Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
https://doi.org/10.5194/amt-2022-21-RC1, 2022

Comment on amt-2022-21, Drinovec et al. 2022

Anonymous Referee #2

Referee comment on "A dual-wavelength photothermal aerosol absorption monitor: design, calibration and performance" by Luka Drinovec et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-21-RC1, 2022

The manuscript "Dual-wavelength photothermal aerosol absorption monitor" by Drinovec et al. describes a standalone and commercially viable aerosol absorption monitor based on photothermal interferometry (PTI). The manuscript outlines a thorough laboratory characterization of a new instrument, the PTAAM. The laboratory characterization includes an interesting concept for calibration. The manuscript also includes a brief characterization of various studies applying the PTAAM. The various studies fall into two categories, first the calibration of filter-based absorption monitors using PTAAM as reference and the deployment of the PTAAM at a few measurement campaigns.

Overall this manuscript is of high quality and should be published in AMT. However, I have a few comments that should be addressed first. Most of these are requests for clarification or a few sentences of discussion. The exception is the filter photometer discussion. This discussion felt rushed and incomplete, and many experimental details are introduced which were not in Methods. Moreover, it does not really belong in a paper which presents a new absorption instrument with potential applications far beyond filter photometers. For these reasons, I recommend that Section 5.4 be extracted out into a brief technical note.

Comments

- The authors mention photoacoustic spectroscopy (PAS) in their introduction and mention that PAS "experiences systematic biases when the sample contains semi-volatile organic coatings or water." However, PAS and PTI are closely related (cf. Moosmuller et al., 2009). PAS uses a modulated heating laser with a microphone for detection while PTI uses a modulated heating laser with an interferometer for detection. Any physical phenomenon which occurs in PAS should occur in PTI, unless there is some specific reason why not, such as differences in laser power density or modulation frequency. If there is a substantial difference, the authors should discuss it. If there is no difference, the authors should acknowledge it. I would expect evaporation can occur in PTAAM because the 2 W and 3 W laser powers used in the PTAAM are high.

Moosmüller, H., Chakrabarty, R. K., & Arnott, W. P. (2009). Aerosol light absorption and its measurement: A review. J. Quant. Spectrosc. Radiat. Transfer, 110(11), 844–878. https://doi.org/10.1016/j.jqsrt.2009.02.035

- Similar to the previous comment, the discussion (e.g. line 110, 625, 735) implies that the PTAAM is an improvement to filter photometers (which it certainly is) but does not acknowledge or discuss the available alternatives like PAS and the extinction-minus-scattering method (EMS, like in the commercial CAPS PMssa). Of course, a full review is outside of the manuscript's scope but the authors can briefly mention the alternatives and point to previous reviews. I do not think the authors meant to imply that PTAAM is better than PAS and EMS. If they did, then a complete and quantitative discussion is required. .

The authors proposed a clever cross-calibration method based on nigrosin. I have a few questions that the authors may address in their discussion. Both of the following questions might easily be answered by the authors demonstrating how the predicted absorption coefficient would change given uncertainty in the refractive index. In other words, please quantify how RIs are "in agreement with" one another by converting them to an absorption related quantity. After this, the following 2 detailed questions might become irrelevant:

- The authors consider that the real refractive index n = 1.81 +- 0.01 agrees with the literature value of 1.78 from Bluvshtein et al. 2017. But Foster et al. (2019, authors' citation) found n = 1.6 and concluded that "the discrepancy between the current [refractive index] and different refractive indices found in the literature at 405 nm suggest that different batches of Nigrosin have different absorptivity and that Nigrosin may not be a good calibration substance at shorter visible wavelengths." (This statement was focussed on the imaginary refractive index but seems to apply here.)

- The authors showed that a nigrosin film has a different RI to a nigrosin solution. I am convinced. But does a nigrosin aerosol have the same RI as the nigrosin film? In future work the authors may consider monodisperse size-resolved aerosol measurements (e.g. Bluvshtein et al., 2012 https://doi.org/10.1080/02786826.2012.700141) to better constrain the nigrosin refractive index. In the present work the authors may consider adding a few comments based on a sensitivity analysis or Monte Carlo.

Minor comments

- Line 585 suggests the noise in the 1064 nm channel is mainly due to its variable offset, could it also be due to the much noisier laser profile (Figure 3 and S2)? Please reword the comment if it is speculation.

- Line 515 is the +- one standard error or a 95% CI?

- Figure 8b, what GSD was modelled? And why start the x axis at zero?

- Generally please clarify that scattering is measured by a different instrument (Aurora 4000) in the captions and tables. This helps the reader since all other data is PTAAM. This includes Figure 11's axis labels.

- Section 4.3 seems to be Results, not Methods.

- The uncertainty section at the end seems to belong higher up (before the campaigns at least).

- Figure 9a, please change and enlarge the symbols, I could not read it in my printout.

- Line 584, I think you mean power-law since the plot is logarithmic.

- Figure 13, why are there 4 points on panel a and 3 on panel b?

- Line 635, what is the new type of filter? Text only states the old.

- Line 645, extrapolated or interpolated? Linearly or power-law fit?

- Line 646, reference to Section 2.3 is wrong.

- Line 655 missing the word 'factor'

- Line 662, what kind of soot? What source? A brief comment would be helpful.

- Figure 16. I appreciate the intellectual honesty of presenting noisy 1-second AAEs. But the data suggest that a calculation based on longer averaging times would be more meaningful.

- The uncertainty discussion in Section 5.6 would be clarified with more focus on the wavelengths. I would also add individual columns to Table 4 for the 2 wavelengths, since some of the uncertainties A to E depend on wavelength/laser.

- Line 550, "Based on our experience" sounds like the authors are relying on experience other than the discussion in Section 4.3. If so, please share it.

- The authors report lower AAEs than I expected for soot and only compare them to the biased AAEs reported by filter photometers. These AAEs are also low compared to PAS measurements and optical models. Please comment briefly in the text.

- In the final line of Conclusions the authors mention using PTAAM for reference absorption measurements. This conclusion was made without any Introduction about the requirements or need for new reference measurements. Please fix.