parameters on different aliquots. It is true that a part of uncertainty exists but the parallel evolution of parameters analysed on different aliquots (eg POC and pigments) indicates that the splitting is probably not an important factor of variability. A comment on splitting and reference to Heussner et al. (1990) will be added.

3 Another possible factor of variability proposed by the referee is the fact that due to trap drifting we could have recorded a spatial variability. We do not think that the trap drifting could register a noticeable spatial variability for at least two reasons:

-the use of drifting traps has for principal objective to locate the sampling in the same water mass during the experiment. In this sense, the drift of traps is on the reverse a better way to stay in the same system instead of using a sampling at the same geographical position.

-the strategy of DYNAPROC 2 cruise was to choose a homogenous sampling zone, and to test repetitively the homogeneity (weak horizontal advection) of the zone all along the cruise. This stability of the zone during the cruise is discussed in a companion paper of the same special issue (Andersen et al., 2008).

4 Linkage between the variability of fluxes and the wind forcing at the sea surface is found "inferential" by the referee. It is true that the wind forcing cannot be estimated through the manuscript since data are missing. We apologize and will add the spectrum of wind intensity during the cruise on figure 2.

5 Technical comments:

- Use of " evolution " referring to temporal changes.

We will check the text and correct

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- Figure 3: P data not readable

new Fig.3 will be redrawn with P fluxes multiplied by 5.

- Use of "quality" alone.

We will check the text and use "chemical quality".

- Comparison between lipids in trap material and in suspended material (in reference to partly unpublished data) to avoid.

We agree that referencing to unpublished data must be avoided. The reviewer 2 also points out that quantitative data should be given to support our comparison. In the revised version we will give the % increase of neutral (triglycerides and wax esters) lipids in suspended and dissolved lipids and the significance of the comparison between the first leg of the cruise (cycle 1) and the second leg of the cruise (cycle 3) (these two cycles are used because a most complete data set in the 0-1000 m water column was sampled). In addition we give the web address of our data base where lipid data used in this paper are in the public domain.

Referee #2

Responses to general comments.

- The referee indicate that the short term sedimentation cannot be said to be influenced by the community structure alone because we do not know enough about the structure and probably this is more a question of interaction between the various communities. Accordingly we have discussed in the paper of zooplankton grazing for example which is certainly an interaction between communities. We propose to track in the text the "community structure" to replace them by "communities structure and interactions".

-The ratio POC/PON is sometimes below the conventional Redfield ratio. The referee is right: most of the POC/PON data are above 6 but a limited number of them are below. We will change in the text the POC/PON ratios "always" higher to POC/PON

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ratios "generally" higher.

-Lack of quantitative data provided for suspended and dissolved pool of lipids. We agree that referencing to unpublished data must be avoided. In the revised version we will give the % increase of neutral (triglycerides and wax esters) lipids in suspended and dissolved lipids and the significance of the comparison between the first leg of the cruise (cycle 1) and the second leg of the cruise (cycle 3) (these two cycles are presented because the data set is most complete as the 0-1000 m water column was sampled). In addition we give the address of our data set that is in the public domain.

-Lack of some discussion for inconsistent variation patterns of bulk organic matter and compound classes

A detailed explanation has been given following the specific comment Page 591.

Specific comments:

Abstract: There are some repeated statements such as ". . . the settling material was in part degraded" and ". . . the organic matter . . . was reworked . . .". What is definition for "free lipid"?

The abstract will be corrected avoiding repetitions. Free lipids are the metabolites resulting from acyl lipid hydrolysis products. The term has been replaced by acyl lipid hydrolysis metabolite, which also fits better with information given in Fig. 8.

Introduction:

Some sentences are repeated. For example, the first sentence can be replaced by the sentence on lines 16-17.

The text will be modified as proposed

Page 578 lines 2-4: this sentence needs to be reorganized to make clear what or who "indicated" that.

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A new sentence will be written

Page 578 line 9: ". . . particles (not particle flux) were collected . . ."

Will be corrected

Page 578 lines 11-13: "C, N, and P in the trap samples were measured not measured in the traps".

Will be corrected

Materials and methods:

Page 580 line 4: "inorganic forms of C, N, and P . . .". What are these forms?

The inorganic forms of C, N, P, are respectively CO<sub>2</sub>, nitrate, orthophosphate. This will be detailed in the text.

Page 580 lines 12-14: It is unclear how the internal standard can be used to correct the presence of residual water in the filter.

The residual water on the filter after filtration of sample changes the volume of solvent during extraction. Adding the standard in known amount at this stage allows the quantitative calculation taking in account this change of volume. We will delete the end of the sentence which appears confusing and unnecessarily over informative.

Page 581 lines 20-22: ". . . the standard deviation of replicate analysis . . . was 6<sup>4</sup>%". What is number 6 here?

The sentence has been changed to: "Under these conditions, the relative standard deviation of replicate analysis (n = 3) of the same sample spotted on adjacent rods for latroscan TLC-FID analysis was 2 to 10 %".

Results:

Page 582 lines 7-10: "two principle wind events. . .". How long did these wind events last?

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The registration of wind speed during the cruise will be added on Figure 2.

Page 582 line 11: "desalt water (<38.3)" should be "low salinity water". What is the normal salinity in the sampling site?

We will change desalted water to low salinity water and delete the reference to 38.3 psu. The low salinity water mass is with respect to overlying water mass and to the salinity before and after the intrusion of this water mass (refer to Andersen et al. 2008).

Page 584 line23: "phaeopigment fluxes" include what pigment compounds (phaeophytin, phaeophorbide, and others?)

Phaeopigments include phaeophorbides and phaeophytins. This has will be detailed in the text.

Page 586 line 4: "metabolites" what is the definition? What lipid compounds are in this term?

Metabolites are the sum of free fatty acids, alcohols, diglycerides and monoglycerides. It is now written in the text.

Discussion:

Page 587 line 9: "dissolved di-nitrogen" should be "dissolved N<sub>2</sub>".

Will be corrected

Page 589 lines 4-10: ". . . correlated with the flux . . . (0.42, n=74), . . . (0.49), . . . (0.55), . . . (0.56), and . . . (0.58). What do these numbers mean? Are they slopes for the correlation lines or r<sup>2</sup>?

Those numbers are (r<sup>2</sup>). This will be written in the revised version. By checking r<sup>2</sup> again, we found that the correlation between MGDG and the pigment "Peri" was an error. This has been removed from the text.

Page 589 line 12: "global" should be deleted.

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Will be deleted

Page 591 last paragraph: total OC (Fig 3) and total lipids (Fig.7A) showed different day/night patterns. What is the reason for the difference?

There is a misunderstanding concerning this comment. We hypothesize that the day night variation is also the subject of the general comment "inconsistent variation patterns of bulk organic matter and compound classes" since POC, total lipids and phaeopigments fluxes have similar general variations (cf page 584 line 25 and page 585 line 12). On the day night scale we agree that the patterns of POC and phaeopigments and lipids are different. In Page 591 is reported that "This diel periodicity of POC fluxes could be the result of the diel fluctuation in zooplankton grazing rate as evidenced by Welschmeyer et al. (1984) or Dagg et al. (1989) even if phaeopigment fluxes do not follow the same rhythm". A possible explanation of this discrepancy between OC and phaeopigments variations may be the fact that phaeopigments are produced by micro and meso zooplankton with different sinking rates and could also be modified by photooxidation due to their long residence time in surface waters in stratified conditions, leading to colourless products (eg Cuny et al. 2002, DSR II, 49) which are included in the OC measurement but not in phaeopigments. Total lipids included hydrocarbons that may be of anthropogenic origin compared to total lipids not including hydrocarbons (LT-HC). The percentage of hydrocarbons was a high percentage of total lipids (22%+/- 15), indicating both natural and anthropogenic inputs (Goutx, 1988). POC represented a much higher mass than lipids in the total flux and was mainly reflecting natural material. Hence anthropogenic contributions may have hindered the day/night pattern of natural compounds resulting in the mismatch between daily POC and total lipid fluxes as shown by a weak regression coefficient  $r^2 = 0.46$  ( $n=73$ ) between total lipids and hydrocarbons. Contrary to hydrocarbons and supporting our conclusion, natural lipids, i. e. lipids not including hydrocarbons (LT-HC) and glycolipids (MGDG, membrane lipids in microalgal cells) exhibited a strong regression correlation ( $r^2=0.65$  and  $0.57$ , respectively,  $n=73$ ) with POC. Co-variations of POC flux and MGDG lipid flux

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were clearly noticeable during the period 26 Sept.-5 Oct. (Fig. 3 and Fig. 7F). Since we have indicated in our conclusion that "In general the day night periodicity of organic fluxes was apparent but of weak amplitude", we have given only a limited discussion on the day/night variations of fluxes. We will give in the revised version the explanation concerning the discrepancy between OC and Phaeopigments.

Conclusions:

The first sentence should be a general statement including all factors affecting the short-term particle fluxes.

A first introductory sentence will be added.

Table1: The details for tracer experiments (methods) should be mentioned.

The methods used for tracer experiments have been described in Marty et al. (2008) for the same cruise. A reference to this paper will be introduced in the legend of Table 1. For the unit of ( $\text{mg m}^2\text{j}^{-1}$ ), what does j mean?

Sorry, the correct unit is  $\text{mg m}^2\text{d}^{-1}$

Fig. 2: The connected curves between sampling periods should be removed because no data showing the variation trends during these days.

Will be corrected

Fig. 3: The title label for nitrogen flux should be consistent to that for carbon flux.

Will be corrected

Fig. 4a: The title label should be "POC/PON ratio".

Will be corrected

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Interactive comment on Biogeosciences Discuss., 6, 575, 2009.

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