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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Optical properties of meteoric smoke analogues" *by* Tasha Aylett et al.

Anonymous Referee #1

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This manuscript reports the combination of two experiments in order to quantify complex refractive indices (i.e., real and imaginary parts)of maghemite (gamma-Fe2O3). This is an important data set since iron oxides are among the candidates for meteoric smoke particles which in turn have been proposed to be involved in a wide variety of atmospheric phenomena.

In order to derive both the real and imaginary parts of refractive indices, two data sets were analyzed: one from a photochemical aerosol flow reactor in which the extinction (scattering plus absorption) of iron oxide analogues was measured in the wavelength range 325-675 nm. These data were then combined with maghemite absorption coefficients measured in an independent experimental system.

Overall the manuscript is very well written and certainly warrants publication provided that the following minor comments are taken care of:

1) There is only one critical point that I see and that is that in the two experiments the particles were generated using completely different experimental techniques. Arguments are presented that allow to identify maghemite as the likely composition of particles in both experimental setups. However, in particular with regard to Figure 3, I am wondering if the authors could try to be a bit more compelling that the particles are indeed made of maghemite. In particular, I would like to see a direct comparison of the measured spectra with the shown spectra for iron oxide standards. While I do agree that some features of maghemite fit the measured spectra well, others do not appear to fit perfectly. For example in the Fe L edge region at 720-725nm the reference spectrum for maghemite shows a clear double peak which I can't see in the measurements. I recommend to plot one spectrum on top of the other and be a bit more critical in the attribution. Also a cautionary note with regard to the proper identification should be added to the conclusions and abstract.

2) Line 204: Section 3.2 should be 3.1

3) Figure 4: The two panels have different wavelength-ranges which confused me initially. This should at least be indicated in the figure caption.

4) Line 309: Please provide a reference for the statement that the mobility radius is an upper limit to the fractal radius.

5) I might have missed it: in Figure 5, radii from 30-100nm, there is a clear mismatch between the measured size distribution and the lognormal fit. This should at least be stated and possibly discussed in the text.

6) Figure 10 and lines 415-418: In the text the authors write that the literature data is for hematite and not maghemite. Subsequently they use the real refractive indices for hematite. Is this really consistent? I admit to be confused. Please explain.

7) Line 443-445: The authors argue that since maghemite emerged as the dominant species in the laboratory experiments it might also play a role in the atmosphere. Can

this inference really be drawn? How comparable are the conditions in the laboratory and the atmosphere?

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