

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

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We thank the reviewer for the comments that helped us to improve this MS. The suggestions are applied in the revised MS. The questions and comments are answered below.

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 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.

Reply: We completely agree with the reviewer about that one should not overlook the role of other nutrients rather than iron in the primary productivity. A short sentence is added to the revised MS to highlight this important point.

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.

Reply: This is a very interesting point. It implies that iron mobilization can take place within the aquatic environments after deposition. This effect could be however limited by the residence time (sedimentation rate) of the ash particles in the surface ocean (Lindenthal et al 2013). But it definitely requires more detailed investigations to distinguish the iron mobilization happening in the eruption cloud and the surface ocean.

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.

Reply: The suggested literature is added to the revised MS.

4) Lines 55-60. It is not clear from the current text why Fe-salts are found in insufficient

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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.

Reply: Ayris et al. (2014) does not provide any quantitative measure that can be used to estimate the amount of iron chloride salt formed in the experiment. Therefore, it is difficult to evaluate their claims concerning iron mobilization in volcanic ash. The text is revised to better clarify this point and the suggested remarks are added to the revised MS.

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 products 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 acknowledge 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.

Reply: As explained in the MS, we use tectonic settings to represent the variations in the magmatic gas composition and not the ash composition. But we agree with the reviewer that this partitioning might be confusing. Therefore we replaced the names convergent plate (CP), divergent plate (DP) and hot spot (HS) with halogen-rich, carbon-rich and sulfur-rich gas, respectively. The suggested literature is also added to the revised MS.

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

Reply: The abbreviations CP, DP and HS are replaced with halogen-rich, carbon-rich

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and sulfur-rich gas in the revised MS.

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.

Reply: The caption is modified and the suggested remark is added to the revised MS.

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 great than measured by Rose and Durant (2009). Given that this would strongly affect the available surface area from nucleation, it is worth mentioning here.

Reply: The suggested remark is added to the revised MS.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 32535, 2014.

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