

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.

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Dear referee,

We appreciate your comments. In the following text we will address your considerations. A marked-up manuscript version with all alterations made considering reviews by anonymous referees #1 and #2 is presented as a supplement.

Referee comments are reproduced here, in italic, to facilitate reading.

Anonymous Referee #2 Summary

This is an article on an interesting topic, but it does not really prove it's point and should

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be rejected. I understand that there is no other way to do the problem, but that doesn't mean one should publish results which don't proof the assertions.

Authors: We have provided several evidences to support our assumptions and conclusions. We understand your comment in the sense that our manuscript does not respond definitively about a possible association of Patagonian dust and ocean productivity – most like any other published study did up to now. Nevertheless, we are convinced that it shades light in the problem, looking that at the point of view presented therein.

Specific topics:

1.a) Anonymous Referee #2

Dust variability is not well established. “To define the dust source areas within meridional South America we relied on the negative correlation of the AAI with a vegetation proxy, employed as an integrative parameter of two time varying surface properties related to dust emission (Jobbágy et al., 2002; Cropp et al., 2013): (i) the soil moisture content, which influences particle cohesion; and (ii) the abundance and structure of vegetation, which influences the transmittance of the kinetic energy from the wind to the surface (Tegen and Fung, 1994; Mahowald et al., 2005). Together, these parameters regulate the threshold wind velocity needed to initiate the dust emission over a specified region. An analysis highlighting areas where AAI increase is related to vegetation decrease could reveal areas of dust emission, provided that areas of biomass burning are excluded.” Why are you adding in the criteria that the NDVI has to negatively correlate in time with the AAI? This assumes that winds don't play a role or are somehow correlated with negative NDVI? I don't understand this criteria or the sensitivity of your final results to this choice. This is not a standard choice: usually just the areas with frequent AAls. Please explain more this unconventional choice and the implications for your study.

Authors: The climate of Patagonia is characterized by strong and persistent westerly

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winds that peak in the summer (Paruelo et al., 1998), the same season when the surface is more susceptible to wind erosion, which creates a favorable condition to dust emission. This occurs because dust sources are mainly seasonal dry lakes and rivers (e.g., Prospero et al., 2002), that expose sediments in the summer, the regional dry season. Also, primary production in arid regions are closely related to water availability, and the Normalized Difference Vegetation Index (NDVI) in Patagonia peaks in the spring (not summer) when moisture is still abundant from the wetter season, but temperature and irradiance are more favorable than they were in winter (Paruelo et al., 1998). This sets a condition for some correlation of those variables. Nevertheless, our study does not assume that wind speed is correlated with the NDVI, but only the transmission of its kinetic energy to the surface, as the NDVI is a proxy for the abundance and structure of vegetation.

Other processes than wind speed, related to surface susceptibility, can explain most of the variability in dust emission, as observed for the Sahara (Prospero and Lamb, 2003). But we also do not assume that winds are not important in the emission process. Dust in transport in the atmosphere is a product of surface and meteorological properties, and therefore it implicitly includes the effect of both surface susceptibility and wind action. What is needed then is to spatialize this relation to estimate source regions. Our approach drew upon the physical process of dust emission, focusing on a property that is specific to the variable being estimated – a surface condition - while winds are less specific. We also excluded volcanic and biomass burning areas by adding a NDVI criteria (not just negative correlation, as explained in the manuscript). This approach is not perfect, as has been previously discussed for dust source area 4, but our view is that overall the addition of a negative correlation with NDVI gives a simple, safe and direct answer to estimate dust source areas over this region.

Previous works may have been based only in the AAI data, but as our study, they are only estimates – and it is positive that all those estimates are coherent, adding confidence that source regions, at least on large spatial scales, are reasonably constrained.

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For that same reason, implications of this approach over our results, if any, should be minimal.

1.b) Anonymous Referee #2

The AAI is also correlated with simply the boundary layer heights, so what you are seeing could also just be interannual variability in boundary layer height. Do you have no other data about the dustiness of the region? What about visibility data, precipitation data (which might be used to infer stronger sources), other satellite data?

Authors: The AAI data is dependent on the height of the base of the aerosol layer (e.g., de Graaf et al., 2005), which might be somewhat correlated with planetary boundary layer height (PBLH) due to their dependency on the convective process of the boundary layer. But we also point out that Li et al. (2008) found with model results that the dust from South America/Patagonia is mostly transported within the boundary layer, not above it. Unfortunately, we cannot evaluate now if the PBLH could be an independent data (in relation to meteorological variables related to dust emission) for validation nor do we have access to such data. We understand that there are different models and algorithms to estimate the PBLH as well different meteorological data or assimilations data bases that could be used in such computations – all of that would have to be evaluated and possible would require a considerable extension in time.

Just to avoid possible confusion, we point out that Mahowald and Dufresne (2004) in the article intitled “Sensitivity of TOMS aerosol index to boundary layer height: Implications for detection of mineral aerosol sources” actually assumed/prescribed that the dust aerosol layer was co-ocuring with the PBLH in order to perform their calculations.

In the manuscript and in the answer to referee #1 we have added a number of other publications evaluating the quantitative use of the AAI. The Torres et al. (2002) classification of the AAI as qualitative is not unanimous and other authors describe it as a semi-quantitative index (Chiapello and Moulin, 2002). The sensitivity of the AAI to the mineralogy and base of the aerosol layer limit cross-comparison for different regions,

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but has been used on different studies for temporal monitoring of dust load over single regions.

1.c) Anonymous Referee #2

In addition, there is not necessarily a simple relationship between source strength and deposition in the adjacent ocean, and basically no justification for this assumption. So the idea that AAI in one or two locations is automatically a good proxy for deposition downwind is not well presented to defended in the text.

Authors: We understand that this question has already been answered to the anonymous referee #1. We argued that only about 13% of the air parcel trajectories leaving Patagonia reaches Antarctica before 10 days and as dust is a short lived species in the atmosphere, the majority of the dust leaving the continent deposit over the ocean - there is no need for a single relationship between source strength and deposition. Also, as discussed in the manuscript, estimation of dust deposition from different models, using different periods and time frames, result in similar deposition pattern over the ocean. Therefore, although variations in deposition patterns with distance from source is anticipated on the scale of individual events, there seems to be a consistent pattern over larger time scales. Another possible effect is the transport by ocean currents that would act to reduce variation on spatial deposition patterns.

2) Anonymous Referee #2

Dust correlations with chlorophyll in the satellite data; known bias of the remote retrievals "Combined, these properties minimize the noise added by dust variation, suggesting a negligible effect of dust on biological proxy estimation in this region (e.g., Johnson et al., 2011)." I think you probably need at least an order of magnitude calculation here to show that in this region the interference from the dust absorption in the atmosphere during dust events is small compared to the change in the phytoplankton and the satellite detection. This is a really big issue that is poorly resolved in some papers in the literature.

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Authors: We also understand that this question has already been answered to the anonymous referee #1. The order of magnitude calculation has been computed using typical values of chlorophyll for the region. We concluded, based on published studies, that to cause an artifact effect it would be needed an event 10 times stronger than needed to stimulate biological production. We also argued that such magnitude has been observed in the Mediterranean, very close to the most strong present day source - the Sahara – and that would be improbable even a single event (let alone a series of them) of that magnitude on the adjacent ocean from Patagonia.

3.a) Anonymous Referee #2

Finally the 'dustiness' predictor correlated with the chlorophyll - Figure 3: I don't understand this plot: is it just the mean AAI over south American sources (e.g. figure 2) correlated with the annual average time series of chlorophyll in each location? Please make sure the figure caption is clear. Please indicate which values are statistically significant. In addition, please include the effects of looking for correlations at so many points (e.g. if you look for statistical significance at a 95% at 100 points, you expect 5% of the points to be significant just because of randomness. Note that physical coherence in your result is not a good argument against this, because you also haven't taken into account the physical correlation between adjacent gridpoints, which would reduce the number of independent points). I just read your response to the other reviewer and do not buy the argument that because statistical approaches are not perfect, you don't have to think about whether you are significant. You have a very short time record, so it's quite likely you are just seeing random effects.

Authors: The reviewer's interpretation is correct, but we will make changes to better clarify the figure caption. As for the statistical analysis, our criticism to the blind use of statistical significance is not based only on personal judgment but is referenced on the literature. We have presented detailed arguments against it and alternative results (confidence bounds) that can also be used directly to infer statistical significance if one so desires. Our arguments remain the same as the ones published in response to the

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anonymous referee #1, and for the sake of brevity, we will not repeat them here.

3.b) Anonymous Referee #2

A simple alternative explanation which is not considered, but is also consistent with both the negative and positive correlation space (if they are significant), is that there is a correlation between the 'dustiness' and the ocean response because both are driven by the same meteorological phenomenon. This needs to be explicitly considered in the text.

Authors: This suggestion is, of course, possible but we would not call it "simple" – as a mechanism of common interaction would have to be proposed and demonstrated, what has not been accomplished thus far. But again, it is possible that other variables (meteorological or of other nature) may have similar temporal patterns, in which case it would not be possible to resolve the specific mechanism with associative studies.

Nevertheless, it should be noted that variations in dust emission may also be the mechanism by which a meteorological phenomenon interacts with the biological system – that is, a common correlation with a meteorological phenomenon would not automatically exclude the dust fertilization hypothesis. For example, the most direct of such phenomenon that could have influence over both dust emission and ocean surface mixing is wind speed. Enhanced surface mixing could result in enhanced supply of micronutrients and the biological variability could be simply due to the mixing process or both mixing and dust deposition – again it would not be possible to resolve the mechanism.

This wind speed suggestion has been proposed and evaluated by Meskhidze et al. (2007). In their published figure 3 it is possible to see clearly that their latitudinal band of response occurs just north of ours, almost in a complementary fashion, with no evidence of wind induced surface mixing influence over our study area (correlations between -0.1 and 0.1). By exclusion then, we could remove wind speed as an external force that could add another mechanism (surface mixing) to the already proposed

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dust deposition. But it would not be reasonable to assume that we must investigate every possible correlated variable, because even if we did, our analysis would not by itself imply causation. This discussion is already present in the manuscript and we will expand it for the reviewed version.

We have been careful to not suggest that we are proving the present day Patagonian fertilization in our study, but only adding evidence in its favor – based on our results and on the discussion of previous published results of different nature.

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