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Interactive comment on "An improved glyoxal retrieval from OMI measurements" *by* L. M. A. Alvarado et al.

Anonymous Referee #3

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The paper by Alvarado et al. "An improved glyoxal retrieval from OMI measurements" introduces an improved glyoxal retrieval procedure. The settings are based on sensitivity studies shown in the paper and the retrieval is compared with retrievals from GOME-2 and SCIAMACHY. Finally glyoxal is shown as an indicator of pyrogenic emissions. The paper is well structured and meets the focus of Atmospheric Measurement Although the authors perform valuable sensitivity tests the conclusions appear to be rather arbitrary. I only recommend this manuscript for publication in AMT if the following major points are addressed and revised accordingly.

Major points:

section 2.3: The authors used the outdated HITRAN database published in 2005 for

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the water vapour cross-section. Use the newest version and redo the retrievals for both synthetic and real data. The retrieval of a weak absorber like glyoxal can be strongly affected by small water vapour absorption features. Also a comparison with the HITEMP database (Rothman et al., 2010) that includes more bands and transitions would be valuable.

section 2.4: The conclusion of the optimal fitting wavelength range does not seem to be logical: Of course, it is reasonable to include the strongest absorption features into the fitting range and also to consider the retrieval with a synthetic spectrum (Fig. 1) to some extent. However, it appears that the authors try to make a point based on the retrieval plots of the real data in Fig. 2. In contrast, their conclusion is based on the consistency with the retrieval pattern of the synthetic spectrum, which looks quite different compared to the one of real data. The point in terms of the homogeneity is not reproducible since start limits between 423 and 428 nm (with the same end limits) appear to be more homogeneous than the claimed start limit range of 432-436nm. Also, if only the real data was to be considered the chosen range would probably have a start limit of 430-434nm and an end limit of 450-456nm. This would give significant data over Africa, Brazil, Europe and NA, and about zero over desert and ocean (Of course, this is not recommended by the referee since the strongest absorption would be excluded here). Moreover, why does the start limit range of 432-436nm leads to a starting wavelength of 433nm (and not 434nm)? 434nm would be in the middle of this range and appears to result in more consistent values for the two-step liquid water retrieval compared to just including the liquid water absorption cross-section.

section 2.5: Again it looks like that the conclusion is drawn from the retrieval of the synthetic spectrum only. What is obvious for the synthetic spectrum cannot be observed by the referee for real data. For real data, it does not appear that any polynomial degree is significantly superior for the chosen wavelength range. If this impression is wrong it must be shown differently. Lower values for polynomial order of 2, as stated, are only visible between 432 and (maximum) 433nm start limit.

section 2.6: Indeed the argument that including the liquid water cross-section directly into the fit would lead to the smallest number of negative values is visible in Fig. 4. In contrast, the fit for the two-step approach looks more homogeneous in the considered wavelength range (start limit 432-436nm, end limit 456-460nm), which makes the two-step approach superior. The authors should elaborate on the criteria which they chose for the determination of the retrieval settings.

section 2.7: It cannot be assessed if it is an improvement or even a difference when including a warm NO_2 cross-section in the two case studies of Fig. 6. Only if errors are added this would be possible. The findings are not clear since for Congo there is almost no difference in the CHOCHO SC while there appears one for Bejing, although both locations have emissions close to the ground in about the same order (at different season though).

section 3: A discussion of the reasons for the different values is missing. Can it be explained by different overpass times?

Minor points:

section 2.1: Add the overpass time of OMI.

section 2.4: Add which order of polynomial (3 (?) from comparison of Fig. 1 and 3) and offset were used. Add the value of the a-priori SC and the concentration at ground for the assumed concentration profile. The authors state that more than 100 spectra were included for the glyoxal retrievals of each region. How does this transfer to the detection limit of each region in Fig. 2. Add this information in order to be able to assess how significant the SC changes are.

section 2.8: I agree with referee #1 that the CHOCHO should be shown here.

Rothman, LS; Gordon, IE; Barber, RJ; Dothe, H; Gamache, RR; Goldman, A; et al.; HITEMP, the high-temperature molecular spectroscopic database. J QUANT SPECTROSC RA , 111 (15) 2139 - 2150. 10.1016/j.jqsrt.2010.05.001, 2010.

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