

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

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Received and published: 6 February 2015

The objective to model in-plume processing and acid condensation onto ash surfaces is both useful and laudable. However, I have some concerns about the use of the model ash compositions in the paper.

The authors use proportions of Fe-bearing phases in their ash which are far in excess of realistic values. For example, in their final comparison between model output and natural ash systems, they use composition B (70% glass + 30% hematite) to derive the values in Table 5. Such a composition does not exist in any of the ash systems which the authors compare to, nor in volcanic ash generally. The authors will note that the reference cited in connection with this matter, Nakagawa and Ohba 2003, contains no

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mention of hematite.

An additional concern is that the authors use the equations for steady-state dissolution in their calculations of proton consumption by silicate glass. As these equations are for steady-state dissolution, there is no consideration of the rapid leaching of Na, K, Ca, Mg, and to some extent Fe, that occurs during the early stages of acidic dissolution. This will likely lead to much faster consumption of protons than predicted, limiting the duration of low pH conditions at the ash surface, and presumably the extent of Fe mobilisation.

In both of the above cases, the modelled systems are both chemically and mineralogically dissimilar to volcanic ash. However, the authors claim that there is good agreement between their theoretical approach and experimental measurements. If the two systems are different, the agreement of calculated values with experimental data has little scientific relevance, as the two systems cannot be causally linked.

Can the authors either comment on the rationale and utility of their selected model systems, or incorporate additional data and parameters into their model to replicate more realistic ash compositions?

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

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