

Interactive comment on “Broadband cavity enhanced spectroscopy in the ultraviolet spectral region for measurements of nitrogen dioxide and formaldehyde” by R. A. Washenfelder et al.

Anonymous Referee #2

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In this paper, Washenfelder and co-workers describe a broadband cavity enhanced spectroscopy instrument for use at the lower end of the short ultraviolet. The sensitivity and performance of the instrument is evaluated with respect to NO₂ and formaldehyde, CH₂O, and shown to be suitable for laboratory and field measurements of these important atmospheric species. The work is a valuable instrumental development in several respects: (1) it extends the broadband technique to shorter near-UV wavelengths, which is a technically difficult region as the authors correctly point out. (2) It is the first specific application (as opposed to the general demonstration of Islam et al., 2013) that uses a laser-driven light source for BBCES. (3) It is the first application of the

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technique to the atmospherically important species CH₂O and thereby brings a new, and highly selective tool to the study of this species. It should be noted that the spectral range that Washenfelder demonstrate in their instrument has broader applicability than just to CH₂O, as other organic species (most notably other carbonyls) and some carbonaceous atmospheric particles start to absorb appreciably at shorter wavelengths. Indeed, this and similar instruments may prove most valuable when applied to particle optical properties.

Specific comments: 1. p.9941: The association/equating of the 1σ standard deviation for a series of measurements with the precision and detection limit is incorrect. The detection limit is usually defined in terms of 2 or 3 times the standard deviation (as on p.9942). This should be clarified.

2. The authors implicitly ignore any spectral dependence to the changing output of the LDLS. Have the authors studied the time dependence of the spectrum to determine that this is really the case? Although the polynomial in the DOASIS fit may remove the effect of small changes for retrieving the concentrations of gases like NO₂ and CH₂O with structured absorption spectra, possible spectral changes in the lamp output would compromise spectral measurements of particles or gases with unstructured absorption bands.

3. An Allan variance study seems to me to be the one obvious omission from the paper. The authors have selected an acquisition time (30 s or 1 min) that seems to provide a good quality spectrum in a reasonable time; however, the assumption based on previous experience that the S/N will improve for up to 10 minutes (p.9941) may not be justified for this novel light source. For readers considering adopting such a light source, explicit quantitative information on stability of the combined LDLS-optical cavity spectrometer system would be valuable.

4. The spectral acquisition procedure includes frequent measurement of dark spectra, to the point of appreciably shortening the duty cycle of the instrument and thereby,

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possibly, reducing the performance of the instrument. Why is this frequency of dark current measurement warranted?

5. Unless I have misread the argument, the reference to δI in Eq. (3) and Sect. 4.5 should really read $(\delta I/I)$. That is, an absorption/extinction measurement aims to measure a fractional change in intensity.

Minor comments:

p.9940, Sect. 4.2: It would be helpful to state the combined uncertainty of the calibration and cross-section.

p.9943: For readers comparing the LDLS to other light sources, it would be helpful to state the electrical and approximate optical power.

Fig. 2(b): For clarity of interpretation, the right axis labels should have the same colour as the axis title and associated curve

Fig. 6: Even if the slope is not meaningful information, the y-intercept would be a useful secondary figure of merit and should be provided in the caption.

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