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Biogeosciences Discussions

Interactive comment on "The role of airborne volcanic ash for the surface ocean biogeochemical iron-cycle: a review" *by* S. Duggen et al.

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The paper submitted by Svend Duggen and co-authors provides a systematic review of the interaction of volcanism and the biogeochemical iron-cycle. As this is an emerging field of scientific study, this review is both warranted and acceptable for publication in Biogeosciences. The manuscript is easy to read, informative, thorough, and will be of use to scientists from a wide range of disciplines. I have a few minor suggestions and comments, which are detailed below.

Yours sincerely

Morgan Jones

Minor Comments:

Abstract –

My understanding is that fine ash will settle out through gravitational sedimentation of weeks to months, rather than years (see Robock, 2000). This will still allow ash to be transported to the more remote oceanic areas. I suggest rewording accordingly.

Volcanism was present before the oceans, so it can be said that the possibility of iron injection to the oceans has occurred throughout Earth's history (instead of "much of").

Introduction –

Section 1.2. The cited references for iron fertilisation experiments (starting Blain et al) should either be in alphabetical or date order.

Sarmiento's 1993 paper also suggested the onset of an El Niño after the eruption could have also assisted in the relative drawdown of CO2. Whether the change in Pacific conditions had anything to do with the Pinatubo eruption is unclear, but larger eruptions are predicted to cause changes in overturning and ocean circulation (see Jones et al., 2005; Jones et al., 2007).

Glaciation-volcanism interactions. Another possible cause of increased volcanism from deglaciation (aside from sea level change) is the changes to crustal stresses associated with inland ice removal (see Zielinski et al., 1997).

Satellite evidence. Aside from the submitted Langmann paper (which I haven't seen), the satellite evidence for a phytoplankton bloom is still somewhat inconclusive. Is there enough evidence from the Montserrat data to separate the chlorophyll signature from the background signal? My understanding from Fred Prata was that the data was still too inconclusive to be attributed to a bloom.

Paragraph beginning "Recent estimates…" The authors refer to the acronym SVZA before detailing what it stands for. I am also not sure if such generalised distinctions can be made between ash samples from a subduction zone setting and from hotspot settings, given the range of compositions in each category. Both settings are capable of producing everything from basalts to rhyolites, which are likely to have markedly different iron contents, mineral assemblages, weathering rates, and volatile compositions. The enhanced Cl and F concentrations quoted by the authors is not unknown for hotspot volcanoes. Without seeing the Olgun et al. paper in preparation, I think there dataset is currently too small to make such distinctions.

Section 2. Subaerial 'and' submarine volcanic eruptions, instead of 'or'.

Section 3. The transport of heavy metals can be as F and S complexes, as well as Cl.

Sentence beginning "Thesalt coatings" should be "The salt coatings"

Section 4. Sentence beginning "Further constraints..." Should be 'subduction zone volcanic ash' or 'SVZA' instead of 'subduction zones volcanic ash'.

Section 4.1.1. and 4.1.4. Again, the assertion that the decay of HSVA samples with time does not occur I think is unfounded. As the authors suggest, the number of pristine samples currently tested is extremely few, but they acknowledge the discrepancy in Hekla ash between the Frogner et al (2001) and Jones and Gislason (2008) studies. While these studies used different sieve fraction, the change between the two experiments appears to be greater that the change in relative surface area. The 45-125 μm fraction used in the latter study included more vesicular scoria particles, which offsets the increase in surface area expected in the 44-74 μm sieve fraction used by Frogner et al (2001). Moreover, prior to the 2000 eruption of Hekla, Paul Frogner and co-authors attempted the same experiments in 1999 with pristine ash from the 1980 eruption of Hekla. The results were never published as there was minimal element fluxes and pH change, despite Oskarsson (1980) detailing considerable adsorption and dissolution at the time. Therefore, I think the decay of HSVA ash-leachates cannot be discounted, nor separated from SVZA samples in this manner.

Section 4.1.6. There 'are' two principal ways...

References: Jones G. S., Gregory J. M., Stott P. A., Tett S. F. B., and Thorpe R. B.

(2005) An AOGCM simulation of the climatic response to a volcanic super-eruption. Climate Dynamics 25(7-8), 725-738. Jones M. T., Sparks R. S. J., and Valdes P. J. (2007) The climatic impact of supervolcanic ash blankets. Climate Dynamics 29(6), 553-564. Robock A. (2000) Volcanic Eruptions and Climate. Reviews of Geophysics 38(2), 191-219. Zielinski G. A., Mayewski P. A., Meeker L. D., Gronvold K., Germani M., Whitlow S., Twickler M., and Taylor K. (1997) Volcanic aerosol records and tephrochronology of the Summit, Greenland, ice cores. Journal of Geophysical Research 102(C12), 26,625-26,640.

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