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Atmospheric Measurement Techniques Discussions

Interactive comment on "A novel single-cavity multi-wavelength photoacoustic spectrometer for atmospheric aerosol research" *by* Claudia Linke et al.

Anonymous Referee #2

Received and published: 1 August 2016

The manuscript by Linke et al. describes a custom-built, three-wavelength photoacoustic instrument. Sufficient technical details are provided, and a generous amount of measurements are presented for instrument validation and demonstration. The manuscript is well written, describes an instrument that would be useful to many atmospheric scientists, and includes some basic measurements of MAC that add to the literature. For these reasons I recommend its publication after the following comments are addressed:

Comment 1:

The manuscript sometimes refers to the three-wavelength photoacoustic as "multi-wavelength". I would say the term "three-wavelength", which is already used in

the manuscript more than once, is much more appropriate, especially in the context of other methods of measuring or estimating absorption, like the 7-wavelength aethalometer.

Comment 2:

This comment is more substantial and relates to the interpretation of the diffusion flame samples. The diffusion flame was operated under various conditions, which resulted in varying % of "OC" being measured, where OC is defined by thermal evolution of carbon in an inert atmosphere.

The manuscript currently discusses this OC % in direct comparison to the OC % of atmospheric aerosols. This is incorrect. The OC that is measured in the particles produced by fuel-rich diffusion flames has been shown to represent incompletely graphitized soot (Maricq 2014). This stands in contrast to the typical OC found in the atmosphere, which forms from "normal" organic material via terpene oxidation, hydrocarbon evaporation, etc. These "normal" organics will be very different from partially graphitized soot, in terms of their volatility, reactivity, light absorption, and hygroscopicity. Therefore, the material currently described as OC is not comparable to atmospheric OC, and the two should not be compared. At the simplest level, a similar OC% between the diffusion flame soot and atmospheric particles clearly does not warrant a comparison of the MAC between such samples.

Of course, the diffusion flame OC is still a reproduceable and well-defined material, so the reported MAC values are likely to be useful to others. But the authors should make this distinction clear, and change the discussion at "Discussion of the chamber results", on page 4 first paragraph, on page 12 second paragraph, and wherever else is relevant.

Note also that the chemical uniqueness of the partially graphitized flame soot also explains why the SP2 did not observe incandescence signals (page 9, line 30). Since the material is incompletely graphitized, what was measured as EC may have formed

by charring during OC/EC analysis, and what was originally present apparently had a MAC that was too low for the SP2 to bring it to incandescence. Alternatively, the material may not have been refractory enough to incandesce.

Comment 3:

On page 7 line 1, the LOD is discussed. What was the averaging interval? What was the influence of longer averaging times? An Allan variance analysis would be the best way to answer these questions. What I am requesting is given by e.g. Onasch et al. (2015).

List of minor comments:

I also have a few minor comments after reading the manuscript carefully.

page 2, line 35 – since BrC typically does not increase indefinitely with decreasing wavelength, please give a number to "shorter wavelengths"

page 3, line 30 here cite Collaud Coen et al. Atmos Meas Tech 2010, 3, 457-474.

page 3, line 41, why is there weak cros sensitivity to particle light scattering?

page 5, line 26, have the authors investigated whether a different transmission efficiency of such gases as NO2 might cause bias in the background measurements? Some gases may interact significantly with the PM filter.

page 6, line 4, is the instrument temperature controlled? Could variations in temperature lead to a bias in the corrected laser power?

page 6, line 29, what does field proven mean? Proven against what reference?

page 10, line 24 and elsewhere. Ideally a quantitative statistical analysis would be given, such as a two-tailed t-test, instead of the statement that values agree nicely or not.

page 12, line 11 Here the reader wonders what the difference between the different

EC/OC protocols is, especially with respect to their charring artifacts (see OC% discussion above). A short comment or citation would be helpful.

page 13, line 36, "home heating season" would be clearer.

page 13, line 37, why were the other periods described if only 31 october is included here?

page 14 line 20, were the limits of sensitivity of the SP2 with respect to particle size corrected for?

Figure 8 is hard to read and includes some labels which are not descriptive, like numbconc, scattSP2 and BBHG.

References

Maricq, M. M.: Examining the Relationship Between Black Carbon and Soot in Flames and Engine Exhaust, Aerosol Sci. Technol., 48, 620–629, doi:10.1080/02786826.2014.904961, 2014.

Onasch, Timothy B., et al. "Single scattering albedo monitor for airborne particulates." Aerosol Science and Technology 49.4 (2015): 267-279.

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