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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling the optical properties of fresh biomass burning aerosol produced in a smoke chamber: results from the EFEU campaign" *by* K. Hungershöfer et al.

T. Bond (Referee)

yark@uiuc.edu

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Summary. This paper presents some quite interesting data and is experimentally strong. However, the analysis and presentation could be strengthened. The language is also very wordy and could be much more concise. This is a useful data set but I would like to see the presentation revised with careful attention to the claims that are made.

General comments

Authors spend quite some time (p 12659) describing how differences in combustion and other parameters can affect optical properties etc. If this is so then the conditions

of this burning (not just the fuel) should be well described. Authors then make the assumption that composition is invariant throughout combustion (p12664 line 10). They soon find that this assumption does not let them predict the optical properties during flaming periods. This should be no surprise, given the background discussion they provided! Why make such a limiting assumption? Authors claim they will assess "the influence of particle size on the observed variability in the scattering and absorption coefficents" (line 12 same page). If another major variable exists (such as chemical composition) then no such statement can be made.

Elemental carbon and/or absorption is said to be of interest here. Analytical technique is described as thermographic (C-mat Stroehlein). The Stroehlein instrument measures only total carbon. Some thermal separation must have been done. Its assumptions should be described.

Description of step 2 (p 12663) is very confusing. Scattering and absorption are measured (two parameters). In step 1, real and imaginary refractive index are derived—OK because two unknowns are derived from two measurements. Then in step 2, it is claimed that one can derive BC fraction, as well as BC and OC refractive index. If more unknowns are obtained than variables measured, something is wrong—it is mathematically impossible.

Spherical particle assumption for fresh particles could be a problem. Authors claim these are compact. One must hope there is no BC. SMPS will also respond incorrectly to fractal particles. Also authors' statements about integrated scattering of nonspherical particles (p 12669 last paragraph) are incorrect when applied to the mass scattering efficiency of aggregates. There is classic literature on this topic; see e.g. Dobbins, R.A., and C.M. Megaridis (1991), Absorption and scattering of light by polydisperse aggregates, *Applied Optics*, 30 (33), 4747-4754.

Claim of good agreement between measured and modeled (p12671 line 5, for example) is completely unwarranted. So many parameters were "tweaked" that lack of

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agreement would be more surprising! Any agreement is not a success of the study but merely demonstration that real refractive index does not vary much.

There is much discussion about the high mass scattering efficiency. Authors honestly acknowledge the possibility of experimental errors. But the (real) refractive index is similar to the one from field measurements. The size distribution is different than field measurements because there are large particles. Is this the reason for the high scattering? Something is not consistent. With the same refractive index as in the field, and size distribution that is almost the same, there ought to be similar scattering unless the large particle mode is contributing. Explanation on (p12670 line 19) is not reasonable. If condensation occurred it would enhance the mass in addition to the scattering.

There is a lot of discussion about whether the material is fully graphitized or partly graphitized (Section 4.2.2). The facts from this study seem to be: (1) thermal method gives a relatively high fraction of apparent EC (6-10%); (2) this is inconsistent with the low imaginary refractive index of the average aerosol; (3) high-MW substances were present. I think the authors have a good point: something is inconsistent. Either the thermal method responds to high-MW substances (or something else), or there is apparent EC that is not completely graphitized and thus has a lower imaginary refractive index. But this is not is not clearly stated. I think the message ought to be that we do *not* have closure between optics and chemical composition+size distribution—End of story. Authors undoubtedly recognize that their refractive indices are only derived, and they are only part of an exploration in seeking how chemical composition and optical properties are linked. However, modelers searching for optical properties don't know that. If authors are not careful, I would not be surprised to see these optical properties appear in a future modeling paper.

Statement in conclusions: "This suggests that the aerosol produced under the controlled laboratory conditions consisted of more highly scattering material than typically reported for biomass burning aerosol. . . " "Highly scattering" is technically correct but could be read as misleading. The material itself doesn't appear to have a higher refrac-

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tive index; thus, probably the size distribution is different, and this suggests that some processes in the laboratory are not representative of those in the field. Authors appear well aware of this, but need to communicate this to the readers as well.

Specific comments

page 12660 - line6 - It doesn't seem that authors were successful in deriving apparent refractive index for light absorbing fraction. This is not really their fault, because the analytical techniques are not good at detecting the light absorbing fraction. However, it should not be stated as a main focus of the paper.

page 12660 - last paragraph - Authors need not devote so much text to describing tests when these results do not appear in the paper.

page 12663 - entire last paragraph - This description is quite verbose. Clarity would be increased if word count were reduced by about 30%.

page 12665 - paragraph beginning on line 19 - This paragraph is much too long—simply say that the particles here are much larger, as indicated by Angstrom exponent, and give some ranges.

page 12666 - line 3 - give a reference for this boundary between smoldering and flaming combustion (11% CO)

page 12666 - line 19 - Parmar is not in the reference list

page 12669 – line 4 – IPCC is not a primary reference. I suggest that authors read and cite the original papers since this is their area of research.

page 12669 – line 25 – this sentence should be in the method section

page 12673—line 28 – statement "Subsequently, these pyrolysis products. . . " implies that this further thermal processing always occurs. Should be rewritten.

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