

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

Anonymous Referee #1

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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 et al 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 particular issues.

The results presented here depend on a geographical relationship between the Kasatichi dust cloud and satellite derived estimates of ocean colour in order to make the

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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.

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.

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 large eruptions are rare and will not necessarily impact iron limited ocean areas.

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

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