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Atmospheric Measurement Techniques Discussions

Interactive comment on "Glyoxal retrieval from the Ozone Monitoring Instrument" *by* C. C. Miller et al.

Anonymous Referee #1

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The authors present in this paper an optimized glyoxal retrieval algorithm for OMI. In particular, they carried out a series of sensitivity tests to define the most appropriate settings for the retrieval of glyoxal slant columns. These tests, based on both synthetic and real spectra, confirmed the optimal choice of the glyoxal fitting window in previous studies and the need of including in the fit a second NO2 cross-section at a different temperature. The liquid water fitting window has also been optimized in order to limit correlations with the spectral response of sandy surface. The algorithm also includes an original normalization procedure using Sahara as the reference area and allowing an efficient destriping. Overall, the OMI glyoxal fields are roughly consistent with those retrieved from SCIAMACHY and GOME-2 presented in other studies, although slightly smaller. Finally, they present for a single season ratios of glyoxal to HCHO VCD which appear to be in better agreement with surface observations than the values presented in other studies.

This paper fits well within AMT. It is well structured and the results are clearly presented. I recommend this work to be published in AMT after that the following comments have been addressed.

Major comments

- A paper has been recently submitted by Alvarado et al. in AMTD. Please replace the reference to the poster presentation by a reference to that manuscript.
- Section 2.1: Could you explain what is the added-value to include in the fit the common mode spectrum (except for decreasing the fit residuals)? As this spectrum results from an average of a set of spectrum residuals, it is by definition orthogonal to the glyoxal cross-section and shouldn't impact the retrieved glyoxal SCD. Also, if this spectrum is generated using also oceanic scenes, it might lead to a degradation of the residuals rather than an improvement (because of liquid water spectral structures not entirely corrected for).
- Section 2.2: Please add a reference for the O2-O2 cloud algorithm. Could you explain how cloudy pixels are treated? Is a cloud correction applied or a simple cloud filtering? In this case, what is the cloud fraction threshold used to reject cloudy pixels?
- Section 2.2: Can GEOS-Chem provide glyoxal profiles over oceans? It would be useful to present typical profiles for different regions and to discuss their reliability.
- Section 2.3: This section focuses on the errors associated to the slant column retrieval, but the authors neglect the errors due to the AMF computation. This should be discussed. Also, please provide an estimate of the total error associated to the retrieval.
- Section 3.1: Please explain already here that the Ring effect is not included in the simulations. The added value of these experiments for the choice of the fitting

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window is not obvious at all since the model to generate the synthetic spectra is very similar to the one used for the retrieval. To support the discussion of section 3.4, it would be interesting to generate synthetic spectra using a RT model including inelastic scattering and to perform retrieval with the simpler fit model. If this is feasible, I think the paper would gain a lot.

- Section 3.2: There is also a consolidated OMI sun spectrum available (Dobber et al., Solar Phys., 2008, DOI 10.1007/s11207-008-9187-7). Using this one might help to limit issues related to the daily measured solar spectra (undersampling, noise,. . .). Did you try this consolidated spectrum in your glyoxal retrievals? That being said, it is likely that it would not remove entirely the need for a destriping correction.

- Section 3.4: The decrease in the retrieved SCD below 435 nm is not very clear in Figure 11. It gives the feeling that the lower window limit could be anywhere between 429 and 435 nm, which questions the discussion on possible interferences with the Ring signature. Could you clarify or adjust the figure to make more visible the decrease you mention? Do you have any idea on the cause of the strong discontinuity at 429 nm?

- Section 4 - line 22: I don't think that the larger GOME-2 and SCIAMACHY glyoxal columns in summer can be explained by interferences with NO2 since its concentrations in the boundary layer peak in winter in NE China rather than in summer.

- Section 4: Your statement about the impact of water vapour cross-section, and the pressure and temperature conditions for which it has been derived is interesting. Again, the paper would benefit greatly if this was supported with a sensitivity test. You should at least mention your choice of temperature and pressure to derive your own RCS.

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- Conclusions: I'd recommend to specify also here the order of magnitude of the ratio glyoxal to formaldehyde for different types of emissions.

Editorial comments

- Equation 7: I think the denominator is wrong and should be $I_{sol}(\lambda) \times \exp(-x_j^{ref} b_j^{hr}(\lambda)) \otimes \Gamma(\lambda)$. My understanding is that all cross-sections are corrected for the I0 effect. Is it necessary?
- Page 6073, line 10: remove "RT"
- Figure 4: Could you add a map showing the true glyoxal SCDs? (it is a little too small in Figure 3).
- Page 6084 - line 4: I suppose H2O means here water vapour. Please mention it.
- Page 6085 - line 1: please mention explicitly what is the biomass burning season.
- Page 6085 - line 17: remove "in"

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 6065, 2014.