

## ***Interactive comment on “Development and characterization of the CU ground MAX-DOAS instrument: lowering RMS noise and first measurements of BrO, IO, and CHOCHO near Pensacola, FL” by S. Coburn et al.***

**Anonymous Referee #3**

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The study by Coburn et al. reports on the “Development and characterization of the CU ground MAX-DOAS instrument: lowering RMS noise and first measurements of BrO, IO, and CHOCHO near Pensacola, FL”. The paper comprises three major sections: 1) a very detailed description of the instrument, 2) the characterisation of the instrument in the laboratory, and 3) the application in the field. In general, the topic of the paper fits into the scope of AMT. However, I doubt some of the authors statements and conclusions given in their study. Therefore major corrections are needed before publication.

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Since I agree with several comments from reviewer #2 and in particular reviewer #1, I'm focussing mainly on those items not addressed in detail so far. One thing I have to repeat: The authors claim several times that they have developed an instrument which is only photon shot noise limited. However, they are not able to proof that under realistic conditions. Most (if not all) of their suggestions to improve the RMS have been addressed by other groups working in this field in a similar way: e.g. improving the temperature stabilisation and using the system in a specified saturation level. Table 1 shows quite clear that these groups end up with a very similar detection limit, which means that in practice other effects might limit the RMS level. But one should mention, and that is the positive thing about this study, to my knowledge the level of investigation for these parameters is really outstanding here and might help novel workers in this field to better understand the limitations of the different instruments. The authors should focus on these (really good) results without arguing that they have solved general experimental problems about MAX-DOAS leading to an instrument which is about 10 times better than others which is obviously not the case.

In section 3 a discussion on the change in FWHM with wavelength is completely missing. When looking to Figure 2 it is quite obvious that the spectral resolution of the instrument is decreasing with wavelength with about 0.74 nm in the Visible and 0.99 nm FWHM in the UV with the latter one being on the upper end of what is needed for a proper DOAS retrieval in that wavelength region. Furthermore it is also visible that the slit function is becoming more and more (non-linear?) asymmetric towards the UV. How this is addressed in the DOAS retrieval and what is the impact on the results? Is the measured slit function used for convolution of the cross sections or a fitted Gauss function?

For section 4 I would like to see some statistics (plot) between derived dSCD, RMS and elevation angle and/or solar zenith angle for all measurements both in the UV and the visible. This should give an impression on the real performance of the instrument. Similar to reviewer #1: What is this scaling factor about? I can imagine what the authors

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mean, but how it is derived? The whole chemistry discussion in this section is useless when not supported by any serious radiative transfer calculation and statistics on the measured columns.

Some more general comments: The authors should avoid strong language (e.g. “first measurements of” in the title and several times in the body text, which is completely waste, since than almost every field measurement is somehow the first one; “one of the most light-efficient instruments” in section 2, which is just speculation, since much more parameters have to be considered to evaluate the light throughput; “approach was first presented . . . in the first field deployment”(p260, l28-29) this approach is used by other groups since several years).

Like reviewer #1: Please state only numbers in the text which can be derived from the results!! E.g. in the conclusions: “The instrument is able to measure [sic!]  $RMS < 1.0e-5$  . . . under laboratory conditions using solar stray-light.”

A lot of acronyms are redundant (CU GMAX-DOAS, MDN, OLF, EPA, FL, IAM, FT) and make the text as a whole difficult to read. Other acronyms are not introduced like proportional–integral–derivative controller (PID).

The level of details given in the different sections is sometimes much too high, and sometimes not sufficient.

Examples (too detailed)

Section 2.1.: “is driven by an Intelligent Motion Systems Inc. MDrive34 Plus motor (48V, 4A maximum)”

Sections 2.2 and 3: No need to mention several times, that custom Labview code is used.

Section 4.2, p.264, l5-11: 54862 individual spectra recorded . . .

(not enough information)

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Section 4.1: What is the method behind the calculation of the Ring spectrum? There is no information given in Krauss, 2006.

References:

Some references are missing (in particular Roscoe et al., AMT, 2010, or Vlemmix et al., AMT, 2010)

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Interactive comment on Atmos. Meas. Tech. Discuss., 4, 247, 2011.

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