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Atmospheric Measurement Techniques Discussions

Interactive comment on "Linearisation of the effects of spectral shift and stretch in DOAS analysis" *by* S. Beirle et al.

Anonymous Referee #1

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The paper describes an important further development of the DOAS technique, which may have significant impact on the data processing for future satellite missions. The paper is well written, with eye for detail. As a consequence I have, apart from a few minor details, only two points.

1. At several places in the paper it is mentioned that using the B-shift (on I_0) instead of the A-shift (on I) has the advantage that the derivative has only to be calculated once, with consequences for processing time. A bigger disadvantage of using the A-shift seems to me that it needs knowledge of the high-resolution radiance spectrum to calculate the derivative (according to eq. B3). While a solar high-resolution spectrum is readily available, generating a high-resolution Radiance spectrum invokes much more

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effort. This is because the radiance measurement itself (on lower resolution) cannot be used. Instead the SCD of the strong absorbers need to be known/estimated from the measured spectrum, and high-resolution cross-sections must be applied to a high-resolution I_0 to obtain the radiances. Please consider this, e.g. in Section 3.3 and/or 5.3. Is the same issue not affecting the zenith-sky reference spectra for ground-based DOAS?

2. The paper concentrates on NO2, which is a medium strong absorber. The paper mentions that the method is less advantageous for strong absorbers. From the errors shown in the paper I have some doubt that the method would be applicable for weak absorbers such as BrO or Formaldehyde. In this case, NO2 may be the only tracegas for coming TROPOMI/Sentinel missions where the new method would give a processing speed advantage, which makes the new method much less interesting to be implemented in Level 2 processing. Have any prelimanary studies for weaker trace gases been performed? What are the expectations of the authors? Any hint to this in the paper would be valuable.

Minor points:

page 8373 line 19 instrument function -> instrument spectral response function

p.8373 line 20 "[instrument spectral response function] can be determined during the calibration". Probably "calibration" in this sentence is meant as done in the framework of the WinDOAS software. But this does not determine the instrument spectral response function (ISRF). At most some parameters which characterise the ISRF (e.g.FWHM) may be fitted. But its shape (or its parametrisation) must still be known beforehand. As it is written, this part of the sentence is both untrue and unnecessary. Please remove.

p.8375 line 1 Why do cross-sections and pseudo-absorbers (like Ring) have to be Doppler-shifted when I_0 is shifted ? This doesn't make sense to me, as they are attached to the unshifted radiance spectrum. Please explain (is I_0 not actively shifted?)

AMTD

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or reword.

p.8384 line 14 "For the linear fit, the results [..] are identical" This must be wrong. In Table 3 the results differ by a factor of 10. Also on line 22 it is said that for the linear fit MATLAB performs better than DOAS. Please correct.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 8369, 2012.