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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Optical, microphysical and compositional properties of the Eyjafjallajökull volcanic ash" *by* A. Rocha-Lima et al.

Anonymous Referee #2

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The manuscript "Optical, microphysical and compositional properties of the Eyjafjallajokull volcanic ash" by Rocha-Lima et al., describes the physical, chemical and optical characterization of a sample of ash taken from the vicinity of the Eyjafjallajokull volcano after it erupted in 2010. I think the data the authors are presenting is of interest; however, the manuscript is lacking a thorough discussion of their results, and a thorough error analysis. I do not recommend publication in ACP in the present form. Following are my recommendations for the manuscript to be suitable for publication.

One of my main concerns is the retrieval of the imaginary part of the RI. First, from the calculation of the mass absorption efficiency (αabs) using Eq.1, the authors assume

that the attenuation is only due to absorption. It is not clear to me that this assumption is correct and "Light scattered forward by the particles will most likely hit the white surface of the filter underneath and scatter backward on its path back to the spectrometer" is not a strong justification and the authors are completely ignoring multiple scattering in the filter. The authors should include either a supplementary section to support their assumption, or add it to the main text. Second, about the size distribution analysis, it is not clear how the authors take into account the clear over-sizing and under-counting by their ImageJ software and how errors in the determination of the number, area, and volume distributions will translate into the retrieval of the imaginary part of the RI. An error analysis is also needed here. Finally, the authors calculate αabs using Eq. 2 by changing the m to get a value for Qabs to calculate the mass absorption efficiency and minimize the difference with the mass absorption efficiency calculated from Eq. 1 (taken from the reflectance measurements). When they reach a minimum in their minimization procedure, they take the inputted m as the derived value. The authors should plot αabs from Eq. 2 (from the spherical assumption and from the T-matrix calculations) in Fig. 8. A minimum difference can always be achieved but showing that the αabs using Eq. 2 is equal (or within error of the αabs calculated with Eq. 1) will show the robustness and reliability of their retrieval. Also, what is their convergence criterion?

The authors mention on Section 2.6 that energy dispersive X-ray fluorescence analysis (EDXRF) of the ïňĄne and coarse particles was used to investigate dependence of the refractive indices with chemical composition, but they do not elaborate, there is no mention of what might be (or is) the dependence. They just conclude the manuscript stating that further studies will be needed to explain the differences in optical properties observed between fine and coarse particles. This section should be elaborated. From their introduction, it is also not clear what is the main purpose of the paper. The authors mention aviation in their introduction, but they don't state or elaborate on how will these results help aviation? They also mention modeling and remote sensing. How will their results improve modeling and/or remote sensing? What are the implications of

their results in comparison with previous publications? Where do the differences come from? etc.

Other comments: The abstract should be re-written. It states the different methods used to do the measurements, but it does not give a concise summary of the results that the authors are presenting.

Why is a value of the aspect ratio of f=3.0 is 'certainly overestimating' the effects of shape? From the filter is clear there are some extreme shapes.

Can the authors expand on why they think their results are different that the previous published values of the RI?

What is the error in the density measurements? Is the value in parenthesis (13) the error? If yes, add a $\pm$. Why is the density value lower than previous reported values?

In the discussion section the authors began by stating that the αabs values are 'different' for the fine and coarse modes. However, it is clear from Fig. 8 that the difference only arises below around 600 nm, and 0.1 m2/g is not a big difference. Then the authors state that the imaginary part between the modes is only 'slightly different', but the imaginary part of the RI is proportional to αabs. The authors should be consistent in their statements.

It is incorrect to say imaginary refractive index; this should be changed throughout the text to imaginary part of the complex refractive index

Page 13273, lines 3-4: what does 'respectively' refer to? The phrase is not clear.

Page 13273, lines 8: 'environment' should be 'environmental '

Page 13275, line 3: add 'the' after (B)

Page 13278, line 6: For the equation αabs = G/(2a), the 'a' should be 'σ', as it was defined in equation 1.

Page 13279, line 13: High imaginary indices. . . High with respect to what?

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 13271, 2014.