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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Volcanic ash from Iceland over Munich: mass concentration retrieved from ground-based remote sensing measurements" *by* J. Gasteiger et al.

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This paper presents a method to estimate the mass concentration out of light extinction measurements of the Ejyafjolla plume that reached Munich in the morning of April 17, 2010. The authors exploit the observations made by means of 3-wavelength, 2 Raman and 2 depolarization-channel lidars as a constraint to an aerosol scattering model to find all the possible non-spherical, monomodal size distributions that match such measurements (2 extinction coefficients, 3 backscatter coefficients, 2 depolarization ratios). The compatible distributions are then used to compute volume and mass of the particles, assuming a particle density of 2.6 g cm-3. The mass to extinction ratio (m/ext) is

then evaluated out of these results.

This exercise represents an original step forward with respect to the first estimates made out of the same observations by employing the single value (1.95 g m-2) OPAC mass to extinction ratio of Saharan dust (Ansmann et al., GRL 2010). Still, the outcome of the paper is similar: a single coefficient (m/ext=1.45 g m-2 at 532 nm), obtained at one location, over one hour of measurements, at a single altitude and for a limited extinction range, is proposed as representative of the mass to extinction ratio of the rather complex Ejyafjolla volcanic plume. Even though the authors admit the uncertainty of the method to be large, this could be much larger if the plume had not been assumed as made of just crustal particles, i.e., not monomodal and mono-component (as instead observed by the DLR Falcon (Schumann et al, ACPD, 22131, 2010), and at the Jungfraujoch (see below).

In this respect, I believe the paper could be improved by answering/addressing the following points:

The aerosol scattering model employed in this manuscript is analogous to the one developed by Barnaba and Gobbi, (JGRd, p3005, 2001), later employed to provide the mass to extinction ratios of Saharan dust published in Barnaba and Gobbi (ACP, p2367, 2004, Figure 13). This latter figure showed the ratio to have a strong dependence on the distribution extinction coefficient, i.e., modal radius (m/ext=0.47 g m-2 at Ext550= 10 Mm-1, m/ext=1.3 g m-2 at Ext550= 100 Mm-1, and m/ext=2.16 g m-2 at Ext550= 300 Mm-1 (particle density 2.6 g cm-3)). As a consequence, attributing extinction to coarse rather than fine mode particles strongly increases their estimated mass. Why the authors search for a single conversion factor when their m/ext frequency distribution (Fig. 3) shows the presence of an analogous large variability of such ratio as a function of particles size? This choice can seriously impact an extended application of their method. Wouldn't the paper benefit from exploiting the size (or extinction) dependence of the computed ratios?

Observations made by the DLR Falcon (e.g. Fig.7 in Schumann et al, ACPD, p22131, 2010), on April 19, 2010 showed the plume size distribution to be at least bimodal, with most of the mass located in the coarse mode and most of the surface (i.e., of the scattering) in the fine mode, likely made of liquid sulphuric acid or sulfate droplets. Observations made by the PSI at the Jungfraujoch (300 km SW of Munich) on April 18 and 19 showed similar conditions, i.e., scattering in the plume to be generated mainly by submicron particles. How can the authors be sure the plume extinction coefficients they observed were generated by monomodal, coarse ash particles alone, with optical properties as in their Table 3? Even if such condition were satisfied, the authors should made clear in the paper the conversion factor they derive is specific to the state of the plume they observed, i.e., it cannot be generalized.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C11836/2011/acpd-10-C11836-2011-supplement.pdf

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