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Biogeosciences Discussions

# *Interactive comment on* "The impact of Saharan dust on the particulate export in the water column of the North Western Mediterranean Sea" *by* E. Ternon et al.

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We would like to warmly thank both reviewers for their constructive comments on the manuscript. In particular, the result and the discussion parts are now two different sections; a new figure and new references were added.

Reviewer 1 Following most of the comments, we significantly change the presentation of some of the data: data from sediment traps at 1000m are now being used to support some of our hypothesis and an additional figure is proposed to illustrate the role of hydrological processes in material export in winter. Below are the detailed responses to reviewer's comments.



"Why weren't 1000m trap results also reported?" The first version of this manuscript was focused on the 200 m depth because our main goal was to relate observations of a potential increase in primary production following atmospheric deposition and subsequent marine export from the surface mixed layer. But we totally agree that 1000 m traps data set being available, its use is helpful in some parts. In particular the 1000 m trap data set is now used: (1) to help the comparison between our data and Lee et al. 2009 (section 3.3.1) and (2) to emphasize the role of winter mixing in exporting rapidly material to the deep water (section 4.1 and Fig. 4).

"I would be inclined to focus more of the figures and discussion in section 3.4 to the relationship between atmospheric lithogenic flux and marine POC flux . . ." Two cases enable a direct comparison between atmospheric lithogenic flux and marine POC flux: the extreme Saharan event of February 2004 and the "fertilising" mixed Saharan event of August 2005. A small section was added to the new version in order to insist on the relationship between high atmospheric deposition and high POC flux (section 4.4). Correlations between high marine POC and lithogenic fluxes ('lithogenic events') were also observed in the absence of direct atmospheric deposition. Those cases are described accordingly. Concerning the possibility of a bias due to Al scavenging: see specific comment below.

"p. 10739, Line 9: inappropriate references" Those were changed with Hamm, 2002; Passow and De la Rocha, 2006 and Ploug et al., 2008.

"p. 10740, Line 17: please provide a short description . . ." The paragraph on atmospheric sampling was completely rewritten in order to make this point clearer.

"p. 10746, lines 11-19: the 2x discrepancy between results from this study and a parallel study from Lee et al. 2009 . . ." This is indeed one of the two cases where the 1000 m traps data set can be used. The $\sim$2 factor discrepancy observed at 200 m is also observed at 1000 m, as explained in the new version (section 3.3.1), which supports the sampling bias hypothesis for one or the other sampling devices, or both.

“p. 10748, lines 25-end: this paragraph is confusing” A more comprehensive paragraph with separated phrases was written in the new version (beginning of section 4) and the statistical test was removed as it was useless for the purpose.

A global answer was made for the three following comments, based on 1000 m sediment trap data. “Section 3.4.1: you should already be able to narrow down somewhat the four hypotheses . . .” “p.10749, line 23: re. Vertical convection . . .” “Earlier (p. 10745): What about the 1000m sediment traps?” To answer those helpful comments and thus improve the discussion section, a new figure (Fig.4) was added, presenting both total marine fluxes at 200 and 1000m along with the depth of the mixed layer for the 3 lithogenic events that occurred in winter. For each case, the increase of the marine total mass flux coincides with the deepening of the mixed layer. The increased fluxes at 200 m is also observed at 1000 m while the mixed layer ‘only’ deepened to 150, 400 and 600 m respectively for the years 2003, 2004 and 2005. Physical mixing is believed to trigger off the aggregation of in situ material, by particles collision (Jackson, 2008), rather than carrying particles to the deeper layers. Aggregates formed in the deeper mixed layer would accelerate particles removal by a massive and rapid sedimentation to the deep (as shown by the concurrent peaks at 200 and 1000 m depth). To explain that process and in addition to Fig.4, a short section was added (end of section 4.1).

“p. 10750, lines 6-11: re. scavenged dissolved Al” We are aware that high dissolved aluminium in the Mediterranean (~75 nmol.L-1 in average, Han et al., 2008) could potentially induce high aluminium scavenging onto particles (confirmed by recent measurements in Lee et al., 2009). This high scavenging potential would indeed lead to a non negligible over-estimation of lithogenic fluxes. This would be particularly true when fluxes and settling velocity are very low. A small section in the new version was added (end of section 4.1) to insist on the fact that lithogenic fluxes could be over-estimated, during low fluxes periods. Titanium is often used as a tracer of lithogenic matter, but unfortunately was not measured for this time-series. However, titanium con-



tent in Saharan dust depends on the source area (0.2 – 0.8 % of the total dust mass, computed from different references) which reflects different Al / Ti in different types of crustal components (from 9.8 for basalt to 33.5 for granite – Taylor, 1964). Al / Ti cannot be considered as constant in the lithogenic material in the Mediterranean Sea, for example it varies from 4.2 to 30.4 for Saharan aerosols collected in the North western Mediterranean (Corsica, computed from data of Bergametti, 1987).

Taylor R.S. (1964). Abundance of chemical elements in the continental crust. Geochim. Cosmochim. Acta, 28, 1280-1281. Bergametti G. (1987). Apports de matière par voie atmosphérique à la Méditerranée Occidentale. Aspects géochimiques et météorologiques. Thèse Université Paris VII, 296 p.

“p. 10750, lines 12-16: this summary paragraph is at this point speculation” This paragraph was removed.

“p. 10751, lines 24-25: large flux event in Dec 2003-Jan 2004 have already removed any older lithogenic particles that had accumulated in the surface layer during stratification?” Thank you for that very pertinent comment. As already discussed above, as confirmed by the fluxes numbers, the marine lithogenic event that rapidly followed the extreme Saharan event can totally be explained by the introduction of those ‘new’ lithogenic atmospheric particles. A small section was added in the new version (section 4.3.1).

“p. 10752, lines 14-20: specify what year you are referring to.” Thank you for detecting this omission. This has been added (title of section 4.3.2).

“p.10754, lines 1-3: Perhaps the main effect of dust “fertilisation” is not to increase total chl, but to shift the community to one that is more prone to export.” Volpe et al. (2009) concluded from satellite measurements that Saharan deposition does not trigger a chlorophyll increase in the marine surface layer and thus conclude that Saharan dust does not play a significant role in the sustainment of primary production in the Mediterranean Sea. However, recent on deck-incubations experiments showed that despite the quite small chlorophyll a increase induced by aerosols additions, new primary production increased significantly (Bonnet et al., 2005): new production and the number of larger cells increased in amended bottles compared to unamended ones. So, indeed a shift in the community is most probable: the corresponding paragraph in the new version was changed accordingly (end of section 4.3.2).

"Section 3.4.3: "Was the June 2006 dust event dry (unaccompanied by rain)? Ie, is a wet/mixed event necessary for a marine flux response to dust deposition?" Dust deposition in June 2006 occurred during a sirocco wind period and was almost dry (less than 0.1 mm of rain). This "pure Saharan event" was characterized by low inorganic nitrogen content, necessary to induce a phytoplankton fertilisation (N – P co-limitation, Bonnet et al., 2005). However, this Saharan event was sufficient to trigger a bacterial response by providing phosphorus (Pulido-Villena et al., 2008). Atmospheric mixed events are often characterized by a larger amount of rain (encounter of European cold polluted and warm Saharan air masses) and higher nitrogen content. The corresponding section (4.5) was rewritten to make our purpose clearer.

"p. 10755, lines 3-7: inappropriate references" This has been changed, as requested.

"Fig 2 legend: please specify in the figure legend what the dots and the blue lines are" This is now specified.

"p. 10745, line 4: "It is also then found:" specify what "It" is" Sentence has been changed accordingly.

"Fig 4 legend: specify if error bars are as in Fig 3; specify what "biogenic fluxes" are" This is now specified. Error bars represent the standard deviation of the mean monthly values. Biogenic fluxes are the sum of ($CaCO_3$ + Opal + OM) fluxes.

"p. 10749, line 15: should be ": (40% and 80% in 2003 and 2005, respectively)"" This has been changed, as requested.

"Reference to Figure 6 occurs before Figure 5 in the text" Numbering has been checked



throughout the text.

Reviewer 2 Following the suggestions of the reviewer, we changed some sequences of the manuscript framework. The 'Method section' was simplified and the 'Result' section was separated from the 'Discussion' one, for a better understanding. Below are the detailed responses to individual reviewer's comments.

"Title: The title, while attractive does not reflect the entire paper." Principal data sets are from marine and atmospheric fluxes measurements and their comparison was the initial aim of this study. As noticed by the reviewer, this paper deals with "lithogenic events" which are triggered either by Saharan deposition or winter convection. However, one of our hypotheses is that those events require lithogenic particles which main source is atmospheric in that part of the Mediterranean. We didn't find a more appropriate title. "2.1. Atmospheric sampling is short" The paragraph on atmospheric sampling was completely rewritten and complementary information were added to table 1.

"2.2.1 and 2.2.3 These two points are confused" Both sections have been grouped accordingly and a new title was given to that section.

"Paragraphs Results and Discussion: I personally think that would be better to separate them." We agree with that pertinent comment. Results and discussion have been separated in the new version with paragraphs 3.1, 3.2 and 3.3.1 taking part of the 'Results' section and further paragraphs in the 'Discussion' section. Titles numbering of both 'Results' and 'Discussion' parts were changed accordingly.

"3.1 Atmospheric flux: Have you seen any difference in the type of particle (mass flow, composition, size...) and the ecosystem response between the two types of events?" Dry "pure" events are very rare. Dust falls which occur with only a few drops are usually reported as dry events in spite of the fact that, in these cases, normal gravity settling of dry aerosols is not the operating mechanism. It is difficult to distinguish "pure" dry events, and so to mark them in Fig.2. However from our experience in the Mediterranean area, there is no significant difference in Saharan dust composition and size between dry ("pure" and "false") and wet events. The main difference between dust events regarding composition and especially nutrient content is related to the mixing of desertic dust with anthropogenic components.

"Figure 2: Change one of the scales of graph. Units of fluxes mg.m-2.d-1?" Symbols for the atmospheric fluxes have been lightened in the new version in order to make a clearer figure. The axis legends were also changed in order to better distinguish the marine particulate flux from the atmospheric flux. Both atmospheric and marine fluxes were expressed in mg.m-2 and not in mg.m-2.d-1 because atmospheric dust deposition often occurs within a few hours up to 1-2 days and is sampled on a variable time basis for the year 2003 and 2005. This precludes the use of the mg.m-2.d-1 unit for atmospheric deposition and thus for that figure.

"Figure 3. Change the figure caption. The mean mass flows of atmospheric deposition are not climatology." This has been modified.

"3.2 Hydrological and biological: It would be interesting to put some figure of T and / or S in the two periods identified" We partly agree with that comment. The reviewer suggests a representation of the mixed layer, characterizing stratified and unstratified periods, with a T or a S plot. However, the mixed layer has been estimated from monthly CTD casts using the criteria that the density gradient between the surface and the base of the mixed layer is 0.05 (Lévy et al., 1998). Throughout this study, density data set was used to estimate the mixed layer depth at all seasons. Stratified periods are characterized by a constant and a strong stratification (less than 10 m, D'Ortenzio et al., 2005) over the summer months. This very important stratification is difficult to be estimated from the Lévy et al. (1998) formula as it is too approximate. Thus no representation of the mixed layer was performed for that period. Nevertheless, the absence of any strong winds throughout the 4 summer periods covered by this study, leads us to estimate the mixed layer as deep as $\sim$10 m. We rather focused on unstratified periods or destratification periods when the mixed layer deepening process is believed to trigger off the aggregation of 'in situ' material, by particles collision (Jackson, 2008),



leading to lithogenic events. This notion was developed in a new paragraph (end of section 4.1) accompanied by a new figure (Fig.4) for the concerned years (2003, 2004 and 2005).

"p10745, line 13: "According to pigment…." This phrase, I guess it refers to the graph 4.i, can you explain more deeply?" Pigment data were from in situ measurements in the water column during monthly cruises at the DYFAMED site (from DYFAMED and BOUSSOLE time series) and data (available on respective web sites) are not shown in our study. Pigments data were only considered punctually in order to determine the dominant biological communities at different periods of the studied years. A sentence was added to the text (section 3.2) as well as an indication in the Figure 5 caption in order to make that point clearer.

"At the figure caption of Figure 4, we have the figures 4.i, the 4.ii, and 4.iii Figure 4. Change the figure caption. This is not climatology." This was fixed: indeed 4.i is 4.a, 4.ii is 4.b and 4.iii is 4.c. This was changed in the Figure 4 caption, being now Figure 5. The word "climatology" was also removed.

"p10745, line 20: 1228??!! Vs figure 2. Fig. 4b or 4.ii?" This comment was interpreted as a surprise reaction from the reviewer in regard to the high value, which is comprehensive as such a total mass flux (1228 mg.m-2.d-1) was never reported before in that area neither in other open-sea areas of the Mediterranean. This flux corresponds to the export induced by the February 2004 Saharan event: 11838 mg.m-2 of lithogenic material were collected in the corresponding trap (11838 / 14 days = 846 mg.m-2.d-1 in 14 days). For that trap, the lithogenic material represented over 70 % of the total mass flux, which leads to a total mass flux of $\sim$ 1228 mg.m-2.d-1. This high total mass flux value was also reported in Table 2, where a range over the 4-years studied is presented (5 - 1228 mg. m-2. d-1). An indication about that high mass flux was added to the caption of Fig.4b (5b in the revised version) in order to make our purpose clearer.

"p10746, line 21: Fig. 4b or 4.ii?" The figure caption was changed accordingly.

“p10746, line 25: “. . . maximum of the total mass flux occurs before the peak of the bloom.” This observation is important, the authors show the presence of a peak in the mass flow and the flow of organic matter (p10748, line 11) before the spring bloom.” We agree with the reviewer that this observation deserves a larger discussion. However, this sentence currently being in the “Results” part, this discussion has been replaced later on, at the beginning of the discussion part. A new paragraph dealing with the different cases of high flux of organic matter in regard to the driving processes (bloom or lithogenic events) was added (section 4.4).

“Personally, watching the figures, 2, 4 and 5, I consider that these maximum mass flow and organic matter, may be due to processes of un-stratification of the water column and the subsequent inflow of nutrients” Section 4.4 dealing with high concurrent POC and lithogenic fluxes have been profoundly modified in particular to emphasis the role of Saharan extreme events regarding the transfer of organic matter to the deeper layers: “Such unpredictable Saharan events can play an important role in carbon export at the yearly scale”. In order to go further on the role of the quality of organic matter, more data would be necessary and so far only hypothesis can be made: “Organic matter quality may thus play an important role in the occurrence of ‘lithogenic events’ but was unfortunately not measured throughout this time-series”.

“3.4.1 Winter high marine lithogenic events See Fabres et al. (2002) for a similar study” The article suggested by the reviewer deals with the temporal evolution of the marine particulate mass flux in the Alboran Sea throughout the years 1997 - 1998, with sites highly influenced by inputs from rivers and margins. Very high autumn-winter mass fluxes were also observed in that area and were related to river sediment discharges. The geographical and hydrological configurations of the Alboran Sea sites and the DYFAMED site are very different. For instance, the Ligurian Sea (DYFAMED site) constitutes one of the Mediterranean convection zones whereas the Alboran Sea (Fabres et al., sites) does not. To our point of view, those differences make difficult any comparison of the processes driving the high winter mass fluxes observed in both areas.



“3.4.2 and 3.4.3 The cases of Saharan dust events. In these cases, to know certain characteristics of the aeolien material (size, composition ...), give many more possibilities for discussion and relation to other processes.” Unfortunately a detailed analysis of Saharan dust grain size and composition is not available for the four studied years. However the chemical and mineralogical composition of Saharan dust is rather homogenous (Guieu et al., 2002), especially if one single Mediterranean basin is considered as it is the case here. The main differences are on the calcium and calcium/magnesium carbonates content which depends on the source zones. The other differences are of second order and are not suspected to have a significant influence on biogeochemical processes.

Guieu, C., Loÿe-Pilot, M.D., Ridame, C., Thomas, C.,: Chemical characterization of the Saharan dust end-member: Some biogeochemical implications for the western Mediterranean Sea. Journal of Geophysical research 107 (NO. D15, 10.1029/2001JD000582,), 2002.

“3.5 Lithogenic particles and organic carbon export Paragraph 3.5, is a mixture of discussion and conclusion.” As suggested, section 3.5 (being newly 4.4), was modified and some parts of that section were included in preceding and/or following sections. This new section focused more on the “lithogenic events” formation and their potential in transferring organic matter to the deeper layers, whatever the triggering process (dust event or winter mixing).

“p10755, line 3: Remove recently, these papers are nothing new in this field.” That sentence was modified.

“p10755, line 16: To see Honjo (1996) and, Boyd and Trull (2007)” The Honjo reference is one difficult to access for many (including us) and was not cited. However, the Boyd and Trull reference was cited accordingly in the section 4.4.

“p10756, line 18: 0.8 is the 40% of 2.” The error comes from calculation approximations. The POC exported (0.8 g.m-2) is actually 45 % of the total annual POC that year

(1.78 g.m-2). This number has been modified accordingly in the new version of the manuscript.

“p10757, line 20 to 25: is this paragraph necessary?” This paragraph was rewritten as requested

“Fig. 1: Add the rivers mentioned in the paper and the ocean circulation in the study area.” Figure 1 has been modified accordingly.

“References: Add the following references” The older reference (Zuniga et al., 2007) was replaced by Zuniga et al., (2008) on several occasions.

Figure 4: Temporal evolution of the mixed layer (ML) depth (black dashed lines) and marine lithogenic fluxes (mg.m-2.d-1) at 200 (blue lines) and 1000 (green lines) meters depth at the DYFAMED site for the years of high marine lithogenic fluxes during winter mixing (2003, 2004 and 2005). For the year 2003, lithogenic flux being not available before the 13th of March, the total mass flux (dotted lines) was also plotted. The mixed layer was estimated from CTD data from the monthly DYFAMED cruises using the criteria that the density gradient between the surface and the base of the mixed layer is 0.05 (Lévy et al., 1998).

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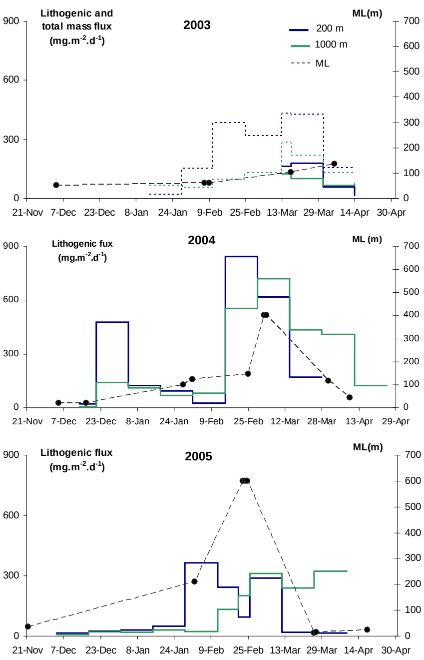


**Fig. 1.** Figure 4_see complete caption at the end of the review