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Interactive comment on "Phytoplanktonic response to contrasted Saharan dust deposition events during mesocosm experiments in LNLC environment" *by* C. Ridame et al.

Anonymous Referee #3

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In the present study the authors try to elucidate the effect of Saharan dust input on nutrient dynamics, phytoplankton primary production and biomass using an experimental mesocosms approach in an oligotrophic ecosystem. The authors found the dust deposition increased primary production and phytoplankton biomass in LNLC environments, which although has been reported in previous similar studies. Chemical and biological parameters were carefully tested in the study. One of the novelties of this manuscript is to test the different effect of dry and wet deposition and found that the pathway/type of dust is important in determining the phytoplankton response. Below follows a few issues that need to be addressed before publication is warranted.

In the experiment, the Sahara dust analog was treated with "evapocondensation" to

simulate the real wet deposition. And this EC treatment led to higher inorganic N content in the wet deposition and thus resulted in larger increase of Chl and PP compared with dry deposition experiment. However, as far as I know, the dry deposition is not pure Sahara dust. The dust mobilization process is highly complex, which is a function of many atmospheric, soil, and terrain properties, and associated with winds as well as occurrence of chemical reactions due to aerosol contacting with changing of chemical environment. The dust could also mix with polluted aerosols during the transportation process. So I wonder if the Sahara dust analog used in the experiment can represent the real dry deposition? Why did not the authors use in situ collected atmospheric aerosol and rainwater to represent the dry and wet deposition?

In the manuscript, the authors made some conclusive statements that "wet and dry deposition events induced contrasted responses to the phytoplankton community", "dry deposition event was shown to be a negligible source of NO3". Considering the dust used in the experiment was not real in situ dry deposition, the authors should be cautious making such statement. I suggested using term "direct addition of Saharan dust" instead of "dry deposition".

Besides, many other studies using non-EC dust (dry deposition) observed significant chl increase (e.g., Paytan et al., 2009; Lekunberri et al., 2010, etc). I suggested the authors comparing the results of this study with others and adding more discussions.

In the result and discussion part, the authors included too many descriptive details on the nutrient dynamics in response to dust deposition (especially in section 3.1 and 4.2). The two parts lack focus and take home messages. Since the title of the manuscript is "phytoplanktonic response to contrasted. . .", it would be better if the authors link the pathways of nutrients to phytoplankton changes.

P758 Line 20: it is not clear why the authors added a concentration of 41 g of mineral dust mimicking a realistic flux of 10 g m-2.

Fig.8 Please add some explainations on how those lines were drawn.

The legends of axes in Fig 2-5 and Fig 7 are too small.

Interactive comment on Biogeosciences Discuss., 11, 753, 2014.