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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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Volcanic ash as fertiliser for the surface ocean Langmann et al.

Using satellite imagery, Langmann et al. report an unusually large phytoplankton bloom in the NE Pacific Ocean in August 2008. The authors postulate that the ash clouds generated by the explosive eruption of Kasatochi volcano deposited enough Fe-bearing ash to fertilise the surface of ocean and increase the concentration of chlorophyll across a 1.5-2.0 x 10ˆ6 km2 area.

The authors provide independent evidence that the amount of volcanic ash emitted by Kasatochi is compatible with this idea. They argue that they have established for the first time a direct connection between volcanic ash deposition and biological response of the ocean. Whilst considerable attention is devoted to Fe inputs via mineral dust deposition, the potential of volcanoes for temporarily affecting the surface budget of Fe

may have been overlooked. This contribution provides new observations which support the idea that volcanic ash is a source of Fe for the ocean surfaces. The research will probably trigger interests of a broad scientific community interested in understanding the biogeochemical cycles of Fe and C as their relationships to the evolution of climate. The paper is clearly written and well articulated. I don't have major scientific comments but I provide a list of minor comments that I hope the authors will be able to address before publication of their manuscript.

1. Inconsistent use of 'iron' and 'Fe'

2. p713, L7: Watson 1997 didn't study the release of iron upon exposure of ash to water. This reference should be discarded.

3. p713, L7: Olgun et al. 2010, not 2009

4. p713, L19: Sarmiento's paper (1991) should be cited as well

5. p714, L8: a bit confused here. Why is the comparison made against Hudson and not Pinatubo, which is the most recent "benchmark" in terms of SO2 loading of the stratosphere.

6. p714, L11: How can you already tell at this stage of the paper that Kasatochi ash settled mainly over the NE Pacific? Is this statement based on GOME-2 and OMI images? I think it needs further explanation.

7. p715, L8-9 and L15: '...first evidence for the correlation between the Kasatochi eruption and Chl-a'. This sentence is a bit misleading. Can the authors be more specific?

8. p715, L27: (average increase from 0.5 mg/m3 to 1.0 mg/m3)

9. p716, L4: 0.7 mg/m3 to 1.1 mg/m3

10. p716, L12: 'suggesting a causal connection to the ash...', delete 'causal' from this sentence as establishment of causality will probably require in situ data.

11. p716, L22: It cannot be the strongest evidence if it is implies some forms of speculation

12. p716: I presume that the presence of ash at the surface of the ocean modifies the ocean colour properties. Can this effect be misinterpreted for a change in chlorophyll a concentration?

13. p717, L4-L8: why would a decrease in seawater pCO2 be necessarily related to phytoplankton activity? Are there other possibilities for explaining a temporary reduction in seawater pCO2?

14. p717, L10: Establishment of a 'quantitative link' requires the development of a 'dose (ash flux)-response (chloro a concentration)' relationship, and this is not done here. I suggest that the authors tone down the claim.

15. p718, L21: I'm not sure at all that the cloud dimension as provided by VAAC can be assumed to represent the actual oceanic surface area which received ash. I would stick to the measured surface area of the ocean which showed a chloro-a anomaly (1.5 x 106 km).

16. p719, L12: What is the range of Fe release values reported for ash? I think we don't have the data necessary to assume that 200 nmol/g is a typical value. In addition, Duggen et al. (2007) and Olgun et al. (2010) used Atlantic seawater in their lab experiments, and the release of Fe from ash in contact with Pacific Ocean water may be different, since it is recognised that the biochemistry of the seawater plays an important role in determining iron solubility. Can the authors discuss this problem in a bit more details?

17. p719, L9: The total amount of Fe is 0.9-1.2 x 10ˆ17 nmol Fe (not 0.9-1.0 x 10ˆ17)

18. p719, section 4.3: I would have structured the paper differently here and integrate this section with section 4.2. The central question is how much ash was deposited in the Gulf of Alaska. A constraint on the volume of ash erupted is needed to answer

this question, and the 1-D model could have been used in first place and the outputs compared with Guffanti et al. (2008)'s estimate. Assuming that all the ash reached the ocean, was this amount sufficient to raise the concentration of Fe to ∼2nM (assuming a mixed layer depth of 30 m)? What was the surface area impacted and can this be matched with other independent estimates? And finally, is it reasonable to assume that all the ash emitted was deposited over the ocean and why?

19. p720, L15: Can you provide a reference for the typical temperature of arc magmas?

20. p721, L15-16: Assuming that all the ash emitted is deposited over the ocean surface…which is unlikely. Please rephrase.

21. p722, L30: …Chl-a data do did not show…

22. p723, L4-5: The claim that the data suggest a new feedback mechanism for major eruptions on climate is overstated. The data presented are not sufficient to support this claim. Please rephrase.

23. p723, L8: Are you really looking at MMP increase? I think it's more appropriate to say "Chl-a increase"

24. p723, L14-16: This sentence is a bit confused. Please rewrite.

25. p723, L16: ..would have been were utilised

26. p723, L19-22: This sentence seems a bit contradictory. Pinatubo ash load was 30 times higher than Kasatochi but only a minor part reached the Ocean, and yet CO2 consumption associated with the Pinatubo ash is inferred to be an order of magnitude larger. Can the authors clarify this section?

27. p724, L2: Is it known that the ash from Huaynaputina was deposited in HNLC areas?

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