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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement and modeling of the multi-wavelength optical properties of uncoated flame-generated soot" *by* Sara D. Forestieri et al.

Anonymous Referee #1

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Review of 'Measurement and modeling of the multi-wavelength optical properties of uncoated flame-generated soot'

Overall impression

The manuscript 'Measurement and modeling of the multi-wavelength optical properties of uncoated flame-generated soot' by Forestieri et al. presents a detailed analysis of laboratory studies of black carbon particles to rationalize a more appropriate (Rayleigh-Debye-Gans approximation) that uses a constant mass absorption coefficient that is size independent, but wavelength specific treatment of uncoated BC particles within climate models. The authors present a clear question, clearly answer it and the findings

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Discussion paper

fall within the scope of ACP. The manuscript is technical, well-written, and provides a clear connection between laboratory experiments and conclusions. I have no major comments and recommend this manuscript for publication.

General comments

Significant time is devoted to placing these measurements in context, but there is frequently no suggestion as to why these measurements are different – when they are that is. I suggest providing the reader some guidance as to the authors view on some differences.

While these data support the conclusion presented in the abstract, it appears from Figure 3 that RDG theory over-predicts the MAC at smaller particle sizes when Mie Theory appears to do a better job, would a combination of these two separate approaches result in a better parameterization? It seems to be that the problem with black carbon absorption may simply be shifted from an underestimation with Mie Theory to and overestimation with RDG.

It's noted in the conclusions that future works will consider coatings, I look forward to seeing this work.

Specific comments

Pg 2, Line 6: consider defining Mie size parameter explicitly.

Pg 3, Line 19: 'fuller' seems out of place, perhaps 'more complete'?

Pg 4, Line 10: what is the associated uncertainty the fuel equivalence ratio?

Pg 8, Line 4: 'size-selected', mobility selected?

Pg 8, Line 4: Which soot, i.e. what lot from which company? There is a certain amount of variability?

Pg 8, Line 4: Was any coating analysis performed on the data collected from the SP2?

Pg 9, Line 18: perhaps citing the original paper (Park et al. 2004 I believe, there may have been one earlier that used the formulation shown in this manuscript) that brought up the Df,m mobility shape factor would be more appropriate than Cross et al. 2010.

Pg 12, Section 5.1.1: As I understand, the methane diffusion flame was used in BC3, BC3+, and BC4. Can you explain why the Df described here is different than that of the Df described in Bhandari et al., 2016? I would expect nascent soot particles to have a lower mass mobility exponent.

Pg 13, Line 27: It appears that Mie also overestimates at smaller particle sizes as well.

Pg 14, Line 6: Can you comment on why these RI values are larger than those used in current global climate models?

Pg 14, Line 8: Specify that this recommendation is for 550 nm.

Pg 14-15, Line various: considerable time is spent discussing the MAC values in context with other literature, but there is little comment as to why these differences may exist.

Pg 17, Line 16: I suggest adding this note "RI fitting was performed. . ." to the caption in Figure 3.

Pg 19, Line 1: Could information regarding the soot maturity be accessed using LEO-fitting analysis with the SP2? At least that would indicate coating thickness which would indicate atmospheric processing.

Figure 2: As presented the fits are hidden behind the data, please bring them to the front to allow the reader to clearly see them. Perhaps a bottom panel showing the % difference or some such metric indicating the deviation from model vs measurement below each plot to clearly identify differences between Mie and RDG could be helpful?

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