

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

**B. Langmann et al.**

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Answers to Pierre Delmelle:

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  $1.5\text{--}2.0 \times 10^6 \text{ km}^2$  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

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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.

We thank Pierre Delmelle for his careful review and address each of his minor comments below.

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

In the revised manuscript we will use 'Fe' only in combination with numbers or formula and replace 'Fe' by 'iron' elsewhere.

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

The reference to Watson (1997) will be deleted at page 713 line 7.

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

Will be corrected to 2010.

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

The reference to Sarmiento (1991) will be included here.

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 SO<sub>2</sub> loading of the stratosphere.

We choose Mt. Hudson as it erupted two months after Pinatubo in 1991 and because it is often overseen. We will modify the manuscript by mentioning Pinatubo as well. See point 26 for more discussion about the effects of the eruptions of Pinatubo and Mt.

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Hudson.

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.

We agree, that we haven't shown explicitly at this stage of the manuscript that the Kasatochi ash settled mainly into the NE Pacific. However, we showed already in Fig. 2 the distribution of volcanic ash in the atmosphere during the first three days after the eruption of Kasatochi. As it is well known that the major amount of volcanic ash settles within a few days (e.g. Rose and Durant, 2009), we can conclude from Fig. 2 that most of the volcanic ash must have settled into the NE Pacific ocean. We will add clarifications into the revised manuscript.

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?

We will replace these sentences as follows: '...first evidence for the build-up of Chl-a induced by fertilisation from volcanic ash released from Kasatochi'

8. p715, L27: (average increase from 0.5 mg/m<sup>3</sup> to 1.0 mg/m<sup>3</sup>)

It is not clear what is meant.

9. p716, L4: 0.7 mg/m<sup>3</sup> to 1.1 mg/m<sup>3</sup>

Again: It remains unclear what is meant.

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.

'causal' will be deleted in the revised manuscript.

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

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ulation

We will rephrase this sentence in the revised manuscript as follows: ‘These comparisons further emphasise the fertilisation of the NE Pacific by volcanic ash from Kasatochi as to our knowledge there was no other trigger within the 8-days period.’

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?

According to Duggen et al. (2007) volcanic ash particles can have residence times in the ocean mixed layer ranging from a few minutes up to 2 days dependent as particle size. This short period excludes a misinterpretation in chlorophyll concentration by ash in the surface ocean as we use 8-day composite and monthly mean data.

13. p717, L4-L8: why would a decrease in seawater pCO<sub>2</sub> be necessarily related to phytoplankton activity? Are there other possibilities for explaining a temporary reduction in seawater pCO<sub>2</sub>?

There are, of course. Organic carbon production is one possibility, other possibilities are calcium carbonate production and air-sea gas exchange. In situ-measurements from ship cruises however confirm that phytoplankton growth was the dominant process, as also pH was increasing (Hamme et al., 2010).

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.

We will delete ‘quantitative’ in the revised manuscript.

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 10<sup>6</sup> km).

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We used the VAAC data for a second area estimate to provide an independent alternative to our satellite data analysis to strengthen the overall conclusions of the manuscript.

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?

We agree that many questions related to the amount of bio-available iron attached to aerosols in general and volcanic ash in particular are still unanswered, see e.g. Baker and Croot (2008) who propose standardised and systematic measurements of a wide range of chemical (e.g. acid and alkali concentration, mineralogy, pH) and physical (e.g. particle mass and size, temperature) variables. Olgun et al. (2010) present iron mobilisation of different samples of volcanic ash from subduction zone volcanoes in natural seawater ranging from about 50 nmol Fe/g ash to 300 nmol Fe/g ash after 60 minutes with more samples exceeding 200 nmol Fe/g ash than being below. Therefore, we assume that 200 nmol Fe/g ash is a good compromise. The revised manuscript will be modified accordingly.

17. p719, L9: The total amount of Fe is  $0.9\text{--}1.2 \times 10^{17}$  nmol Fe (not  $0.9\text{--}1.0 \times 10^{17}$ )

Will be corrected in the revised manuscript.

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

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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  $\sim 2$  nM (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?

About the sequence of sections 4.2 and 4.3, it seems to be a matter of taste. We prefer to stay with the chosen sequence. About the assumption that all ash emitted was deposited into the ocean, there are several arguments. First, the island of Kasatochi is only a few kilometers in diameter. Second, as outlined before under point 6 it is well known that the major amount of volcanic ash settles within a few days (e.g. Rose and Durant, 2009), so that we can conclude from Fig. 2 that most of the volcanic ash must have settled into the NE Pacific ocean. Third, according to the modelling study of Langmann et al. (2010), the fall-out into the NE Pacific Ocean makes up 92 % of the total ash mass removed from the atmosphere within eight days after the eruption of Kasatochi. A figure of the modelled amount of ash removed out of the atmosphere is shown in the section with the responses to Peter Croot.

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

Arc magmas are generally more evolved than mid ocean rich basalts and ocean island basalts. However in rare cases there can still be eruptions in an arc setting that are very close to a basaltic composition. Coming up with an average temperature for arc magmas is therefore really difficult. On average we estimate that they are between 900 and 1000°C with exceptions above and below. We refer to Schmincke (2004) for a more general introduction to magmas.

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

See under 18 for our arguments that the volcanic ash of Kasatochi is mainly deposited into the NE Pacific.

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

We will replace do by did in the revised manuscript.

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.

Also the anonymous reviewer and Peter Croot made this point. Therefore we will weaken our conclusions about the climatic effect of large volcanic eruption on ocean iron fertilisation

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

Also Peter Croot pointed out to distinguishing clearly between MPP as rate for carbon uptake and chlorophyll as a proxy for biomass, which will be done in the revised manuscript.

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

We rephrased the sentence as follows: 'The the C/Fe ratio at which phytoplankton from Fe-limited oceanic areas incorporate C and Fe in their tissue (about 1 Å 105; Watson, 1997) allows an estimate of the amount of CO<sub>2</sub> transferred into biomass as a consequence of the 2008 Kasatochi iron-fertilization event.'

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

We will replace would have been by were.

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 CO<sub>2</sub> consumption associated with the Pinatubo ash is inferred to be an order of magnitude larger. Can the authors clarify this section?

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To clarify we will modify page 723 from line 20 on: '... , but it is unclear if terrestrial or oceanic biosphere processes (Sarmiento, 1993) or processes independent of Pinatubo lead to the observed atmospheric CO<sub>2</sub> reduction. The 1991 Pinatubo eruption released about 1.3–1.8 × 10<sup>16</sup> g of ash and thus about a factor of 30 more material than the 2008 Kasatochi eruption, of which, however, only a minor part (few percent) could have reached the Fe-limited Southern Ocean for fertilisation (Watson, 1997; Sarmiento, 1993). Interestingly, the direct deposition of volcanic ash from the Mt. Hudson eruption in Chile in 1991 into the iron-limited Atlantic sector of the Southern Ocean (Scasso et al., 1994) was not even considered in explaining the observed CO<sub>2</sub> drawdown of the 90ies, although the estimated amount of Mt. Hudson ash (6.5 × 10<sup>15</sup> g) which settled into the Southern Ocean exceeds the contribution from Pinatubo by far.'

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

Yes, in de Silva and Zielinski (1998) a tephra map over land is provided clearly pointing to further dispersion of volcanic ash released by Huaynaputina over the equatorial Pacific which is known as a HNLC area.

#### References

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