

***Interactive comment on* “Technical Note: Can ozone be used to calibrate aerosol photoacoustic spectrometers?” by Donald A. Fischer and Geoffrey D. Smith**

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1. Around line 25 on page 2, the important paragraph on previous work is presented. It would be good to go into the difference in the bath gas discussion between the Davies and the Bluvshstein work here, since this plays such a strong role in the results of the current paper.

We thank the reviewer for this suggestion and have added a couple of sentences discussing the differences at the end of that paragraph.

2. The authors present amplitude measurements for photoacoustic signals from gases

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and aerosol, but no phase measurements. The phase information may help determine if time lags for physical/chemical processes are important.

While we agree with the reviewer on this point, phase measurements were unfortunately not conducted due to limitations with the DAQ system and cannot be presented here at present.

3. Some of the historical literature on the subject may be useful, e.g., Harshbarger, W. R., and Robin, M. B. 1973. The opto-acoustic effect: Revival of an old technique for molecular spectroscopy. *Acc. Chem. Res.* 6:329–334. doi:10.1021/ar50070a001 for example.

We thank the reviewer for making this suggestion and have referenced this paper in paragraphs 2 and 4 of the Introduction.

4. Between lines 15 and 20 on page 4, use of copper tubing is mentioned for delivering NO₂. We find that copper tubing can remove NO₂ until passivated, though that's probably not important for the short length of tubing and the experiment here.

We appreciate the referee bringing this to our attention. We observe the same problem (regardless of tubing composition), which is why we [1] keep the length of tubing short (~10cm) and [2] make an effort to passivate the tubing with a high concentration of NO₂ prior to analysis. The manuscript has been updated here to reflect these things under "NO₂ Measurements".

5. Around line 5 on page 5, ". . . particles were size selected at 500, 550, 600, and 650 nm using an electrostatic classifier .. ". Just to be sure, it would be good to specify if this is diameter or radius. Particle loss issues and thermal relaxation rate of aerosol might be important to study, and that distinction would be important.

Thank you for pointing out our imprecise language here. We have updated the manuscript to reflect that these values represent mobility diameters.

6. In Fig. 4b an 'error bar' would be useful for the NO₂ measurement.

We thank the reviewer for the suggestion. We have added a 3σ error bar to the NO₂ sensitivity in Figure 4B.

7. Any speculations or discussion about why the NO₂ calibration is higher than the nigrosine calibration?

In figure 2, the NO₂ sensitivity is 10% higher than the nigrosine sensitivity. We speculate that at least some of that difference could be attributed to uncertainty/error on CPC particle concentrations and in the refractive indices of nigrosin used in calculating the nigrosin absorbance. We have added a couple sentences to the results under “Non-ozone Methods of Calibration” indicating this.

8. The need to check calibration with various combinations of gases highlights the need for accurately measuring the resonance frequency and quality factor for quantitative measurements.

Yes, we agree! This is why we measure the resonant frequency each time the gas composition is changed or every 30 minutes, whichever comes first.

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