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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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Answers to Peter Croot:

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.

We thank Peter Croot for his fair and critical review. We agree that the Kasatochi eruption did not show any feedback on climate. The climate impact of iron attached to volcanic ash via surface ocean fertilisation and activation of the biological CO2 pump remains speculative until now. The manuscript will be modified accordingly.

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.

Thanks for this advice. The manuscript will be modified distinguishing clearly between MPP as rate for carbon uptake and chlorophyll as a proxy for biomass.

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.

There must be a misunderstanding, maybe caused by the fact that we mention the amount of SO2 released during the eruption of Kasatochi on page 714. During the rest of the analysis of satellite data we make no use of SO2 data. Fig. 1 of the manuscript shows a MODIS picture in the visible wavelength range with the white colours showing water clouds and the brownish colours showing the location of the ash cloud released from Kasatochi (see figure caption). As described in section 4.1. we used the BTD method to qualitatively track the Kasatochi ash cloud as shown in Fig. 2 of the manuscript. We would like to emphasise here, that Fig. 2 shows ash distributions, no SO2 data. However, we would also like to note that different to previous volcanic eruptions, a separation of the ash and SO2 released from Kasatochi has not been observed (Langmann et al., 2010, Prata et al., 2010) so that in the case of the Kasatochi erup-

tion SO2 and volcanic ash travelled along the same transport pathways, even though the atmospheric ash content was considerably reduced after a few days due to rapid sedimentation.

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.

The manuscript of Langmann et al. (2010) describes a three-dimensional regional modelling study of the atmospheric dispersion of volcanic ash after the eruption of Kasatochi and its removal out of the atmosphere. The southern edges of the simulated deposited mass distribution (see Fig. 1 below) match the areal distribution of the chlorophyll bloom (Figure 3E of the manuscript) pretty well. The atmospheric dispersion of volcanic ash shown in Fig. 2 of the manuscript as derived from satellite data also supports to close relationship between surface ocean biomass and volcanic ash distribution. We already mentioned in the manuscript on page 719 that the assumption of a uniform deposition flux is a simplification. With the assumption that bio-available iron is released from iron salts coating the surface of ash particles, we can conclude that finer ash particles carry more iron per mass unit than coarser ones due to the larger surface area of the finer particles. After a volcanic eruption, ash particle diameter and mass principally decrease with time and distance from the volcano, so that close to the

volcano generally bigger particles with less iron sink relatively faster through the ocean mixed layer than further away from the volcano, where smaller ash particles with more iron per unit mass sink slower. These processes can compensate each other to a certain extent and can lead thereby to a more uniform distribution of iron in the surface ocean than expected. More explanations will be added to the revised manuscript.

Specific Comments:

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

The following reference will be included into the manuscript: Castro, P. and Huber, E. M.; Marine Biology, McGraw-Hill Book Company, 2007.

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).

On Page714 line 6 we mention the amount of SO2 released during the eruption of Kasatochi. SO2 in the atmosphere forms sulphate via oxidation and these sulphate molecules can nucleate or condensate on existing particles thereby growing with time. The recent paper of Martinsson et al. (2009) published in GRL reports about measurements of sulphate and carbonaceous aerosols after the eruption of Kasatochi detected within the CARIBIC network over Europe. It is an interesting paper, but it is about sulphate and not SO2, therefore we did not include it into the reference list.

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.

Thanks again for this advice. The manuscript will be modified distinguishing clearly between MPP as rate for carbon uptake and chlorophyll as a proxy for biomass.

P 719 line 4. (sp) Duggen

Corrected in the manuscript

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.

A mixed layer depth of 20-40 m is typical for the North-East Pacific during August/September (Whitney and Freeland, 1999). Fig. 2 showing the Argo float data for August 2008 (http://www.pac.dfo-mpo.gc.ca/science/oceans/Argo/Argo-melange-mixed-eng.htm) confirms the shallow mixed layer depth during August 2008. The revised manuscript will be modified accordingly.

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?

The upper value will be corrected to 1.2 x 10^17 nmol Fe – thanks for noticing this. Unfortunately, only a few 'fresh 'ash samples are available from the Kasatochi eruption, collected from the ship that rescued two biologists from Kasatochi Island a few hours before the eruption. We would be really interested to receive some of these samples, but this was not possible until now and we are also not aware of other analysis of Kasatochi ash samples.

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

Will be corrected in the revised manuscript.

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.

We agree that the Kasatochi eruption did not show any feedback on climate. The climate impact of iron attached to volcanic ash via surface ocean fertilisation and activation of the biological CO2 pump remains speculative until now. The manuscript will be modified accordingly. We will add to the legend of Fig. 2: ... based on MODIS level 1b data at 11 and 12 micrometer using BTD. More explanations can be found in section 4.1 of the manuscript. Please note here again: Fig. 2 shows the volcanic ash signal not that of SO2.

References

Langmann, B., Zakšek, K. and Hort, M.: Atmospheric distribution and removal of volcanic ash after the eruption of Kasatochi volcano: A regional model study, accepted by J. Geophys. Res., special issue on Aleutian volcanic eruptions in summer 2008, 2010.

Prata, A. J., Gangale, G., Clarisse L. and Karagulian, F.: Ash and sulphur dioxide in the 2008 eruptions of Okmok and Kasatochi – insights from high spectral resolution satellite measurements, submitted to J. Geophys. Res., special issue on Aleutian volcanic eruptions in summer 2008, 2010.

Whitney, F. A. and Freeland, H. J.: Variability in upper-ocean water properties in the NE Pacific Ocean. Deep-Sea Research II, 46, 2351–2370, 1999.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 711, 2010.

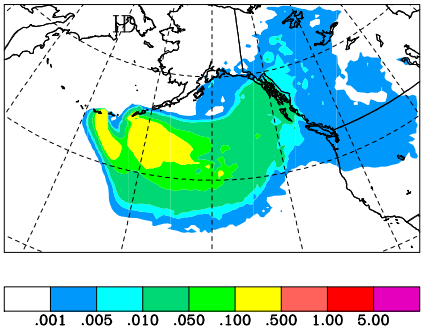

Fig. 1. The figure is based on Langmann et al (2010) showing model simulation results of the sum of mass of volcanic ash [mm/8d] removed from the atmosphere after the eruption of Kasatochi.

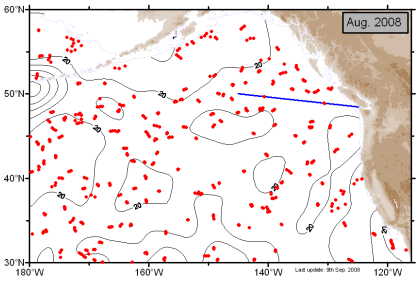

Fig. 2. Argo float data for August 2008 (http://www.pac.dfo-mpo.gc.ca/science/oceans/Argo/Argo-melange-mixed-eng.htm) .