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## ***Interactive comment on “Estimating greenhouse gas fluxes from an agriculture-dominated landscape using multiple planetary boundary layer methods” by X. Zhang et al.***

### **Anonymous Referee #3**

Received and published: 3 June 2014

1. Does the paper address relevant scientific questions within the scope of ACP? Yes.
2. Does the paper present novel concepts, ideas, tools, or data? Regional flux estimate has been a persistent knowledge gap. The novel idea of this research is to conduct a comprehensive uncertainty analysis in multiple methods (inverse modeling, PBL equilibrium approach, tower-flux upscaling...) for estimating regional fluxes of multiple greenhouse gases (CO<sub>2</sub>, CH<sub>4</sub>, N<sub>2</sub>O). Such an analysis is valuable and useful for studies with intends to upscale the local tower-flux measurements to a regional scale.

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3. Are substantial conclusions reached? Yes.

4. Are the scientific methods and assumptions valid and clearly outlined? I agree with review #1's comments. I think that the basics of each method are clearly outlined. However, some descriptions and reasoning on assumptions need more efforts because the whole point of this research is to study uncertainties in different methods for regional flux estimations. Particularly, very large uncertainty is associated with the equilibrium approach. To improve the manuscript, I have a few suggestions for authors to consider. (1) One more uncertainty sources associated with EQ approach might be in using the concentrations measured at Niwot Ridge (NWR, 40°3'11"N) as the proxy data of free-tropospheric CO<sub>2</sub> data (c<sub>+</sub>) at the KCMP tower site (44°41'19"N). Although both sites are in the Ferrel cell with prevailing west winds aloft, CO<sub>2</sub> concentration has a prominent increase towards to high latitudes in northern hemisphere (Denning et al., 1996). I suggest adding discussion on the above uncertainty source for EQ approach in the discussion section. Denning, A.S., Fung, I.Y., Randall, D.A., 1995. Latitudinal gradient of atmospheric CO<sub>2</sub> due to seasonal exchange with land biota. *Nature* 376, 240–243.

(2) I also suggest one more option that the Marine Boundary Layer CO<sub>2</sub> ([http://www.esrl.noaa.gov/gmd/ccgg/globalview/co2/co2\\_description.html](http://www.esrl.noaa.gov/gmd/ccgg/globalview/co2/co2_description.html)) measured at the same latitude as the KCMP tower site is located can be used as background-free-tropospheric CO<sub>2</sub>, i.e. c<sub>+</sub> in equation (2). The real values of free-tropospheric CO<sub>2</sub> (c<sub>+</sub>) can be provided by aircraft measurements as shown in Figure 2 in Yi et al. (2004).

Yi, C., K. J. Davis, P. S. Bakwin, A.S. Denning, N. Zhang, A. Desai, J. C. Lin, and C. Gerbig, The observed covariance between ecosystem carbon exchange and atmospheric boundary layer dynamics at a site in northern Wisconsin, *Journal of Geophysical Research*, 109, D08302, doi:10.1029/2003JD004164, 2004.

GLOBALVIEW-CO<sub>2</sub>: Cooperative Atmospheric Data Integration Project – Carbon Dioxide. CD-ROM, NOAA ESRL, Boulder, Colorado, also available on Inter-

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net via anonymous FTP to ftp.cmdl.noaa.gov, last access: August 2011, Path: ccg/co2/GLOBALVIEW, 2006. If authors can find aircraft CO2 data available to use, it would be incredibly helpful.

(3) Authors have estimated  $\Delta C_{\text{air}}$  by several approaches, comparisons are valuable. I suggest authors using a figure or a table to summarize the comparison across the approaches.

5. Are the results sufficient to support the interpretations and conclusions? Yes.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? See suggestions in 4th question.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Generally yes, but a few references below are necessary: Denning, A.S., Fung, I.Y., Randall, D.A., 1995. Latitudinal gradient of atmospheric CO2 due to seasonal exchange with land biota. *Nature* 376, 240–243.

Yi, C., K. J. Davis, P. S. Bakwin, A.S. Denning, N. Zhang, A. Desai, J. C. Lin, and C. Gerbig, The observed covariance between ecosystem carbon exchange and atmospheric boundary layer dynamics at a site in northern Wisconsin, *Journal of Geophysical Research*, 109, D08302, doi:10.1029/2003JD004164, 2004.

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8. Does the title clearly reflect the contents of the paper? It would be better if the title is changed into something like “Uncertainty analysis in multiple planetary boundary layer methods for estimating regional fluxes of greenhouse gases”.

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9. Does the abstract provide a concise and complete summary? Yes.
10. Is the overall presentation well structured and clear? Yes.
11. Is the language fluent and precise? Yes
12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes.
13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See my suggestions in question 4.
14. Are the number and quality of references appropriate? See my suggestion in question 7.
15. Is the amount and quality of supplementary material appropriate? I agree with reviewer #1's suggestions.

Miscellaneous Page 3249, line 3, remove “other”.

“our tall tower” has been used many times throughout the paper. It would be better if “our tall tower” is replaced by “the KCMP tower”.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/14/C3179/2014/acpd-14-C3179-2014-supplement.pdf>

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 3231, 2014.

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