Atmos. Chem. Phys. Discuss., 12, C8289–C8290, 2012
www.atmos-chem-phys-discuss.net/12/C8289/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Composition and evolution of volcanic aerosol from eruptions of Kasatochi, Sarychev and Eyjafjallajökull in 2008–2010 based on CARIBIC observations" *by* S. M. Andersson et al.

Anonymous Referee #1

Received and published: 17 October 2012

This manuscript describes the collection and chemical analysis of volcanic aerosol in the lower stratosphere / upper troposphere using an airborne platform and presents an approach to estimate the atmospheric residence time of volcanic sulfur dioxide from the experimental data. Considering the general difficulty of obtaining volcanic aerosol samples directly in a plume, the CARIBIC platform represents an ideal tool for such investigations, the collected data set is very comprehensive and of large value for the scientific community. The description of the applied techniques as well as the presentation of the data is scientifically sound, and I recommend the manuscript for publication

in ACP after addressing one critical issue regarding the interpretation of the data.

The authors calculate the residence time of SO2 with help of the Fe/S mass ratio. They argue correctly that the obtained residence time is underestimated due to a higher sedimentation velocity of ash compared to the sulfate particles. Furthermore, the authors also point out that e.g. within the Eyjafjallajökull plume, the ash mode diameter was considerably higher even far away from the source (larger than 2 micrometers, see e.g. airborne measurements by Schumann et al. (2010) and Bukowiecki et al. (2011, same ACP special issue, airborne measurements) than measurable with the size cutoff of the CARIBIC instrumentation (2 micrometers). This means that, at least for the Eyjafjallajökull data, only the lower size end of the ash mode was analyzed. Doesn't this have a drastic influence on the Fe/S mass ratio which is subsequently used for the calculation of residence times? Furthermore, what does a change of Fe mass from one filter to the next mean? Is it a) due to a change in plume concentration (dilution), or is it b) rather due to a change in size distribution (fractionation)? What impacts do these two scenarios a) and b) have on the overall findings, and is it possible at all to separate these two situations? These questions need to be addressed in more detail by the authors; otherwise the deduced findings remain somewhat diffuse. I suggest including a estimation of uncertainty (at least semi-quantitative), if this is not possible the limitations of the interpretation should be clearly emphasized throughout the manuscript (also in the abstract).

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 21481, 2012.