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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.

Anonymous Referee #2

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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.

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?

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

Specific comments

P measurements - Detection limit for P measured via spectroscopy in a long pathlength for the aerosols was $\sim$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.

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.

Interactive comment on Biogeosciences Discuss., 7, 8087, 2010.