

Interactive comment on “Assessing the role of dust deposition on phytoplankton ecophysiology and succession in a low-nutrient low-chlorophyll ecosystem: a mesocosm experiment in the Mediterranean Sea” by V. Giovagnetti et al.

Anonymous Referee #1

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Title: ASSESSING THE ROLE OF DUST DEPOSITION ON PHYTOPLANKTON ECO-PHYSIOLOGY AND SUCCESSION IN A LOW-NUTRIENT LOW-CHLOROPHYLL ECOSYSTEM: A MESOCOSM EXPERIMENT IN THE MEDITERRANEAN SEA Authors: V. Giovagnetti, C. Brunet, F. Conversano, F. Tramontano, I. Obernosterer, C. Ridame, and C. Guieu

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General comments

The paper deals with the effects of atmospheric dust inputs on the phytoplanktonic community, addressing both changes in ecophysiological traits and the succession of autotrophic organisms after dust pulses. The results are part of a large experimental effort covering numerous aspects of atmospheric dust deposition, and I believe it represents a valuable scientific contribution within the scope of Biogeosciences.

Positive aspects of the manuscript are: - The specific focus on surface waters of low-nutrient, low chlorophyll (LNLC) areas, because surface communities in oligotrophic regions are often overlooked in favour of their counterparts within the deep chlorophyll maximum; - The discussion on the physiological mechanisms underlying the response of phytoplankton to dust pulses; - The accurate methods, with large clean mesocosms and three replicate tanks per condition. Good combination of flow cytometry and HPLC pigment analysis to distinguish different groups within the pico- and nanoplankton size fraction.

However, I feel that the manuscript has some flaws that require additional work:

(1) Although the authors refer to a number of companion papers in the materials and methods section, many of them are not yet published and so the reader lacks important information on the experimental design and the physico-chemical background. It is necessary to include at least one table summarizing basic environmental conditions at the beginning of the experiment (temperature, irradiance, mixed layer depth, background nutrient concentrations, mean nutrient inputs coming from the dust additions). Likewise, they provide information on the grams of dust added (line 4, page 19206) but we have no information whatsoever on the magnitude of this addition: does it correspond to a large dust storm? It is on the order of mean dust pulses over the western Mediterranean? How often occur similar pulses in the study area? Some more information on the general context of the study would be desirable.

(2) The discussion gives an interesting overview on phytoplankton succession after

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dust pulses, and provides valuable reasoning on nutrient uptake and physiological traits involved in phytoplankton shifts. In my opinion, however, the discussion would gain insights if the authors considered the effects of competing heterotrophic organisms. In oligotrophic regions, bacteria have been shown to quickly respond to dust pulses (Pulido-Villena et al. 2008, Marañón et al. 2010, Romero et al. 2011), depleting available resources and somehow delaying the transfer of nutrients to larger phytoplankton cells (Romero et al. 2011). The potential interaction with bacteria is briefly mentioned with regard to the consumption of phosphate during the second dust pulse, but to what extent that may explain the delay in the response of phytoplankton shall be further discussed. A few lines on how the effects observed in this experiment might change under some other conditions (nutrient-replete waters in winter, for instance) would also be interesting.

(3) The positive effect of atmospheric inputs on phytoplankton in the Mediterranean region has been recently challenged (Jordi et al. 2012). Accumulation of elements such as copper can progressively inhibit algal photosynthesis by altering electron transport and by inactivating a fraction of the PSII reaction centers. The authors provide data on dissolved Fe (Table 1), are there Cu measurements available? could toxic elements such as Cu relate to the observed results of some photosynthetic parameters?

(4) Statistical information is scarce; the authors should include a table providing, for each parameter measured, the statistical significance of the difference between dust-enriched (DM) and control (CM) mesocosms. It would be a helpful summary of the results.

(5) The text is a little long, in particular the results section, and some paragraphs in the discussion could be merged to shorten the ms (e.g., lines 10-25 page 19218). The authors should also pay attention to the language, because some sentences are hard to understand. The term 'since', for instance, is sometimes misused (see technical comments below), they should check that throughout the manuscript.

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Specific comments and technical corrections

Some terms and sentences are not enough specific and/or clear:

Page 19202, line 8: "high light pressure": meaning high light conditions/exposure?

Page 19202, line 15: "have often paid limited attention": do you mean that the topic has received relatively less attention? Please rewrite this sentence.

Page 19202, lines 20-23: "causing a paucity of data by which defining...": this sentence is confusing, please rephrase it.

Page 19202, line 26: "due to the important amount (...)": I am not sure whether this sentence is correct. 'given the important amount'?

Page 19204, line 4: "since their greater capacity (...) than bigger ones.": this part of the sentence is hard to understand, please rewrite it.

Page 19204, line 14: "since their different types of adaptation (...)": do you mean that *Synechococcus* and *Prochlorococcus* present varying relative proportions as a function of their different adaptation to physico-chemical conditions? Please rewrite this sentence.

Page 19204, line 24: "to deeply investigate the ecology of picoeukaryotes" sounds a bit extreme to me. Although the combination of pigment analysis and flow cytometry gives useful information on the groups of picophytoplankton present in the water, it is not the ultimate method to assess the ecology of picoeukaryotes. Please clarify this.

Page 19204, line 29: "better able": most able?

Page 19206, line 24: "two solvent mixtures: methanol, aqueous ammonium acetate (70:30) and methanol". Methanol appears twice, is it a mistake?

Page 19207, line 11: "since the low phytoplankton": meaning given the low phytoplankton?

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Page 19207, line 19: "would have induced any stress": could induce any stress?

Page 19210, section 3.2. Statistical information (p-values) should be given with regard to the difference between CM and DM mesocosms for the different parameters measured (see previous comment).

Page 19211, lines 1-3: "It can be highlighted (...) after the dust additions (DM)": I do not quite understand this sentence. Please rephrase it.

Page 19216, lines 15-16: "since the size-fractionation of samples for pigment analysis". This sentence is unclear.

Page 19217, line 28: (0.0091 $\mu\text{g Chl L}^{-1} \text{d}^{-1}$): Where does this value come from? Isn't it 0.0077 as appears in Table 1?

Page 19219, line 18: "constrains": constraints. Check this word throughout the ms.

Page 19220, line 1: "strongly respond": meaning significantly respond?

Page 19220, line 22: "prevented us a meticulous": did not allow a meticulous...? The whole sentence is confusing.

Page 19220, line 25: "the increase in greater cell size community biomass": this expression is not clear. Please rephrase this sentence.

Page 19221, line 9: "nitrogen (NO₃+NO₂)/phosphate (PO₄) ratio": what about NH₄? NH₄ can be the preferential N source for small cells in oligotrophic waters.

Page 19224, line 1: "after the second rather than the first dust addition": remove this part of the sentence.

Page 19225, line 8: "distinctively": differently?

Page 19225, line 15: "abiotic and biotic energy": could you please clarify this?

Page 19225, line 17: "the first group responding": you should clarify that it is the first group among the autotrophic organisms.

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References: I am not sure whether unpublished papers (manuscripts in preparation) shall be cited as XXX et al. (in prep.) instead of XXX et al. 2012, because the latter is confusing. There are many of them throughout the ms.

Figures: Fig. 7: I would include the p-value of the correlation in the figure. Although the correlation in Fig. 7b is significant (line 3, page 19216), it looks strongly dependent on just one point (aprox. X=110, Y=13). Please verify that it is a valid measurement.

Additional references:

Jordi A, Basterretxea G, Tovar-Sánchez A, Alastuey A, Querol X (2012) Copper aerosols inhibit phytoplankton growth in the Mediterranean Sea. *Proceedings of the National Academy of Sciences*; doi:10.1073/pnas.1207567110.

Marañón E, Fernández A, Mouriño-Carballido B, Martínez-García S, Teira E, Cermeño P, Chouciño P, Huete-Ortega M, Fernández E, Calvo-Díaz A, Morán XAG, Bode A, Moreno-Ostos E, Varela MM, Patey MD, Achterberg EP (2010) Degree of oligotrophy controls the response of microbial plankton to Saharan dust. *Limnology and Oceanography* 55(6): 2339-2352.

Pulido-Villena E, Wagener T, Guieu C (2008) Bacterial response to dust pulses in the western Mediterranean: Implications for carbon cycling in the oligotrophic ocean. *Global Biogeochemical Cycles* 22, GB1020, doi:10.1029/2007GB003091.

Romero E, Peters F, Marrasé C, Guadayol O, Gasol JM, Weinbauer MG (2011) Coastal Mediterranean plankton stimulation dynamics through a dust storm event: An experimental simulation. *Estuarine Coastal and Shelf Science* 93(1): 27-39.

Interactive comment on Biogeosciences Discuss., 9, 19199, 2012.

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