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Atmospheric Measurement Techniques Discussions

Interactive comment on "The influence of the baseline drift on the resulting extinction values of a CAPS PMex" *by* Sascha Pfeifer et al.

Anonymous Referee #2

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General comments

This paper addresses biases associated with aerosol extinction measurements from Aerodyne CAPS PMex sensors occurring in the presence of gaseous absorbers. In particular, biases become apparent when gaseous absorption varies on a timescale faster than the CAPS baseline characterisation interval. It is shown that the bias can be severe, particularly at blue spectral wavelengths where nitrogen dioxide, which is often co-located with the particulate matter of interest, absorbs strongly. The paper proposes a correction scheme that users can apply to reduce these systematic sampling biases.

It is without doubt important that users of CAPS PMex systems are aware of these biases. I would however note that these biases are in no way surprising and are already very well documented in the literature for similar research-grade instruments. Indeed,

several instruments adopting cavity ringdown-based techniques for aerosol detection have implemented specific measures to eliminate biases from gaseous absorption entirely. In my mind this does raise the question of whether this paper presents genuinely novel insight.

I find that the correction scheme proposed is at best a partial fix to the problem. It is shown to reduce error in the example data well, but there are scenarios where it is likely to provide insufficient error compensation. For example, when NO2 concentrations do not vary smoothly between baseline periods, it may be far less effective (and potentially even make things worse than the standard corrections – see below). I would have preferred the conclusion of the paper to have directed users to a more general and reliable solution, which ultimately could require physical modification of the instrument, sample conditioning (e.g. to scrub NO2/O3) or mode of operation (e.g. to run a designated monitor with a permanent filter to measure the gas phase background and variability). More focus is also needed to examine and explain the limitations of the suggested correction method. For these reasons I would recommend significant revision before the the paper is considered for publication.

Specific comments

- The suggested correction scheme works for the example data provided. However it will not work as effectively when there is significant variability in NO2 between consecutive baseline measurements. Indeed, it would appear possible that in some circumstances the applied correction could make the bias worse compared to the standard method (e.g. where the baseline increases between consecutive baseline periods but gaseous absorption decrease in the interim period). For this reason I question what applying this new correction really enables users to say with confidence about the accuracy of their resulting extinction numbers. This needs to be examined in more detail in the paper.

- To overcome the above, the authors suggest that users will need to tune their smooth-

ing parameters based on the data they have, but this approach sounds unsatisfactory to me. It has worked for this study because characterising the impact of gaseous absorbers was the focus and thus collecting long datasets while filtering was possible. In reality, people who have purchased the CAPS PMex want to be measuring aerosol extinction and thus it is undesirable to run on filter for extended periods as suggested. Indeed even if users did run with a 50% duty cycle, it still may not completely allow bias correction for reasons discussed above.

- It appears to me that there could have been scope for developing a more complex baseline correction scheme to try and overcome some of the above limitations. For example, it is shown that the red wavelength PMex units and not impacted by gaseous absorbers. Could correlations between red and green/blue wavelength units have been used to add extra constraint?

- More discussion is needed related to the realistic accuracy that applying the new corrections provides. Given the findings in the paper, how can a user quantify the accuracy of their CAPS PMex blue/green aerosol extinction measurements if they don't have a simultaneous measurement from a monitor that was run on filter?

- More discussion is needed on alternative approaches to enable reduction/elimination of biases, including those adopted by other users (e.g. scrubbing, gas reference channels). Rather than presenting a single solution, the paper would be enhanced by presenting a range of solutions that users could consider implementing to improve the quality of their data (with accompanied discussion on the merits/complications of each).

- Page 2, lines 9/10: The referenced work of Petzold et al., as far as I can ascertain, only undertook characterisation of a 630nm CAPS PMex unit. It would be worth stating this explicitly, particularly given that the biases in this work are only seen for units operating at wavelengths where the NO2 absorbs strongly.

- Page 2, line 13: here and throughout the manuscript the paper refers to the gaseous absorption leading to a baseline drift. I think this is confusing. The baseline in these

instruments is determined by such quantities as mirror cleanliness, mirror alignment, and near-constant Rayleigh scattering. Over time the baseline may drift due to temperature, ambient pressure changes etc. However, I view the NO2 absorption bias as more akin to a signal measurement. It doesn't drift, but rather is a quantity that varies dynamically and, as shown in the paper, with strong correlation to the particle signal of interest. I would prefer to have separation in terminology for these two distinct causes of error i.e. drift vs gaseous absorber signal.

- Section 3: can you explain why the ascending flank is steeper than the descending flank (line 19)?

- Section 3.2: do the data presented in Figure 3 represent an independent test of the correction scheme. i.e. are the data that were used to tune the scheme the same data that have been plotted in the histograms?

- Conclusions: I think the conclusions could be seen to provide contradictory guidance to users currently. On the one hand they suggest the new corrections could allow reduced frequency of baseline periods, but on the other suggest users should spend a lot of time filtering in order to characterise backgrounds adequately. I think the paper needs to more clearly describe that, in the absence of scrubbing of gaseous absorbers, users will never do better than having a designated CAPS measuring the gas phase background. If setups have less than this then it could come with cost in terms of residual errors from gaseous absorbers.

- Figure 4: the biases described in the bulk of this paper impact measurement accuracy rather than precision. I think the Allan Variance analysis in Figure 4 risks confusing readers with respect to understanding the absolute uncertainty of measurements. For example, it needs to be made clearer than despite the left hand panel of figure 4 suggesting a 1 sigma precision of around 0.1Mm-1, the total measurement uncertainty could be a lot bigger for these measurements.

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