

## ***Interactive comment on “Plankton community response to Saharan dust fertilization in subtropical waters off the Canary Islands” by G. Franchy et al.***

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This article attempts to assess the effect of Saharan dust deposition on the planktonic community of the subtropical northeast Atlantic. In this area, atmospheric dust events are frequent but their influence on marine biota is poorly known. To our knowledge, only the work by Neuer et al. (2004) presented field data to assess this question in the area. As we pointed out in the Introduction, the majority of the results are based on experiments or satellite estimations, not only in the Atlantic but also in other areas. For this reason we think this article could contribute to improve our knowledge about the actual effect of the dust deposition in the field.

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In order to better describe the contents of the article and taking into account the comments of the two referees, we have considered changing the title for “Plankton community response to Saharan dust deposition in subtropical waters off the Canary Islands”.

#### Reply to Anonymous Referee #1

"The temporal resolution of the in situ measurements is rather coarse considering that atmospheric deposition events are highly episodic and short-lived."

According to the literature, neither the time of biological response nor the depth to which this response could be found after a dust event is well established. The response in the field can be almost immediate or lagged up to several weeks. For example, in the eastern Mediterranean the bacterial activity increased just one day after a dust storm (Herut et al., 2005), while in the north Pacific an increase in POC concentration was observed after 5 days and peaked after 2 weeks (Bishop et al., 2002). A high variability in the time lag of the response of phytoplankton, between less than 8 up to 16 days, was also reported from satellite chl a measurements after different dust events in the northeast Atlantic (Ohde and Siegel, 2010). Thus, the impact of dust on marine communities would be longer than a few days and, therefore, we consider that the frequency (weekly) of the sampling is suitable for the intended objective of this work. The discussion of this point has been included in page 17287.

"Can the authors have any confidence that, for instance, the higher PP rates they measured after the March dust event were in fact a result of the event? Can the authors rule out the possibility that the increase in diatom biomass was due to some other factor, for instance changes in water-column physical and chemical conditions during the onset of spring?"

The variability of PP and phytoplankton (chl a) in the oligotrophic waters of the Canary Current System is mainly driven by winter convection as we mentioned in the Introduction and it is widely described in the literature. As we argued below (comment on nutrient data), a substantial nutrient input through physical mixing was discarded.

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Thus, we considered the dust deposition the only potential bottom-up force that could drive the productivity during this period. Of course, trophic interactions, as grazing and predation, also modulate planktonic variability in a shorter scale. In fact, grazing was considered in the Discussion to explain the decrease of some autotrophic groups. However, it is not easy to evaluate separately the influence of different factors affecting planktonic variability in subtropical waters where biological interactions are complex and diverse. In fact, data we showed here are not enough to assess how top-down forces could enhance or counteract the effect of dust deposition. In any case, we think this question is widely discussed in the manuscript to conclude that the significant increase in PP and diatoms was due to the Saharan dust deposition event considered.

"There is no information on dissolved nutrient/metal concentration in seawater. This is a serious problem, because it prevents the authors from showing that the dust deposition event did in fact affect nutrient supply and also because the variability in community structure and productivity during the study period cannot be related to resource availability."

Data on nutrient concentration in the mixed layer were not available for the whole period, but some measurements were performed during the sampling (Benavides et al., 2013). We think that these data besides hydrographic sections presented here and the knowledge about nutrient dynamics in the area indicate that it is unlikely a substantial nutrient input through physical mixing during the studied period. Furthermore, high values of nitrate+nitrite and phosphate ( $> 0.5 \mu\text{M}$ ) were measured even in May (see Benavides et al., 2013), a typically stratified period with low nutrient concentrations ( $<0.1 \mu\text{M}$  nitrate+nitrite,  $<0.05 \mu\text{M}$  phosphate) in these waters (Neuer et al., 2007). On the other hand, the release of nutrients (nitrate, phosphate and iron) from Saharan dust has been demonstrated as it is discussed in the text (e.g. Bonnet et al., 2005; Herut et al., 2005). The magnitude of the nutrient release from dust deposition and the effective increase of their concentration in the mixed layer is a complex process. It depends on a wide variety of abiotic and biotic factors (e.g. interaction with organic ligands,

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particle aggregation, microbial uptake) and it is not totally understood yet. In this work we cannot directly demonstrate the link between dust deposition and seawater nutrient concentration. However, as we have discussed here and in the article, there are enough evidences (atmospheric suspended matter, hydrographic data and seawater nutrient concentration) that strongly support that the intense atmospheric dust deposition during the period studied promoted the high nitrate and phosphate concentrations in the mixed layer. Nevertheless, we have modified the text from line 18 in page 17286 to line 9 in page 17287 in order to improve the discussion in this regard.

"In the Introduction and Discussion sections, the authors emphasize the role of iron. However, their study region is not a HNLC region but a LNLC region where there is no evidence that iron is limiting primary production."

Agreed. It is known that primary production in low nutrient low chlorophyll (LNLC) systems as the subtropical northeast Atlantic is usually limited by nitrogen or co-limited by nitrogen and phosphorus, while iron is considered the limiting element in high nutrient low chlorophyll areas. However, it seems that iron could be also limiting in LNLC waters where its addition promoted the increase in chl a and primary productivity (Bonnet et al., 2005). In any case, it was not the aim of this work to ascertain what specific element limits productivity, but to study if primary production is enhanced by Saharan dust deposition and its effect on the planktonic community. Nevertheless, the text has been modified in the Introduction in page 17276, lines 24-26 considering this comment. Furthermore, a more recent reference was included (Moore et al., 2013).

"The primary production rates reported are impossibly high. The authors are aware of the problem, but nevertheless have decided to use the data. Some error must have occurred, but there is no basis to assume that the error has been systematic and has not affected the temporal trends in addition to the absolute PP values."

Agreed. We assumed a possible error in primary production (PP) data but we could not find out the cause. Nevertheless, we decided to use those data considering that

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temporal trends were not affected based on PP rates measured during 2011. We used the same method during this study in 2010 and during 2011 and we observed high rates in both years. However, the temporal variability of PP in 2011 showed the characteristic seasonality of these waters (see Fig. 1 attached). PP was correlated to chl a concentration and its increase followed the deepening of the mixed layer. These results indicate that PP variability would not be affected despite absolute rates are too high. We have included this paragraph in page 17288, line 16, to sustain the discussion in this respect.

"Why were PP measurements conducted with seawater from 20 m if atmospheric deposition affects mostly the surface layer?"

Regarding to the vertical extent of the response, it is difficult to know until what depth is the planktonic community affected as the response in the field has been measured from satellite data in many cases. However, it seems that the effect of dust deposition is not limited to surface waters according to some results. The increase in POC concentration in the north Pacific measured robotically by Bishop et al. (2002) reached 40 meters depth. In the East Mediterranean the effect of a dust storm was measured up to 40 meters for *Prochlorococcus* and in the first 15 m of the mixed layer for chl a and heterotrophic bacteria (Herut et al., 2005). Therefore, the depth of the sampling (20 meters) is suitable for the intended objective of this work as planktonic communities could be affected not only in the surface waters, but deeper in the mixed layer. The discussion of this point has been included together with the first point (temporal resolution of the sampling) in page 17287.

"Summary, 1st sentence. The authors refer to 'the high atmospheric iron, and nitrate and phosphate concentrations found in the mixed layer', which is rather puzzling. Either the concentrations are atmospheric or they were measured in seawater – not both. My understanding from the Methods section is that metal concentrations were measured in atmospheric material, not in the seawater. This should be clarified."

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Metal concentration was measured from total suspended matter (TSM), while nitrate and phosphate concentration in the mixed layer was measured elsewhere and referenced in this work to sustain our discussion. We have rewritten this phrase to avoid misunderstandings.

"Methods – PP incubations lasted between 6 and 22 h, depending on the sampling date. Using different incubation times may have introduced significant error in the calculated PP estimates, because the extent to which fixed  $^{13}\text{C}$  is respired, excreted or recycled is strongly dependent on incubation time."

Incubation time in primary production measurements did not really vary as much because all incubations lasted between 6.5 and 10 h excepting on March 10, when incubation lasted 22 h because of logistical issues. We have included this information in Material and Methods.

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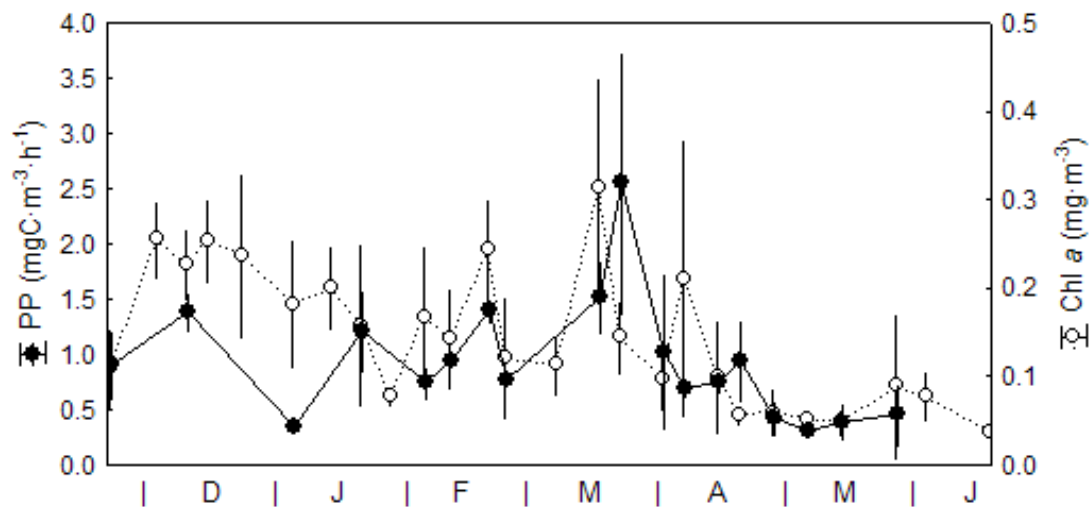


Fig. 1.

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