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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 Reviewer I:

The authors correctly note that the impact of volcanic emissions have not been particularly considered in assessments of the impact of atmospheric dust on the oceans, although Boyd et al. 1998 (cited here) and Shroth etal 2009 Nature Geosciences have considered this source. The lack of extensive studies of the role of volcanoes arises In part because estimates of global dust sources suggest they are much smaller than desert dust emissions (Jickells et al., 2005). However, volcanic emissions are highly episodic, and hence may have a significant short term impact on the oceans. The results presented here may provide a demonstration of such an impact, and hence represent a useful contribution to our understanding of the global climate/dust cycle. However, prior to final publication I would suggest the authors need to consider a few

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particular issues.

We thank the reviewer for the fair and critical evaluation of our manuscript and appreciate the advice to be more careful when discussing the climate feedbacks of volcanic induced iron fertilisation of the surface ocean by pronouncing the episodic nature of major volcanic eruptions.

1) The results presented here depend on a geographical relationship between the Kasatachi dust cloud and satellite derived estimates of ocean colour in order to make the case for the volcanic plume affecting ocean productivity. Conversion of satellite data to ocean colour and hence to chlorophyll is no trivial task and, since I am no expert in this area, I would suggest the authors need to explain how they have; 1) Corrected for the atmospheric dust in their atmospheric corrections of the satellite signal and 2) Corrected for the presence of dust in the water column in their conversion of ocean colour to chlorophyll.

As already stated in the section 3.1 of our article we use MODIS Aqua Level 3 Chl-a data (8-days and monthly mapped 9-km) processed by Ocean Color Web – see http://oceancolor.gsfc.nasa.gov/forum/oceancolor/topic_show.pl?tid=2372. Level 3 data are made from level 2 data that are already atmospherically corrected. The correction algorithm (used operationally by NASA) is described in http://oceancolor.gsfc.nasa.gov/VALIDATION/atm.html and Gordon and Wang (1994). Based on Shettle and Fenn (1979), 12 different aerosol classifications for different humidities are available, from which only three types (maritime, coastal and tropospheric) are used. As volcanic ash and SO2 atmospheric burden over the NE Pacific Ocean was enhanced only for about four days directly after the eruption of Kasatochi (prior to the phytoplankton bloom!) an influence on the atmospheric radiative transfer on chlorophyll-a detection from volcanic ash and SO2 is negligible. In the revised manuscript it will be mentioned that MODIS level 3 data is already atmospherically corrected.

ACPD
10, C709–C712, 2010

2) The remainder of the calculations in the paper seem to me to be sound and consistent with their case that the volcanic eruption could have stimulated a bloom. If I understand the paper correctly, the dust input took place over a few days in August 2008. The authors argue that the effects persisted until October, and this seems at the upper end of the duration of impacts from deliberate iron addition experiments (Boyd et al., 2007 Science) and this should be considered.

Boyd et al. (2007) distinguish FeAXs (mesoscale iron addition experiments) from FeNXs (Fe natural experiments), where persistent blooms within HNCL waters are observed with Fe supplied by upwelling or recycling. The bloom in the NE Pacific during summer 2008 is one of such sustained blooms, which according to Boyd et al. (2007) can last for months. The bloom area in the NE Pacific in 2008 exceeded that of FeAXs by far. The input of iron attached to volcanic ash, which occurred during a few days only, was the trigger to increase MPP. In the shallow mixed layer of the stratified surface ocean the depletion of phytoplankton took several weeks until October 2008, also visible in the seawater CO2 partial pressure measured at Papa which stayed at reduced levels until the end of October (http://www.pmel.noaa.gov/co2/moorings/papa/data_145w_all.htm). Some more discussion will be added to the revised manuscript.

3) In considering the potential climate significance of volcanic iron fertilisation, the authors need to be careful to note that impacts on primary productivity will only occur in iron limited waters, unless they consider extending their analysis to consider impacts on nitrogen fixation (Jickells et al., 2005) or emissions of other nutrients from volcanoes (Uematsu et al 2004 Geophys Res Lett). Overall, I feel the authors need to be cautious in arguing for a major climate feedback given that the evidence is that the climate impacts of dust fertilisation are modest (Jickells et al., 2005, Boyd et al., 2007), and the impact of large volcanic emissions augmenting this impact must necessarily be rare since since large eruptions are rare and will not necessarily impact iron limited ocean areas.

ACPD
10, C709–C712, 2010

We will weaken our conclusions about the climatic effect of large volcanic eruption on ocean iron fertilisation by pronouncing the episodic nature of volcanic ash induced iron fertilisation events of the surface ocean in the revised manuscript.

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