The paper presents results of observations of a volcanic ash plume observed over Munich, Germany, in late spring 2010. Data were collected with lidar, sunphotometer, and ceilometer. The authors present mass concentrations which is an important parameter needed for the assessment of the radiative impact of volcanic ash plumes. Furthermore aviation security would strongly benefit from estimates of mass concentration. The mass concentrations are derived from T-matrix and geometrics optics computations. The authors use a tool, called HYPERFUN which allows to model non-spherical particle geometries in a more realistic way than what is usually done.

The authors provide some valuable results on particle size of volcanic ash particle. Little information on such particles is available; see for instance Munoz et al. The authors need to address several points, as listed below:

1) It remains unclear in how much the author deal in their computations with the fact that the observed plume likely did not consist of non-spherical ash particles only. There must have been sulfuric acid particles present.
2) In view of 1) what is the effect on the results if two different refractive indices (two different particles types) are used?
3) The authors should address in more detail the issue of choosing an appropriate axis ratio distribution. To my understanding the authors use results from Kandler et al., who present axis ratios for mineral dust particles observed over the Sahara (SAMUM experiment in Morocco).
4) How much proof do the authors have regarding the applicability of a log-normal size distribution in their computations? Volcanic ash may not show such a shape of the size distribution.
5) Do the authors have proof for the completeness of the solution space (in the mathematical sense)? There may be "gaps" in the grid of parameters they use in their computations. The parameters of the particle size distribution and particularly the complex refractive index are quite restricted. For instance, a denser grid could result in different mean values. Furthermore, the uncertainty level might increase if a broader range of complex refractive indices is used in the computations.
6) Considering 5) in the computations could change the mass estimates.
7) The authors present ratios of radiances in their consistency check (section 4, page 14). They use two angles and the wavelength 1020 nm. I agree that 1020 nm is the best wavelength to consider. However, are the results consistent with computations at another wavelength? Particle shape may have some influence on the computations at different wavelengths, and in fact this test might tell a bit more about particle shape itself.
8) The authors compare their results from the radiative transfer model to lidar observations? This comparison raises two questions: i) how do the authors account for the overlap effect? ii) How do the authors treat the fact that there is urban pollution in the boundary layer, whereas volcanic ash is on top.
9) The authors compare lidar in Maisach with sunphotometer at the university in Munich. How is the spatial difference accounted for? The data sets describe different portions of the plume, and the plume certainly was rather

inhomogeneous. I assume that number concentration varied quite a bit, and particle size may have change, too.

10) Page 15, first full paragraph: the authors say that they use a scattering angle of 3 deg and 4 deg for their computations. Is there any reason that no angle closer to the sun disk was used?
11) On page 15 the authors say that "the largest wavelength of the CIMEL is best suited … less affected by the boundary layer aerosol." So let me ask again: are the results consistent with computations done at another wavelength? Or let me rephrase: what does "less affected" mean in quantitative terms?
12) Page 15, 4.2, MYSTIC: does this code treat particles of non-spherical geometry? From the text it does not become clear if MYSTIC applies Mie theory. In view of the unusual measurement situation it may be crucial that all tools used in this study (lidar, cimel, radiative transfer, forward simulations) can treat the particles on the basis of non-spherical scattering theory.
13) Page 16: how did the authors consider the fact that the AERONET algorithm cannot derive particle size distributions for which particle radius is above 15 mu? Papers by Mueller et al. (JGR 2010, paper # D07202; SAMUM results) and Mc Connell et al. (JGR 2008, paper # D14S05; DODO campaign) point out to this problem.
14) Page 20, second paragraph: the authors say that their results are consistent with AERONET retrieval results. Again let me point out that AERONET cannot retrieve effective radii larger than around 2 micrometer because the retrieved particle size distributions are restricted to 15 micrometer. The authors should discuss this specific effect in more detail.

Specific comments:

Abstract: The abstract should contain more information on the implications.

Page 2: The authors refer to Jaeger et al. As they speak of lidar techeniques they should are refer to papers by Ansmann et al, Wandinger et al., Carswell et al. (Raman lidar).

Page 3, first line: "in the order" should be "on the order".

Line 2: "few years" should be "several years".

Line 3: in the order should be on the order.

First full paragraph on page 3: please rephrase the sentence "During the eruption of the …. effects for Europe". The sentence is difficult to understand.

Page 3, line 13: "was known" should be "is known".

Page 3, line 6 from bottom: "allows" should be used with "allows for…" or "allows one to …" I find this wording "allows" in many spots of the paper. Please make changes accordingly.

Page 5, line 3: please provide the geographical location of the Iceland volcano.

Page 5, line 13: delete comma before “7 km”.

Page 6, last line: no comma after “because”?

Page 7, line 2 from the bottom: “an upper limit”. Please specify.

Page 8, first line: what means “improved geometrics optics code”? Please specify.

Page 8, line 3: You write “better reliability in the lidar-relevant backscattering direction”. What means “better reliability”? Please specify.

Page 8: “are weighted according to the actual …”: please explain this procedure in more detail. It is unclear how this is done.

Page 9, effective radius definition: you write 3V/4A. It should be 3V/A.

Page 13, you refer to Patterson et al 1983. You should point out what Patterson et al actually measured, and whether it is comparable to your kind of data. Keep in mind that mineral dust may be highly variable. There likely is no “unique” value for the refractive index.

Page 16, line 7/8: how do you treat particles in the overlap region (Angstrom exponent assumption)?

Page 16, line 12: you consider an aspect ratio of 1.8 in your computations. Is this a representative value, given the fact that the ash particles possess an aspect ratio distribution.

Table 4: I am a bit confused about the numbers. Why do you assume the possibility of rather high absorption (imaginary part of up to 0.05) in the boundary layer, whereas you neglect this possibility for the volcanic ash plume? Your effective radius is up to 3 micrometer in the volcanic ash plume. Is there a reason why you do not test any larger value for reff? Simply give it a try regardless of what Angstrom exponents tell you.