

Interactive comment on “Ash iron mobilization in volcanic eruption plumes” by G. Hoshyaripour et al.

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This paper addresses an important field of study, namely the in-plume interactions between solid and volatile species. The approach is rigorous, well thought out, and for the most part stands up to scrutiny. There are a few areas where I believe clarifications and/or slight alterations will strengthen the manuscript, but otherwise I believe this is worthy of publication here. I have made a couple of suggestions for additional references; the full citations are at the end of the text. The specific points are:

1) I believe too much of the focus on nutrient release from volcanic ash concerns iron. While very important and a key limiting nutrient in some open ocean settings, it is not the only nutrient to do so. Nitrogen, P, Si, Ca and many transition metals are all

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important to the healthy functioning of an ecosystem, and the fertilisation potential of volcanic ash deposition reflects that many of these elements are present in soluble surface ash coatings. HNLC regions are only ~20% of the world's oceans, so this focus on just one element is counterintuitive. I suggest a revision to use Fe as an important nutrient and also a proxy for poorly soluble metals, rather than the inferred suggestion that it is the sole driver of primary productivity.

2) There have been recent studies (e.g. Pearce et al., 2013) using basaltic fluvial sediment and isotopic analysis that show that insoluble elements (in this case Nd, but the same would apply for Fe) rapidly change the isotopic composition of seawater once they arrive in estuaries without changing the dissolved concentrations. This means that there are fluid-rock interactions (dissolution and precipitation, and/or element exchange) that occur in saline fluids could release Fe from glass and minerals without the need for surface coatings. In other words, ash iron may not be as insoluble as first thought, due to the effects of salt, organic breakdown, or a combination of factors. An article currently in press by Morin et al. (2015) would be good to read as well if it is published in time for this revision.

3) Line 25: There are previous investigations that showed a link between ash deposition and fertilisation, namely Uematsu et al. (2004) and Censi et al. (2010). These should also be cited here.

4) Lines 55-60. It is not clear from the current text why Fe-salts are found in insufficient quantities. Please clarify, and add the caveat that while these species were clearly not the main source of Fe from Kasatochi ash, this is still a sample size of one.

5) Given the range of volcanic ash compositions within each tectonic setting, I do not find it useful to classify volcanic products on the basis of convergent plate, divergent plate and hotspot. There is more in common between basaltic volcanism from all of these settings than the range of eruption chemistries at convergent plate boundaries. This section still merits attention, but I think it needs altering to reflect explosive prod-

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ucts are likely to be silica rich and/or phreatomagmatic. With the latter, the washing/incorporation of acids by excess water is an important factor for the availability of acids for leaching. The authors even acknowledged that the initial subdivision by Olgun et al (2011) was based on a limited dataset. The extensive review by Witham et al (2005) should also be referenced here. The message here that melts with high Cl (which may be more prevalent at convergent boundaries, but also coastal volcanoes such as Surtsey) and higher acid species are more likely to release more Fe from solid phases.

6) The abbreviations CP, DP and HS confuse matters and should be deleted.

7) Table 3 – please put in the caption where this data is from. Also mention in the main text that these are just a snapshot of volatile speciation, and that volcanic gas chemistries can vary considerably.

8) Figure 2. – Recent ash-fall particle size measurements from Eyjafjallajökull and similar have shown that in some instances the fine fraction can be considerably greater than measured by Rose and Durant (2009). Given that this would strongly affect the available surface area from nucleation, it is worth mentioning here.

Aside from these minor revisions, I think this is a strong piece of work and a positive advancement of our understanding of in-plume processes.

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B) Morin, G.P., Vigier, N., Verney-Carron, A., 2015. Enhanced dissolution of 1 basaltic glass in brackish waters: impact on biogeochemical cycles. *Earth Planet. Sci. Lett.*, in press.

C) Pearce, C.R., Jones, M.T., Oelkers, E.H., Pradoux, C., Jeandel, C., 2013. The

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effect of particulate dissolution on the neodymium (Nd) and Rare Earth Element (REE) composition of seawater. *Earth Planet. Sci. Lett.*, 369, 138-147.

D) Uematsu, M., Toratani, M., Kajino, M., Narita, M., Senga, Y., Kimoto, T., 2004. Enhancement of primary productivity in the western North Pacific caused by the eruption of the Miyake-jima Volcano. *Geophysical Research Letters*, 31, L06106.

E) Witham, C.S., Oppenheimer, C., Horwell, C.J., 2005. Volcanic ash-leachates: a review and recommendations for sampling methods. *J. Volcanol. Geoth. Res.* 141, 299-326.

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