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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.

Anonymous Referee #1

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Summary: The authors examine relationships between time series of atmospheric lithogenic flux and marine lithogenic and biogenic flux data. Like previous studies, they find that biogenic mass flux at 200m sediment traps generally do not follow the seasonal surface chlorophyll maximum, but rather precede it. They further find that high marine POC flux events are generally associated with high marine lithogenic flux, but not necessarily with high atmospheric lithogenic flux. Indeed, they find that marine lithogenic flux is not directly related to atmospheric lithogenic flux, but rather, it is modulated by hydrographical and biological conditions in the euphotic zone. They identify and discuss 3 cases of atmospheric vs. marine lithogenic flux relationships: i) low atmospheric, high marine (winter); ii) high atmospheric, high marine; iii) high at-

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mospheric, low marine. This study illustrates the complexities and subtleties regarding factors affecting marine particle flux.

General comments: The authors focus only on the 200m trap results. Why weren't 1000m trap results also reported? Several issues that came up could be addressed by looking at the 1000m trap results (eg. potential undercollection relative to Lee et al. 2009 traps; vertical convection processes explaining high 200m flux in late winter, p.10749); section 3.4.2 (already mentioned but data not shown). Fig 7 nicely shows that high marine lithogenic flux events are correlated with high POC flux events. 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 rather than marine lithogenic flux, since the response of POC flux to dust is what is most compelling. An additional advantage to recasting the discussion to focus more on POC is that the possibility that the marine lithogenic flux could actually be dissolved Al that is scavenged onto biogenic particles no longer becomes an important caveat to the discussion.

Specific comments: p. 10739, Line 9: inappropriate references: neither Armstrong 2002 nor Francois 2002 discuss aggregation as a process by which lithogenic particles influence export.

p. 10740, Line 17: please provide a short description of how the contribution of Saharan dust was estimated, as the reference cited (Loye-Pilot and Martin, 1996) is one that may be difficult for many (including me) to access.

p. 10746, lines 11-19: the authors suggest that the 2x discrepancy between results from this study and a parallel study from Lee et al. 2009 during times of high flux is due to high analytical errors by Lee et al. due to their low sample size. However, there was good agreement during times of low flux, when analytical errors would be most important. This suggests one or more of many other possible causes: spatial patchiness; quality of the material during high flux events may be leading to under or over collection by the PPS 5 or Lee system, respectively; etc. Was the temporal

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trend the same between the Lee et al dataset and this one? What about between the 200m and 1000m sediment traps? The 1000m sediment trap should be less prone to collection biases.

p. 10748, lines 25-end: this paragraph is confusing. Asynchronous fluxes and seasonality in fluxes focuses on the timing of events, whereas the t-test tests for differences in the mean of atmospheric vs marine fluxes. These two ideas do not follow from one another. Either the t-test is an inappropriate test to use here, or it belongs somewhere else in the text.

Section 3.4.1: it seems that you should already be able to narrow down somewhat the four hypotheses proposed for the winter high marine lithogenic events with data at hand. p.10749, line 23, re. vertical convection: are there data on the mixed layer depth during these two events (Feb 2003, Feb 2005) to confirm or refute the potential for homogenization of the upper 250m? Earlier (p. 10745), you imply that there were deep mixing events in 2004 and 2005, but not 2003. What about the 1000m sediment traps? If the enhanced flux were due to homogenization only, you might not expect concurrent (or near concurrent) peaks in the 1000m traps. p. 10750, lines 6-11, re. scavenged dissolved Al: scavenged Al onto opal is a very real possibility. New data from the US GEOTRACES intercalibration experiment confirm that dissolved Al has very high affinity for sorbing onto silica, and this would lead to an overprediction of lithogenic material. For this reason particulate titanium may be a better element from which to determine lithogenic material. Was Ti measured by ICP-AES? p. 10750, lines 12-16: this summary paragraph is at this point speculation, as there are no data from the current study to confirm or refute the hypotheses presented, and should therefore be presented as speculation or removed.

Section 3.4.2: P. 10751, lines 24-25: if lithogenic flux in late winter as described in section 3.4.1 is from removal of accumulated lithogenic particles in the surface during stratification, wouldn't the relatively large flux event in Dec 2003-Jan 2004 have already removed any older lithogenic particles that had accumulated in the surface layer during

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stratification? (and therefore assume that the Feb 2004 event is mostly "new" lithogenic material) p. 10752, lines 14-20: specify what year you are referring to p.10754, lines 1-3: The current reference to the Volpe et al 2009 paper is currently not obvious unless one already knows what the Volpe paper is about. The observed increase in POC flux following an atmospheric flux event, despite no increase in satellite-derived chlorophyll, is an interesting point, particularly in contrast to the Volpe 2009 paper, and this discussion deserves to be expanded. 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.

Section 3.4.3: hypotheses i) and ii) seem like they should also apply to the August 2005 case discussed in 3.4.2, where there was a lithogenic and biogenic export response, and therefore cannot, by themselves, explain the lack of a response. 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?

p. 10755, lines 3-7: inappropriate references: while Armstrong et al. 2002, Francois et al. 2002, and Klaas and Archer 2002 all discuss the relationship between ballast mineral and POC flux, it is important to note that their conclusions applied only to flux from >1000m traps, where "free/excess" POC is minimal. None of them refer specifically to ballast minerals acting as glue, although it's true that that's how these papers were often interpreted. At 200m, most of the POC flux is "free/excess". More appropriate references would be those papers that report on roller-tank aggregation experiments (including Passow and De La Rocha, but also Engel et al. 2009, DSR2, Hamm 2002), which better simulate the proportion of POC to ballast mineral that would be found in the euphotic zone.

Technical corrections: Fig 2 legend: please specify in the figure legend what the dots and the blue lines are

p. 10745, line 4: "It is also then found. . .": specify what "It" is

Fig 4 legend: specify if error bars are as in Fig 3; specify what "biogenic fluxes" are

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p. 10749, line 15: should be "...(40% and 80% in 2003 and 2005, respectively)"

Reference to Figure 6 occurs before Figure 5 in the text

Interactive comment on Biogeosciences Discuss., 6, 10737, 2009.