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Atmospheric Measurement Techniques Discussions

# *Interactive comment on* "Measurements of diurnal variations and Eddy Covariance (EC) fluxes of glyoxal in the tropical marine boundary layer: description of the Fast LED-CE-DOAS instrument" *by* S. Coburn et al.

**Anonymous Referee #1**

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The manuscript presents measurements of eddy covariance fluxes of glyoxal over the tropical ocean, a great experimental achievement. The work is quite thorough with substantial detail appropriate for the topic. The technical section is extremely well written and an impressive piece of work. This type of measurement capability represents an important advancement and is suitable for AMT and of interest to readers. I recommend publication after the points below have been addressed:

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.

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.

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.

Minor comments:

4. P. 6246 Line 5: Should this be "effective Henry's Law constant"

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.

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.

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

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

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?

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

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

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

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)

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.



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

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.

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.

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Interactive comment on Atmos. Meas. Tech. Discuss., 7, 6245, 2014.