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Atmospheric Measurement Techniques Discussions

Interactive comment on "A single-beam photothermal interferometer for in-situ measurements of aerosol light absorption" *by* Bradley Visser et al.

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The authors thank the reviewers for their time in reviewing the manuscript and their constructive questions and comments. The manuscript will most certainly be improved by implementing the suggested changes. On a personal level, the authors also very much enjoyed the high level discussion of our instrument and PTI in general.

Addressing the comments and questions of Anonymous Reviewer #3 in order:

- With regards to page 6, line 11 and the criteria for selecting I_low and I_high: Yes, the reviewer is completely correct and a larger difference between I_low and I_high leads

to an increase in signal. The initial selection of I_low was determined by the accuracy at which the signal during the cooling (or low) phase could be determined. As this data was used to lock quadrature and provide some qualitative indication of the signal during the cooling phase, a minimum laser power of 20 mW was required to provide acceptable signal to noise. It should however be noted, that it is possible to lock quadrature with only the heating phase (or high) signals, thus making it possible for I_low to be zero, if no information about the cooling phase is required. However, with the AOM we have so far only been able to achieve a 20x reduction in the laser intensity in the main beam and therefore we have not been able to test this without reconfiguring the outputs of the AOM. Additional information will be added to the manuscript to explain this.

- With regards to page 8, lines 8 & 9 and the description of the noise sources: The authors agree on this point and the manuscript will be updated to make it clearer in this respect.

- With regards to page 8, lines 15-17 and the effect of particle RI contributions at high concentrations: This is a very interesting question and one that will require some further study to experimentally validate. In theory, PTI measurements are only sensitive to changes in the measured refractive index at the modulation frequency. This means that static differences in refractive index, as well as slow changes in the refractive index (even relative changes) should not affect the measured signal, regardless of the source. Even at very high concentrations, where the RIs of the various particles contribute in a meaningful way to the ensemble, the authors do not believe that any significant artefacts due to a static difference in RI will be present. At such high concentrations however, light attenuation by the sample will be large and therefore the interferometric contrast will suffer, reducing the accuracy of the measurements. We have not tested the MSPTI at sufficiently high concentrations to investigate this, but there is almost certainly an upper concentration limit beyond which the measurements are no longer valid. The implications for thermal lensing are the same as for the two-

beam PTI configurations. A thermal lens will be formed in the sample beam, which is the equivalent of the sample beam / pump beam thermal lens in the two-beam PTI configuration (with some differences due to the beam geometries). In the absence of light absorbing gases, no thermal lens will be formed in the reference beam, just as for a two-beam configuration. It is however slightly different in the case where an absorbing gas is present in the reference chamber. Under these conditions a thermal lens will be formed in the reference beam of the MSPTI, whereas none would be formed in the two-beam PTI. However, since the absorption of the gas is the same in both sample and reference chambers, the contribution to the thermal lens from the gas absorption in both chambers should also be equal.

- With regards to page 10, line 4 and the citation: The citation will be added to this line of the manuscript.

- With regards to page 11, line 16 and the comparison of Figures 5 and 8: This is unfortunately confusing, however Figures 5 and 8 don't show the same data or effect. Even the heating curves for the data points in the linear dependence region of Figure 8 are not themselves linear. The linear dependence of the PTI signal on the heating period in Figure 8 only shows that the shape of the heating curves has remained consistent within this range. The discrepancy between the heating curve and the linear fit outside of this range increases with increasing heating period, causing the nonlinearity of the PTI signal as a function of the heating period (Figure 8).

- With regards to page 11, line 22 and the clarification of text: Yes, thank you for helping to clarify this section of text. The manuscript will be updated to clarify this point. The saturation should occur at longer times by increasing the diameter of the probe beam, assuming no other cause of non-linearity becomes dominant. However, the signal strength is the intensity weighted average of the phase shift across the beam cross-section. The signal at shorter heating times will be reduced due to the time it takes for the heat from the absorption process to be conducted into the entire probe beam volume.

- With regards to page 11, line 36 and the reordering of the data presented in the figure: Yes, the authors concur that this would be preferential, both in a scientific as well as aesthetic manner. The figure will be updated in the manuscript.

- With regards to page 12, line 2 and the availability of denuders: This is correct. The authors were trying to make the point that it is possible for the MSPTI to measure aerosol particle absorption in the presence of absorbing gases without the need to modify the sample. The manuscript will be updated to clarify this point and the availability of denuders.

- With regards to page 12 and the comparison with the Aethalometer: This is correct. The mass concentrations were only intended for comparison with other measurements and to give aerosol scientists a metric to better understand the measured absorption (i.e. mass concentrations). The manuscript will be updated to focus more on absorption and use eBC as a secondary metric to clarify this point.

- With regards to page 23, Figure 11, sources of noise and the Allan deviation: As the quadrature lock circuit was operating at a frequency of 1 Hz or below, one source of the deviation from the inverse square ideal line could be partially due to the constant adjustments from this circuit. The main contribution however seems to be the low frequency drifts in the baseline. The authors feel that the standard deviation is more appropriate than the Allan deviation in expressing the uncertainty in PTI measurements. It is however acknowledged that presentation of the Allan deviation would better enable comparison to other measurement techniques. A plot of the Allan deviation using the same data as Figure 11 will be added to the supplementary information.

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