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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Volcanic ash as fertiliser for the surface ocean" *by* B. Langmann et al.

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Overview:

This manuscript presents data which is the most convincing to date of the impact of volcanic ash deposition and its potential for short term enhancement of primary productivity in the ocean. It is an important paper that deserves to be published because it marks the first example where the effect seems to be more widespread, and with better data coverage than earlier works examining the same process (Duggen et al., 2007; Uematsu et al., 2004). It also includes the first attempt at making a budget for the iron supply from such an eruption, which is an important and necessary evaluation, in order to assess the climatological importance of these events. However I would also stress that this approach not only needs to be applied to eruptive modelling but more crucially to deposition models where a real link between the spatially observed signals

and the modelled deposition fluxes can be made.

The paper makes a good case for the importance of volcanic aerosols on short term climate on the monthly timescale but contrary to the authors conclusions it does not strongly suggest a feedback mechanism for major volcanic eruptions: firstly because there is no evidence supplied for any feedback link between the climate and the frequency of eruptions. Secondly eruptions are sporadic and episodic in nature and cause perturbations to the climate record, which are important to understand, but there is no evidence here that they have any lasting influence on the climate record for more than a year or two.

General Comments:

Marine Primary Productivity (MPP):

In the present manuscript the authors use the term MPP to often describe two related but different parameters. It needs to be clearly stated that; productivity is a rate based measurement which involves the uptake of typically C per unit time, while chlorophyll concentration is used as a proxy for biomass. Thus individual MODIS satellite chlorophyll data give information on chlorophyll concentrations, and by proxy biomass, but not about productivity. These are important distinctions as upon relief of iron limitation cells firstly increase their photosynthetic capability, leading to an increase in chlorophyll, but this does not necessarily lead to an immediate increase in cell number nor C biomass which may follow several days later in high latitude regions (Boyd et al., 2000; Hoffmann et al., 2006). This is also seen in the Duggen et al. laboratory experiments where Fv/Fm has increased rapidly in the first 48 hours but chlorophyll responds only after 6 days (Duggen et al., 2007). While productivity can be estimated from satellite chlorophyll data (Behrenfeld and Falkowski, 1997) it needs to be more clearly stated in the manuscript that the change in observed chlorophyll between monthly averaged satellite data is interpreted as an increase in primary productivity.

SO2 as a tracer of the ash plume:

I was missing information on the utility of SO2 as a tracer for the ash plume. It is known that in the presence of volcanic ash the satellite retrieval of SO2 are typically overestimated (Corradini et al., 2009) unless corrections are made. Additionally other recent work shows that there is a separation between the ash and the SO2 in the eruption cloud (Doutriaux-Boucher and Dubuisson, 2009; Prata and Kerkmann, 2007; Rose et al., 2000) and that using SO2 to track the ash cloud could be dangerous for aircraft (Prata and Kerkmann, 2007), so the question is, is SO2 a good tracer of the ash over long distances? I think some information on this question needs to be provided by the authors because it is important for modelling and assessing the spatial extent of the deposition field.

Model of the deposition pattern:

Linked to the above comment, does the region of the chlorophyll response match the deposition field? Have any modelling efforts been made on this important aspect of the work? It seems to me that the assumption of a uniform deposition flux (P719 line 21) would not be in reality the case with much more of the ash deposited close to the source. This raises questions then about the size spectra of the deposited aerosols, where finer aerosols may be more soluble and provide more Fe per g ash. The Duggen et al. (2007) work which the authors use for their estimates of Fe supply was performed on relatively large aerosol particles collected close to the source. Currently I am aware of no samples that have been collected from a plume that has covered a long distance and these small ash particles may be more soluble (Baker and Croot, 2008). Some comments on this aspect of the work would greatly improve this paper.

Specific Comments:

P712 line 19. Please supply a citation for the upwelling source of iron to the ocean.

P714 line 6. A recent paper in GRL also presents data over Europe for the SO2 cloud resulting from this eruption (Martinsson et al., 2009).

P717 line 2. See general comment above about the use of the term productivity in this context. Also in the IronEx experiment (Martin et al., 1994) both the measured chlorophyll and primary productivity doubled within the first 24 hours after the iron addition. It is in the high latitude regions that the response is slowly.

P 719 line 4. (sp) Duggen

P719 line 9. Is this a representative mixed layer for the North-East Pacific at this time of year? Some supporting data should be provided on the relevance of this estimate. There are Argo float data available for this (Ohno et al., 2004) and data from climatological atlases that could be included. The information on MLDs supplied on line 4 P722 should also be included here.

P719 line 10. There is a mistake here as the estimate for the Fe required by the calculation scheme given here yields 0.9 – 1.2 x 108 mol Fe for the given fertilised area estimates. P719 line 12. Have any samples of this ash been collected and analyzed?

P719 line 14. As for line 10, this should be reported as 4.5 – 6.0 x 1011 kg to be consistent.

P723 line 5. This is an overstatement of the papers results as the climatic impacts beyond a few months, at best, has not been shown. The statement should be tone downed to fit the actual findings. See also the overview above. Figure 2. The authors should please include in the figure legend what primary data source (SO2 retrieval?) is being used to generate this plot.

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