Atmos. Chem. Phys. Discuss., 9, C2129–C2134, 2009
www.atmos-chem-phys-discuss.net/9/C2129/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Temporal and spatial variability of glyoxal as observed from space" *by* M. Vrekoussis et al.

M. Vrekoussis

vrekoussis@iup.physik.uni-bremen.de

Received and published: 23 June 2009

We would like to thank both reviewers for their comments and suggestion which helped to improve our manuscript. Below we present the responses to comments and describe the modifications made to the manuscript.

Reviewer 1:

Comment 1: As mentioned above, it is hoped that satellite observations of glyoxal will be able to shed further light on VOC photooxidation processes on a global scale. One of the most intriguing aspects of the data set is the prevalence of glyoxal over the tropical ocean, particularly given its short lifetime with respect to photooxidation (3 h). The authors discuss this lifetime in section 1.3 and describe it as an average

derived from global modeling studies. It would be helpful if the authors could comment on the difference in the lifetime of glyoxal that might be expected in the boundary layer versus the upper troposphere. In the ITCZ, one might expect glyoxal of marine origin to be rapidly convected to the UT (as discussed in section 3.5.7). If glyoxal has a longer lifetime at higher altitude might that help explain its distribution over the tropical oceans?

Reply: As proposed by the reviewers, the relationship of the lifetime of glyoxal vs. the altitude has been investigated via the TM4-ECPL chemical transport model (Myriokefalitakis and Kanakidou, personal communication, see fig.1 last page of comments (C6)). As depicted in this figure, the lifetime of CHOCHO is significantly lower in the boundary layer in comparison to higher altitudes. Several parameters control the total lifetime of glyoxal, e.g. photolysis, OH radical reaction, wet and dry deposition. At the lowest level of the troposphere CHO.CHO's lifetime is small mainly due to the dry deposition. While the lifetime of glyoxal is longer in the free troposphere as the reviewer suggested, it still is rather short and long-range transport of glyoxal itself cannot explain the enhanced values observed over the tropical Pacific. This signal must either originate from local sources or from secondary production from precursors with a much longer lifetime.

Comment 2: Page 9011, lines 7-9. You state that there is a 6 month shift in the seasonality of CHO.CHO between box 4 and 6. This is not obvious to me. The minima appear to be offset by only 3 months April (6) v. July (4), while the maxima are both in December with local maxima in August (6) and September (4). Although the fire counts are offset, so are the EVI in a way that appears to me to mitigate the seasonality of CHO.CHO. You go on to discuss this further, but this sentence doesn't match the figure and I would recommend dropping it.

Reply: We agree that the seasonal variability of CHOCHO is more complex than the oversimplified one reported in our manuscript. Over N. Africa the minimum glyoxal values are found in summer and the max in winter (about 6 months difference). However the seasonality over S. Africa (at the selected box) is less profound. Two maxima are

found in summer and winter and two minima in spring and autumn. These differences may lead to misinterpretation of the results. For this reason the manuscript has been modified as suggested:

“In West Africa fires peak from December to February and in Southern Africa during May-August (Fig. 7, red columns). This results in different patterns in the observed seasonality as depicted in figure 7, in regional boxes 4 and 6”

Comment 3: Page 9011, the paragraph beginning at line 10 is written in such a way that it implies the differences between boxes 4, 5, and 6 are due to a north-south gradient. Is this correct? All three boxes are near the equator, might the differences simply be attributable to differences in vegetative cover and fire statistics?

Reply: This point is a misunderstanding. The “gradient” reported at the text was a characterization attributed to the observed values. It was not written in a way to imply a north-south gradient. As pointed out at the text “Each of the three regions is controlled by different types and mixtures of emissions.” The reported different emissions (biogenic and biomass burning) are responsible for the reported differences. However in order to avoid any future misunderstandings when reading the paper the word “gradient” has been omitted.

Comment 4: Section 3.5.6. The fire season ends in December, but the maximum VCD persists through February. This is puzzling given the short lifetime of glyoxal and the relatively minor variation in EVI. Can the authors comment further on the two month lag between the end of the fire season and the decrease in VCD? Reply: Indeed as correctly pointed out, the VCDCHOCHO increase during the fire season but peak two months after it. This behavior coincides with the observed changes in the EVI variability. The amplitude of the EVI variability is equal to 0.1, a number greater than the one reported for e.g. Indonesia and smaller than the 0.2 reported for e.g. North America. When plotting the VCDCHOCHO=f(EVI) graph (not shown here) emanating from all the monthly mean data of both CHOCHO and EVI, a linear correlation is found

equal to VCDCHOCHO=1.25E14xEVI. For EVI = 0.3 the expected VCD(CHOCHO) is 3.75E14molecules.cm-2 a value which is within the given uncertainty of the observed (5.2$\pm$1.5)E14molecules.cm-2. It should be noted that CHO.CHO variability is highly correlated to the one reported for HCHO (Fig 14, De Smedt et al., 2008, Atmos. Chem. Phys., 8, 4947–4963, 2008).

Technical Comments: Pg. 8997, line 8, delete "the" from "are the ethylene" - Corrected

Pg. 8998, line 24, "yields" should be "yield"- Corrected

Pg. 9000, line 8, "wavelenght" should be "wavelength"- Corrected

Pg. 9002, line 23, "in the order" should be "on the order"- Corrected

Pg. 9007, line 19, "devision" should be "division"- Corrected

Pg. 9013, line 1, I would recommend not describing the agricultural burning contribution in N. Asia as "large", given the other values for the fire counts in Table 2 and the fact that VCD appears to follow EVI quite closely.

Reply: We agree that the word large is excessive for the N. Asia region so it has been removed.

Pg. 9015, line 13, "reach" should be "rich"- Corrected

Table 1. 1) Capitalize second "rural" 2) Fix the spacing for #9 such that "Summer/Autumn" does not obscure the following number. - Corrected

Table 2: 1) To be consistent with the text and figures, I would recommend changing "USA" to "N. America". - Corrected

2) RGF = 0.045 for USA, but the text (pg. 9009, line 8) says 0.044, 3) RGF = 0.043 for box 2 and 0.048 for box 3, but the text (pg. 9010, lines 8-10) says 0.045 and 0.049, respectively, ... I’ll stop here, box 4 RGF is also inconsistent between the table and text, please double check all values to ensure they match.

Reply: In fact this point is a misunderstanding. We used two different ways to present our data. In the text we used the mean values coupled with their standard deviations. Table 2 presents the median values. So the data presented to table 2 are the correct median values calculated for each region of interest.

We also added a sentence in section 3.5.1 saying: “Please note that in order to provide additional statistics, table 2 presents the median RGF values”.

Figure 2b caption, "tranfer" should be "transfer"- Corrected

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8993, 2009.

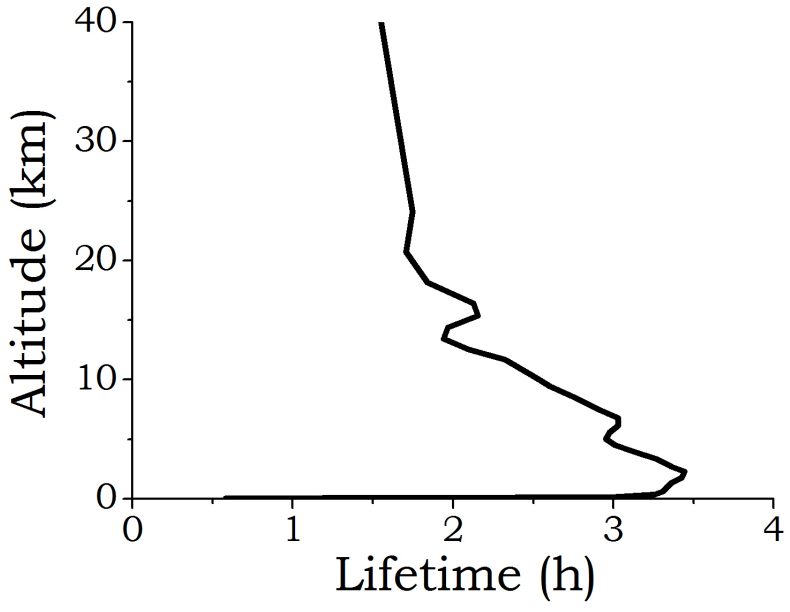

Fig. 1.