

We like to thank the referee for taking the time to review our manuscript and provide the helpful comments. Our responses are detailed below (in blue).

C1.1 My main comment is on the effect of water. Mahajan et al. (JGR 119, 6160-6169, 2014) state "Further studies are required to rule out the influence of water cross sensitivities on glyoxal retrievals." The manuscript also points out that water vapor is one of the main features in the glyoxal region. This raises the question whether water fluxes can have an impact on the measured glyoxal fluxes, and of course concentrations. From Mahajan and other work it appears that uncertainties in the water cross section may result in biases in glyoxal. If a bias in a trace gas concentration measurement is caused by water (concentration), I would think changes in the flux of water could result in an artificial contribution to the flux signal in the trace gas. I am sure the authors considered this. The authors should explicitly address the statement by Mahajan and the question of whether this can affect the glyoxal fluxes and how big this effect may be. Furthermore, not just the RH but the water flux should be added to figure 7 to visually show whether there is a correlation, although that obviously does not equal causation. This is the main comment I have, which I recommend be explicitly addressed.

R1.1 The influence of water cross-sensitivities on glyoxal retrievals was studied by Thalman et al. (2014) (ATMD), which is referenced in this work. It explicitly addresses this concern with respect to cavity enhanced DOAS measurements in a series of intercomparison campaigns using a large variety of *in-situ* measurement techniques that do not suffer from such interference. They find that the effect of water is largely decoupled from the glyoxal retrieval for a very similar cavity enhanced DOAS instrument.

To explicitly address this concern we have analyzed our data to retrieve water fluxes based on our absorption measurements (included in a new figure with corresponding discussions in Sects. 3.3.3 and 4.2.2). The water flux is positive, consistent between the day and night, and between both Southern and Northern Hemisphere. In particular, the variability found in the glyoxal fluxes does not mirror those in water fluxes; there is no obvious cross correlation between the two measurements. A correlation plots between the two flux measurements shows no distinct trends, and has been added to the manuscript as supplementary material.

C1.2 How can it be ruled out that the upward flux of glyoxal observed at night results from gas-phase chemistry right above the ocean surface but below the measurement height. This may be unlikely, but if it cannot be ruled out it should be briefly acknowledged.

R1.2 A semi-quantitative argument has been added to Section 4.2 about why we think gas-phase chemistry below the inlet is unlikely to affect our glyoxal fluxes. In essence, the nighttime increase in glyoxal is attributed to a dark reaction involving ozone. However, we observe hemispheric gradients in O₃ that are reversed to those observed for glyoxal. If a gas-phase reaction was forming glyoxal below our inlet, this production should be higher at higher O₃ concentration, and lead to greater fluxes in the NH. However, the opposite is observed. Additionally, NO₂ fluxes have been calculated from our absorption measurements and included in a new section (Section 4.2.2). NO₂ is produced from the gas-phase reaction of O₃ with NO, that is photochemically produced, and emitted from the ocean. The rate constant for NO reactions with O₃ is at least 5 times faster, and possibly much faster than that of O₃ with glyoxal precursor VOCs. If chemistry was occurring below our inlet, we would expect to see positive

NO₂ fluxes. However, NO₂ fluxes do not show any evidence for a diurnal cycle, and if anything are slightly negative, consistent with expectations (see discussion in Sect. 4.2.2). As such, we do not see any evidence that would suggest chemistry below our inlet affects the fluxes of NO₂, and even less of an effect is expected for glyoxal.

C1.3 P. 6263-6267 is a rather drawn out discussion of comparison with satellite measurements, sources of glyoxal etc. I recommend shortening this section and just stating the main points very succinctly and referring to future work. As I am sure the latter is already under way I don't see the downside of this and it will eliminate paragraphs that span two pages. I am not saying that the findings are not interesting, actually it is the opposite, but as written, readers may miss these or not read them due to the structure of this section.

R1.3 We acknowledge this relevant point. The revised manuscript now contains a separate sub-heading to avoid this discussion be buried. We feel that all the information contained within this paragraph is timely, and highly relevant; any shortening would compromise the impact of the message. Section 4 has been restructured into several shorter sections with appropriate headings to inform and prepare the reader of the information contained within each new section.

Minor Comments

C1.4 P. 6246 Line 5: Should this be “effective Henry’s Law constant”

R1.4 Done

C1.5 P. 6246 Line 17: 2Hz are mentioned here and in figure 6 it is mentioned that the Nyquist frequency is 1Hz. It might be helpful to readers to clarify this at some point in the paper.

R1.5 The reference to Nyquist frequency was added to this line

C1.6 P. 6246 From figure 3 it looks like 2 ppt is the theoretical 1 sigma precision at high photon numbers (2E9), which based on the 8 minute data is roughly 1 hr. Is 1 sigma precision equivalent to limit of detection? Also, the experimental data do seem to trend away from this and be slightly higher. Mainly I think a better explanation of the 2 ppt in 1 hr would be helpful.

R1.6 The text in the abstract has been changed to reflect the ~6 pptv value for the ~8 min spectra cited in Section 3.2.

C1.7 P. 6246 Line 22 I would recommend replacing “and” with “which” in line 22.

R1.7 This line has been modified to accommodate this change.

C1.8 P. 6246 Line 26-27 “Ours are the first EC flux measurements of glyoxal” seems unnecessary as it is already mentioned in the first sentence?

R1.8 This sentence was removed.

C1.9 P. 6251 Line 12: is there a model number or output power that could be quoted as well as FWHM in nm of the emitted radiation?

R1.9 Text has been added to state the model number and peak optical power of the LED.

C1.10 Why is the grating blazed at 250 nm if the used wavelength are either 440-470nm (mirrors) or 390-530nm (spectrometer)?

R1.10 There is no particular reason. Despite the difference in blaze angle, the attainable noise is close to a white noise sensor.

C1.11 Figure 2: I assume the numbers in the figure are the slant column density?

R1.11 That is correct, text has been added to the figure caption to explicitly state this.

C1.12 P. 6254 line 9 “effect of ln(?) order”

R1.12 This sentence has been corrected.

C1.13 Figure 4:GG420, BP, f/4 should be explained. Although explained in the text MFC could also be explained here and the sonic pointed out and the distance between the N2 pulse valve and the overall inlet length (see next point)

R1.13 Appropriate text has been added to the figure caption to address these points.

C1.14 Section 3.1.1 This may be naïve regarding the time lag introduced between the logging of the wind and trace gas data. This is probably mainly due to the travel time in the tubing with perhaps some contribution from data acquisition. In the experimental figure it looks like the N2 is not injected directly at the inlet. Does the length of tubing between inlet and N2 valve have to be accounted for as this represents additional travel time or was this amount insignificant, which seems likely given the overall tubing length and flow speed.

R1.14 The reviewer assumes correctly in that the phase correction comes mainly as a result of time the air spends in the inlet rather than from data logging. As for the second aspect of the comment, this is actually not an issue because the N₂ is injected directly at the inlet and this confusion comes from the diagram itself. The figure has been appropriately modified to make this more explicit.

C1.15 P. 6260 line 18. “reasonably” is a little vague for a technical manuscript.

R1.15 The text in this sentence has been modified.

C1.16 P. 6251 line 21-P.6252 line 4 and figure 2. I highly recommend also showing the right hand panels in figure 2 for the clean period and the left hand panels for the period with NO₂ contamination. If I understand correctly the SCDs are shown but this is not indicated.

R1.16 The purpose of Figure 2 is twofold: 1) to show spectral fits in both of the fitting windows utilized, one optimized for glyoxal (433-460 nm) and one optimized for O₄ (457-487 nm); and 2) to demonstrate both clean and contaminated spectra. The first point is the primary focus of the figure and for this reason the glyoxal fitting window was chosen for the left hand panels. Labels have been added to Figure 2 to note these different fitting windows.

C1.17 P. 6261 line 25-P. 6262 line 4. The fact that the instrument has excellent performance could be stated more succinctly and be focused on the topic of this manuscript. It is irrelevant for this manuscript that the instrument performed better than others. It is highly relevant and very well described that the instrument performed extremely well for the task at hand. I recommend eliminating or shortening the statements to make them relevant to this manuscript.

R1.17 We agree, and certainly do not mean to make such a comparison. We have modified the text to emphasize the benefit of what we have learned from the comparison with the many other techniques about the robustness of CE-DOAS.