

Interactive comment on “Cross-validation of inferred daytime airborne CO₂ urban-regional scale surface fluxes with eddy-covariance observations and emissions inventories in Greater London” by A. Font et al.

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Dear Ron:

I am writing in response to your request for a follow-up review of the paper submitted

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to ACP, *Cross-validation of inferred daytime airborne CO₂ urban-regional scale surface fluxes with eddy-covariance observations and emissions inventory in Greater London*. As noted in my first “quick report,” the described data set is a worthy addition to the relatively sparse measurements of urban CO₂ reported in the literature. I would further like to congratulate the authors on the revisions they have made to the text since the previous draft. The paper is much more focused now, and many of the qualitative and quantitative arguments are clearer. The majority of comments made in my quick report have been addressed.

Below, I include a list of only the outstanding questions and comment that remain from my preliminary report. A few additional (minor) points are also included. Assuming these comments are addressed, I see no reason why this manuscript should not be published in ACP.

1. The paper now includes a brief statement of why the data on October 12 and October 19 were not included in the IMBL calculations. However, I am still unclear on why the advection term on October 19 could not be quantified. I would like to see a sentence or two in the text that spells this out explicitly.
2. More information is still needed regarding the described error propagation. For example, does the listed standard deviation reflect both the accuracy and precision of the measurements? Also, how are the errors in the various measurements calculated? For example, the mixing heights in section 3.2 are quoted to great precision, even though they are estimated from visual inspection. Is an error in the mixing height included in the error propagation, and if so, how is it estimated? This information on the error propagation is especially crucial, given the reference to it and its significance in the last part of the discussion section.
3. A number of the quantitative results reported have changed since the last draft. For example, in section 3.2, the current draft reports the urban-regional CO₂ surface flux on 13 October 2011 as $50.7 \pm 18.8 \mu\text{mol CO}_2 \text{ m}^{-2} \text{ s}^{-1}$, whereas the

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previous draft quoted an uncertainty of $9.2 \mu\text{mol CO}_2 \text{ m}^{-2} \text{ s}^{-1}$. The corresponding uncertainty on 17 October 2011 has also increased. Uncertainties for the results on 24 October 2011 and 25 October 2011 have also changed (e.g., compare Figure 6 in the previous draft to the current draft). Changes to the text are not necessarily required to adequately address this comment. However, as it is not clear to me from the present manuscript the reasons for these changes, I would like a concise summary of all the quantitative results that have changed, and a corresponding brief explanation.

4. The aspect ratio for all subpanels in Figure 2 is misleading. Could the authors please edit this figure such that the aspect ratio is the same as that used in Figure 1?

Sincerely,

Holly Maness

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 13465, 2013.