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Atmospheric
Chemistry
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Discussions

# *Interactive comment on* "Physical and optical properties of 2010 Eyjafjallajökull volcanic eruption aerosol: ground-based, LIDAR and airborne measurements in France" *by* M. Hervo et al.

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**Reviewer: You derive mass extinction coefficient using 550 nm scattering properties an 637 nm absorption properties and apply to a 355 nm LIDAR. How is the mass extinction going o differ between 355 nm and 550 nm and how does this uncertainty propagate to LIDAR retrieved mass concentrations?**
Author: The following sentence is now included in section 2.1: "The scattering properties are calculated at 355 nm using the scattering Ångström exponent ($\alpha$). According to previous studies the uncertainties on the scattering coefficient can be consider of 7%

(Anderson and Ogren, 1998;Anderson et al., 1999). After propagation (Wagner and Silva, 2008), the value of the error in the calculation of $\alpha$ was determined ($\Delta\alpha$=0.32). Extrapolating the scattering coefficient at 355nm, from measurements at 450nm and the Ångström exponent, generates an error of 19% with the following equation:

$$\Delta\sigma \mathrm{sca}(355)/\sigma(\mathrm{sca}(355) = \ln(637/450)\Delta\alpha + \Delta\sigma \mathrm{sca}(450)/\sigma \mathrm{sca}(450)$$

The absorption coefficient is calculated at 355nm with an Absorption Ångström Exponent (AAE) equal to unity (i.e. the absorption is directly proportional to the wavelength variation: $\sigma$abs(355)≈637/355*$\sigma$abs(637)). AAE is correlated with aerosol composition or type: for most urban industrial site, the AEE value is close to one but can increase until 2.5 for others kind of particle (Russell et al., 2010). Using an AAE of 1 ($\sigma$abs(355)=1.79*$\sigma$abs(637)) instead of 2.5 ($\sigma$abs(355)=4.31*$\sigma$abs(637)) implies an important error on the absorption coefficient by a factor of 2.41. Nevertheless, as the single scattering albedo is relatively high (0.97±0.01) during the measurement period (i.e. the scattering coefficient is significantly higher than absorption coefficient) the absorption impact on the extinction is limited. Indeed the extinction was calculated for both AAE of 1 and 2.5 in order to estimate the error generated by such an approximation. When using the AAE of 1 instead of 2.5, the extinction was underestimated by only 4.2% on average with a 10.4% maximum on the period considered in this study. The same error will be propagated to the mass-to-extinction ratio $\eta$ and to the LIDAR mass concentration.”

**R: Some reasons for differences between measurements and model are suggested. One is in-cloud sulphate production. O’Dowd et al., 2011, measured of the order of 10 micrograms/m3 sulphate in the plume.**
A: This is a useful reference that we now include in the paper.

**R: You can retrieve ash layer thickness in the Free Troposphere. Is the dilution factor of the plume after mixing into boundary layer or in-situ measurement layer consistent with the ratio of in-situ mass to LIDAR derived concentrations in the**

**FT ash layer aloft?**

A: On the 18 May ashes were detected at 2300 to 3100m a.s.l. From 6:00 to 7:00 UTC with a LIDAR extinction of 502±46Mm-1. The mass concentration retrieved was 788±79$\mu$g.m-3. It leads to an integrated total mass of 630400$\mu$g.m-2 in the plume. On the 19 May ashes were detected in the Boundary Layer. The mass retrieved was 608 $\mu$g.m-3. If we consider a Boundary Layer thickness of 1800m (from the ground to 2200m a.s.l.), the total mass retrieved is 1094400$\mu$g.m-2. Corresponding to 57% of the mass measured previously. This mass is coherent with the in situ measurements presented in section 3.1.3, were we show that the ashes represent roughly 60% of the total mass. It is also consistent with the entrainment of volcanic ash into the boundary layer simulated by FLEXPART. However it is highly dependent of the heterogeneity of the ash layer and it cannot explain the mass concentration difference between LIDAR and in-situ measurements. This is now mentioned in the manuscript.

**R: "it suggests that the volume size distribution is not dominated by the ashes but by the accumulation mode centred on 350 nm." This implies you do not consider ash particles occurring in the accumulation mode! I believe that this is an unfounded assumption, can you substantiate it?**

A: This sentence has been replaced by: "it suggests that the volume size distribution is not dominated by the supermicronic ashes but by the accumulation mode centred on 350 nm (including ashes and anthropogenic particles)."

**R: What can you say about water uptake or CCN properties?**

A: We did not measure hygroscopic properties during the presence of ash particles.

**R: There is a special issue in Atmospheric Environment which contains related papers. One relevant paper is the Mace Head characterization of the ash plume (O'Dowd et al., 2011) which warrants a mention in the introduction as well as in the discussion section.**

A: This paper was not available while we were preparing the paper. It is now included.

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