

Interactive comment on “Emissions from potential Patagonian dust sources and associated biological response in the Atlantic sector of the Southern Ocean” by A. Castagna et al.

Anonymous Referee #1

Received and published: 24 September 2014

The topic of this manuscript is the response of the oceanic plankton to inputs of iron and other nutrients from the deposition of airborne dust. The topic itself is interesting and relevant. While Patagonia is a small dust source area compared to the large desert regions mainly located in the northern hemisphere, small changes in dust deposition may make large impacts. The authors claim that they find a response in plankton concentration to changes in Patagonian dust emissions, indicating an important role of airborne dust deposition. This finding is derived from satellite observations of different parameters (NDVI, AAI, Chlorophyll, phytoplankton carbon, . . .) which are correlated with each other for a time period from 2003 to 2010. Unfortunately the conclusions are based on many assumptions without direct evidence. Therefore I cannot recommend

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it for publication in the current form.

Following issues are problematic:

- The methods are not made entirely clear in the manuscript. How were the correlations computed? Were they just based on annual means, monthly or daily fields? Did I overlook some information on this? Seasonal correlations may provide additional information.

- 8 Years is not a great basis to provide statistically significant information from correlations. Please provide information about the statistical significance of the correlations of dust and plankton signals.

Some assumptions that were insufficiently supported by the information given in the manuscript:

- Dust microphysical properties and transport altitude exhibit lower variability than dust optical thickness -Coherence between dust sources and deposition - Ratio between dust load and oceanic biomass low enough such that the interannual signal is not disturbed.

- The point that atmospheric dust load does not interfere with the retrievals of oceanic biomass (chlorophyll-a, diatoms, phytoplankton carbon) is crucial for the credibility of the results, but not sufficiently supported in the manuscript, apart from stating some references (section 2.2). Which are the ratios of the relative concentrations that would be low enough so that the signals do not interfere with each other? What concentrations are present in the area, are they below that ratio? In particular when looking at the interannual variability, the high dust years may disturb the phytoplankton signal, which would make the presented results meaningless. Please provide further evidence that this is not the case. The comparison of the correlation with the chlorophyll concentration is not conclusive – as far as I understand the total ocean area below 40 deg S was used for this analysis, however the correlation maps show that there appear to be

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different controls in different parts of the ocean. To be conclusive only the values in the region with the significant positive correlations should be taken into account in figure 4.

- A possible misinterpretation of biomass burning aerosol as dust aerosol was not discussed, but could potentially greatly impact the results. As far as I see by excluding areas with $NDVI > 0.4$ the authors claim to reduce the biomass burning impact, however biomass burning smoke is frequently transported over great distances. One approach may be to use information from the MODIS 'fine mode' optical thickness product to exclude biomass burning influence.

- Section 3.1: That area 4 is misrepresented as dust source casts doubt on the method to determine dust sources.

- What is the motivation of using '10% trimmed AAI' as individual strong events may also induce strong ocean deposition events and plankton growth? If you are after some background signal, you should use the median values instead.

- Minor remark – some language improvement would be needed.

Interactive comment on Biogeosciences Discuss., 11, 11671, 2014.