

## ***Interactive comment on “Understanding the transport of Patagonian dust and its influence on marine biological activity in the South Atlantic Ocean” by M. S. Johnson et al.***

### **Anonymous Referee #1**

Received and published: 2 January 2011

The paper deals with the dynamics of dust emission and deposition to the ocean and evaluation of its feedbacks on the marine biota. The topic is of major interest and uncertainties are still large, thus the motivation of this study can be considered as relevant.

The paper is based on two case studies, and it is articulated in two parts: the first one describes the outputs from model simulations of dust emission, transport and deposition using the GEOS-Chem/DFeS model for the two case studies, which are compared with remote sensing data from MODIS and CALIPSO; the second part combines outputs from the GEOS-Chem/DFeS model with offline calculations to determine

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



the bioavailable Fe from mineral dust and the correspondent Chl-a production, that is compared with SeaWiFS retrievals. The first part uses the same model described in previous works, and besides setting the bases for the second part, is interesting per se as an analysis of two case studies of dust transport from Patagonia, including the vertical structure and synoptical features of the events described. The second part is an interesting contribution to the study of the potential indirect climatic effects of mineral dust, and includes a discussion of the uncertainties of the model and parameterizations.

Overall the paper is very well written and the figures are clear and adequate for supporting the discussion. I would recommend the publication of this paper, but I suggest that a few relevant aspects listed below should be considered by the authors in editing the final version.

General remarks:

Section 2.2 would require some clarification: the relations linking Fe(leachable) from equation (1), sol-Fe from equation (2) and sol-Fe calculated using GEOS-Chem/DFeS should be made explicit, and Table 1 modified accordingly if necessary. Throughout the paper it is not unequivocal whether the leached Fe from equation (1) has been used for calculations of  $D[\text{Chl-a}]_{\text{obs}}$  plotted in Figures 8 and 9 or just for the uncertainties analysis depicted in Figure 10.

Section 3.2 and Figure 9: while model/parameterizations uncertainties and SeaWiFS retrievals quality have been discussed, some comment on spatial and temporal variability of Chl-a products in Figure 9 is lacking. Spatial variability looks very high, with many areas where  $D[\text{Chl-a}]_{\text{obs}}$  is largely negative, within the variability range depicted and compared with the positive values. Could you comment on this? What is the area-averaged  $D[\text{Chl-a}]_{\text{obs}}$ ? In addition, could you give some measure of the background temporal variability (i.e. from the before-storm week, or provide a reference)?

Specific remarks:

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Lines 69-71: “Based on positive correlation ... from Patagonian and southern Australian regions ... is controlled by Patagonian dust ...” sounds here a bit contradicting. Please rephrase.

Line 112: “resulted” is it a typo for “resulting”?

Line 160: add within brackets the value of  $f\text{Fe}(\text{leachable})$ , the same way as it is done for the other variables.

Lines 233-235: check the chronological consistency between the statement (“... since 2008 ...”) and the references cited. In the present form it sounds a bit weird.

Lines 267-268: reading this in combination with line 264, and looking at Figure 3, is it possible that the model simulated (and captured as it seems from CALIPSO data – Figure 3a,b) two (likely spatially) distinct dust outbreaks (with converging plumes) that in the daily-averaging stage resulted in that “v” shape of the simulated dust burden (horizontal plain in Figure 3 plots)? Some supporting information on this can result from comparing MODIS (figure 1a) and CALIPSO timing.

Lines 377-380: not too clear. Does this mean that - within the 8 days after the dust event - the leached Fe, calculated from Eq. (1) assuming a residence time of 30 days in the formula, was about 50% compared to the atmospheric flux of rapidly-released Fe? Please rephrase.

Table 1: is the reference for sol-Fe just GEOS-Chem/DFeS? Or rather GEOS-Chem/DFeS + Equation (1) as lines 178-180 in the text would suggest?

Figure 1: please complete the reference adding the time for satellite imagery. (1a) 18:45 UTC (1b) In addition, I am confused by the units used for column dust concentrations, they look like depositional fluxes. Please check and modify figure and caption accordingly.

---

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 27283, 2010.