Atmos. Meas. Tech. Discuss., 7, C1429–C1436, 2014
www.atmos-meas-tech-discuss.net/7/C1429/2014/

Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #1

Received and published: 27 June 2014

The authors present in this manuscript a new tropospheric glyoxal product from OMI observations. They focus on the optimization of the DOAS settings, which helps to make the product more robust. In particular, they have carried out different sensitivity tests to select the most appropriate fitting window and polynomial. They have also compared different techniques to limit as much as possible spectral interferences caused by liquid water absorption. They also show that including a second NO2 absorption cross-section at higher temperature is necessary in polluted regions to avoid spectral interferences with the tropospheric NO2 signature. The last part of the manuscript aims at illustrating the product with some inter-sensor comparisons and with two case studies making the link between strong pyrogenic emissions and enhanced glyoxal concentrations.

The topic of this work fits well within the scope of AMT. Although the paper is well structured, the text needs to be carefully revised in order to be more precise. I made a few suggestions in the editorial comments below, but this is certainly not sufficient. Overall, the discussion of the results is not detailed enough. Additionally to the description of the results, the authors should also try to interpret them. One striking example of this lack is in the inter-sensor comparison. The authors report that the SCIAMACHY glyoxal columns are larger than those from other sensors but do not discuss at all any possible cause for this. In addition to this general remark, I have a series of major comments that need to be taken into account to make this manuscript suitable to AMT. In its current shape, the paper is not mature enough.

Major comments

- Some important aspects are currently missing in the description of the algorithm. What is the reference spectrum for the DOAS analyses? The OMI daily measured irradiance spectrum, the consolidated OMI sun spectrum or a radiance spectrum measured in a remote area? Does a wavelength calibration procedure need to be applied to the OMI spectra? Is an intensity offset fitted? Also OMI suffers from the so-called row anomaly after 2007. How do you deal with this?
- Section 2.3 For the water vapour cross-section, a reference to Rothman et al., 2005 is given. The HITRAN database, including the water vapour line shape parameters, has been updated several times since the version of 2005. Interferences with the water vapour may have a significant impact on the retrieved glyoxal slant columns. Is there a good reason to keep using HITRAN 2005 for the water vapour or is it only the reference that needs to be updated?
- Section 2.4:
  - It can be inferred from the different figures that these tests are based on a polynomial of degree 3 without any specific correction for the liquid water

interferences. This should be explicitly mentioned in the text.

- In figure 1, the choice of the reference value to compute the relative differences is strange. Usually, the “truth” is the reference. Please compute the differences as (SC_fitted - SC_true)/SC_true. Please mention also this true value.
- The deviations observed from the closed-loop tests are rather small. This is expected as the same cross-sections are probably used for both creating the synthetic spectra and retrieving the slant column. So errors due to spectral interferences and use of imperfect cross-sections can’t be estimated using this technique. On the other hand, the authors should discuss the origin of the deviations, even if they are small, and their dependence on the fitting interval. What are the differences between the forward model used to create the synthetic spectrum and the DOAS model. How is the Ring effect simulated?
- The fact to have deviations in the closed-loop tests so small compared to those observed with real data raises some questions about their added-value to the discussion. Is it possible to make a link between these two aspects?
- Why different dates had to be used for the different regions?

- Section 2.5:
  - See comments from previous section.
  - In figure 2, the deviation patterns significantly differ from a region to another. Is the impact of the polynomial degree the same for all regions? It might be useful to show at least one other region, which could possibly replace the tests based on the synthetic spectra.
- Section 2.6:

  - If I understand correctly, a liquid water correction is only applied over oceanic regions. I think there is a risk of discontinuity between the retrieved glyoxal fields over oceans and lands. In theory, the liquid water signal should obviously be negligible over lands. In practice, spectral interferences with other species might lead to a bias in the retrieved liquid water SC. If there is such a bias, It would be better to have the liquid water cross-section also included for the treatment of pixels over lands to ensure a spatial homogeneity of the product. Did you check that the retrieved liquid water SC is small over lands (and consequently has no impact on the glyoxal)?
  - Some of the authors are also co-authors of the paper recently published in AMTD by Peters et al.. Did you try to apply the method proposed there to better account for all liquid water effects in glyoxal satellite retrievals?
- Section 2.7:
  - In figure 5: In order to better see the impact of including a second NO2 cross-section on the retrieved glyoxal columns, I would suggest to replace the second series of CHOCHO maps (third row) by maps of the CHOCHO absolute differences (2 NO2 cross-sections - 1 NO2 cross-section).
- Section 2.8:
  - The authors refer to the Wittrock et al. paper for the typical glyoxal profiles used in the AMF computation. I unsuccessfully tried to find the information in this paper and in the references cited therein. For the sake of transparency, could you add a figure illustrating these typical profiles?
- Section 2.9
  - This section is quite general and could be made shorter by adding a few references. The authors suggest that the systematic errors are constant over

time. This is probably erroneous as a significant fraction of these systematic errors is caused by spectral interferences with other species, of which the concentration may depend on the season. As mentioned by the authors, the normalization procedure helps to reduce the impact on the glyoxal product of these interferences. Nevertheless, the spatial variability of these systematic errors remains. This should be clarified in the text and an estimate of the systematic errors should be given. Finally, is there a basis for the value of 2x10^{14} molec/cm^2 used for the glyoxal background or is it somewhat arbitrary?

- Section 3:
  - For these comparisons, the SCIAMACHY and GOME-2 data appear having been reprocessed using a second NO2 cross-section. Why did the authors not apply any correction for liquid water interferences for these two sensors? This inconsistency may lead to significant inter-sensor biases because of the normalization procedure. Indeed, the oceanic glyoxal fields will be strongly different for all three sensors, which means that the reference values removed from all measurements of the day (including lands) as part of the normalization will differ significantly, but do not reflect necessarily instrumental specificities. I wonder if the important SCIAMACHY bias could come from this.
  - A destriping correction is mentioned without any details. Please explain a bit more. What does it correct for? How does it work?
  - To me, the three data sets do not compare so well. Differences could be expected between OMI on one hand and SCIAMACHY/GOME-2 on the other because of possible diurnal variation but large SCIAMACHY-GOME-2 differences such as those displayed in Fig. 8 are quite unexpected. This should be discussed a bit more. Is it because of the absence of correction for liq-

uid water absorption in SCIAMACHY (see above)? On the other hand, the seasonal cycles of SCIA and GOME-2 roughly agree.

- Section 5
  - How should negative correlation coefficients visible in figure 10 be interpreted?
  - In figure 11, it would be interesting to show a similar CHOCHO map for another year in order to highlight the exceptional nature of the event.
- Conclusions
  - “Significant differences were found over regions with large anthropogenic emissions. Moreover, a similar seasonal behaviour is observed among the three products.” This is inconsistent with the text of section 3: “good agreement is found in the temporal behaviour among data sets over regions dominated by biogenic emissions and also with large anthropogenic activities, such as China (South and East)”. In addition, the OMI seasonal cycle appears to differ from that of the two other products in several of the regions presented in figure 8 (it is generally less pronounced).

Editorial comments

- Abstract - line 11 : “reduction of negative columns“ is not a very clear formulation. Please rephrase.
- Page 5561 line 21: “Similarly to the biogenic and anthropogenic emissions” instead of “Similar to biogenic emissions”. Anthropogenic emissions are certainly not better quantified.
- Page 5561 line 24: delete “various”, “VOCs” instead of “VOC”

- Page 5562 - line 11: “spaceborne” instead of “space-born”
- Page 5562 - line 15: “have been used to derive CHOCHO distribution at the global scale by applying. . .” instead of “to retrieve global maps of CHOCHO distribution by applying. . .”
- Page 5563 - line 11: “a description of the method used to limit interferences with liquid. . .” instead of “a description of the interferences with liquid. . .”
- Page 5563 - line 14: Remove “glyoxal products” after SCIAMACHY and GOME-2.
- Page 5564 - line 10: “are removed by the fit of a low-order polynomial.” instead of “are removed by a polynomial in wavelength”.
- Page 5564 - line 12: “by including corresponding absorption cross-sections in the DOAS fit” instead of “by taking into account the absorption cross-sections of all relevant trace gases.”
- Page 5564 - line 12: What is a “good DOAS fit”? Do you mean “to derive accurately SCs”?
- Page 5564 - line 19: rephrase as “460 nm, which includes different interference species, and use polynomial of order 2,3 or 4 for the removal of broad-band signatures (Wittrock et al., 2006; Vrekoussis et al., 2009).”
- Page 5564 - line 26: “a new retrieval algorithm for the OMI instrument. . .” instead of “a new retrieval for data of the OMI. . .”
- Page 5564 - line 27: “details” instead of “detailed”
- Page 5570 - line 6: Suggested sentence: “This result shows that the temperature dependence of the NO2 absorption cross-section needs to be taken into account

in case of large tropospheric NO2 columns to limit as much as possible (seasonal dependent) systematic errors on the glyoxal column caused by spectral interference.”

- Page 5570 - line 10: please rephrase. It is not only useful to compute vertical columns, but it is mandatory since the SCs are not directly applicable in any atmospheric application.
- Page 5571 - line 26 “of” instead of “ot”
- Figures 8 and 9: Specify explicitly from which sensor (OMI) are derived these maps.
- Section 4 and figure 9: Please avoid as much as possible to use the terms “summer”, “winter”, “autumn” and “spring” in the discussion since those terms are only valid locally.
- Page 5574 - line 1: remove “part of the”
- Page 5574 - line 7: replace “anthropogenic” by “highly populated”
- Page 5575 - line 8: “shown” instead of “show”
- Page 5575 - line 19: remove “days”
- Page 5576 - line 21: “reduction of negative glyoxal values” is not very clear. Please reformulate.
- Page 5576 - line 25: remove “at high NO2”

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 5559, 2014.