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Biogeosciences Discussions

Interactive comment on "Longitudinal variability of the biogeochemical role of Mediterranean aerosols in the Mediterranean Sea" *by* E. Ternon et al.

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General comments

This is an elegantly written, clear and fluent manuscript. The logics, order and presentation are good, the data are seemingly of high quality, and the use of literature is impressive. However, data are missing to support some of the major issues raised in this paper (as explained below). I assume that this is the result of splitting up of the data collected in the TransMed BOUM cruise between different manuscripts and possibly can be corrected by redistributing the data (or that the left-over aerosol filters be

analyzed).

The main issues are: 1). The enrichment factor used to deduce anthropogenic contribution to the aerosols is currently based on P values only and a constant P/Al ratio. Measurements of other metals such as Zn and Cd can help strengthen this argument and are likely to be obtained in the ICP analysis made.

Indeed, more measurements could have been done to trace the partitioning between natural/anthropogenic contributions for different elements but we were interested in phosphorus. Concerning the P/Al ratio that has been used, we have added to the figure 4 several P/Al from "references": P/Al ratio in the upper crust (Wedepohl, 1995; Taylor and Mc Lennan, 1995) and from Herut et al., 1999b for Saharan aerosols in the eastern Mediterranean. Adding those values show that even though there is indeed some variability of the ratio, all values fit within a quite small domain. The corresponding paragraph was also modified accordingly: "The (P/Al) ratio of both groups was mostly consistent with results reported for coastal area in Corsica: from 0.03 ("crustal source") to 0.07 ("anthropogenic source") in Bergametti et al. (1992), but well higher than the crustal reference (0.008-0.012 in Wedepohl, 1995; Taylor and Mc Lennan, 1995) and ratio in Saharan aerosol from eastern Mediterranean (Herut et al., 1999b) and "Saharan end-member" in the Western Mediterranean (Guieu et al., 2002)."

2). The nutrient input from aerosols and saharn dust analog are based on calculations rather than measurement. Since large part of the discussion is centered on the comparison between input and demand this is certainly a big gap. Can you obtain these data by dissolving the left over aerosols and the Saharan dust analog in filtered seawater to look at the potential nutrient release?

This is indeed a key experiment that was performed on-board with the left-aerosol at each station. These dissolution experiments represented a challenge as the new LWCC method was preferred over the classical spectrophotometer due to the very low concentrations encountered for both inorganic P and N. Unfortunately analytical

issues were encountered resulting in non workable data. This is the reason why we had to make estimations in the manuscript. Although this is very disappointing, we still consider those estimations provide useful information about the inputs of new nutrients to the sea surface layer during summer in the Mediterranean Sea.

3). While the productivity and N2 fixation are interesting and provide some insight the other components of the biological responses to the dust addition is lacking. Are there grazer data available? As this was defined in the cruise goals there maybe some micrograzer numbers at least for the initial conditions in the water. Chlorophyll? If Chl did not change but productivity did this will support the grazer explanation.

The microcosm approach we used was not devoted to follow the changes in the whole assemblage mainly due to practical reasons (a volume of 4.5 liters is not sufficient to provide a large number of parameters, especially those that are volume consuming). Another microcosm experiment using 20 l tanks was performed by Tanaka et al, (this issue) covered the whole food web from the bacteria to the grazers with the objective to define the limiting elements for the biological assemblage. Thanks to their measurements natural abundance of grazers are available for each station and data can be found in this companion paper. Microcosms performed by Tanaka et al (this issue) implied to use bottles of 20 Liters and was one of the major microcosm experiment of the cruise. Our experiment was performed with 4.5 L bottles. With those smaller volumes, all parameters could not be measured: choices were made and as primary production/nitrogen fixation measurements are highly water consuming, other major biological parameters such as chla/micrograzers could not be measured.

4). Too bad bacterial productivity was not evaluated with tritium – they are presumably important contributors to the population

Following the results obtained by Bonnet et al, (2005) our microcosm experiments were performed to look for changes in autotrophic organisms diversity. CA and SDA additions both favored the small phytoplankton species (in particular synechococcus).

However, we do agree that bacterial productivity data would bring complementary information.

Specific comments

5). P measurements - Detection limit for P measured via spectroscopy in a long pathlength for the aerosols was _2nM. For seawater however you report a detection limit of 10nM without explaining the method (just citing Pujo-Pay et al., 2010). Why not measure the ambient seawater in the same way as the aerosols? Clearly mapping the P concentrations along the cruise and in the experiment water was a top priority for defining the oligotrophy level and the starting conditions, a detection limit of 2nM was useful here.

As explained above (general comment 1), inorganic P was measured along the cruise with the LWCC method. Analytical problems that occurred on board resulted in non workable data, preventing any publication of this work. This is very disappointing to us as we also consider this was a key-measurement.

6). Section 3.2. Initial features at 8-m depth at the 4 tested stations- Can you provide a clear picture of the conditions in the stations you worked in. Maybe you can add some chl or nutrient data to the table that appears below the cruise track. In the section itself you refer to different experimental data you obtained, which is fine. But since these graphs focus on the experiments themselves it is not easy to get a feel of the relative differences between the stations. This is only a minor point that maybe fixed alternatively with some rephrasing of the section or a summary sentence.

Chl and nutrient measurements reported in the text of section 3.2 were performed over the whole cruise by other scientists who present their results in companion papers of this special BOUM issue http://www.biogeosciences-discuss.net/special_issue63.html). The present manuscript partially uses the obtained values without details as we did not perform the measurements. Results can be found in Pujo-Pay et al. for nutrient data (M. Pujo-Pay, P. Conan, L. Oriol, V. Cornet-Barthaux,

C. Falco, J.-F. Ghiglione, C. Goyet, T. Moutin, and L. Prieur, Integrated survey of elemental stoichiometry (C, N, P) from the Western to Eastern Mediterranean Sea, Biogeosciences Discuss., 7, 7315-7358, 2010) and in Lopéz-Sandoval et al. for chlorophyll data (D. C. López-Sandoval, A. Fernández, and E. Marañón, Dissolved and particulate primary production along a longitudinal gradient in the Mediterranean Sea, Biogeosciences Discuss., 7, 8591-8617, 2010).

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