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***Interactive comment on*** “**Mediterranean basin-wide correlations between Saharan dust deposition and ocean chlorophyll concentration**”  
**by R. Gallisai et al.**

**Anonymous Referee #2**

Received and published: 28 August 2012

The paper by Gallisai et al. aims at describing the role of mineral dust, originating from Sahara and Middle East deserts, in the regulation of phytoplankton dynamics in the Mediterranean Sea. Authors used chlorophyll concentration data as derived by the NASA standard processing to address the phytoplankton dynamics and the 3D atmospheric deposition dust model to investigate the desert dust patterns of variability. Their main finding is that phytoplankton response to dust addition is different in different areas of the basin and depending on the trophic/dynamical regimes these areas are subject to. The paper is clear and well written and presents an interesting piece of work especially for the monthly deposition patterns. However there are several issues that need to be addressed before the paper can be considered for publication in

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biogeosciences.

## Main issues

1) One of the most important issues is concerned with the temporal resolution of the ocean color data. One of the most important findings of Volpe et al. (2009) is that weekly average fields should not be used for seeking phytoplankton response to dust addition as dust can mimic phytoplankton absorption producing an error that can easily propagate when averaging over this time scale. This is particularly relevant as the authors used a chlorophyll product retrieved via standard processing which does not account for dust neither in the atmosphere nor in the surface water. There is a bit of the discussion which is centred over this point but then I had the impression that authors completely miss the point when saying "Because of the potential signal interference of AOT with chlorophyll reported by these authors [Volpe et al. (2009)] we shy away from a higher temporal resolution that may give spurious correlations". I strongly encourage the authors to perform the same analysis they already made, but using daily fields for both the deposition and chlorophyll fields. I understand that this imply a substantial amount of work, but I believe that this is the only way of improving the paper.

2) A non trivial issue is concerned with the use of cross-correlation between variables that present high autocorrelation, as is the case at least for chlorophyll. This has strong implications on the significance of the correlation values found by the authors. For example, although the entire eastern basin presents acceptable correlation values (figure 5), the level of confidence (95%) may result much lower if autocorrelation is taken into account ultimately affecting the overall outcome of the paper. This needs to be properly addressed.

3) Most of the discussion presented in this paper rely on very low correlation values (in figures 6 and 7  $r$  never exceeds 0.3-0.4 which results in  $r^2$  to be 0.09-0.16). Again, the significance of these values should be first tested accounting for autocorrelation. In any case, my personal interpretation of such low correlation values (independently of

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their significance) is that chlorophyll and dust deposition are significantly low correlated (pretty much as uncorrelated), at least at this time scale.

4) The chlorophyll retrieval algorithm used by the authors has been extensively shown to produce unrealistic values in the Mediterranean Sea. Although this is not as crucial as the other points, nevertheless, if the paper deals with the phytoplankton dynamics in the Mediterranean Sea, I would suggest the authors to use a more appropriate algorithm.

#### Minor issues

L23 of page 8614: primary production should be substituted with phytoplankton biomass.

L8 of page 8616: bilinearly

L19-L21 of page 8622: this sentence is a little confusing as authors attribute the low phytoplankton response to large dust outbreaks as due to the fact that dust clouds travel away from the basin. If I understood it correctly authors used deposition not transport data, and if so, how can a dust outbreak, which is detected through its depositional pattern, travel away from the area of interest?

I suggest the authors to change the colorbar of the figures as, for example, orange indicates both mid- and high values which makes difficult to unequivocally understand the patterns that these figures are meant to represent.

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Interactive comment on Biogeosciences Discuss., 9, 8611, 2012.

**BGD**

9, C3614–C3616, 2012

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