

ACP

Estimating greenhouse gas fluxes from an agriculture-dominated landscape using multiple planetary boundary layer methods

X. Zhang, X. Lee, T. J. Griffis, J. M. Baker, and W. Xiao

Principal Criteria	Excellent (1)	Good (2)	Fair (3)	Poor (4)
Scientific Significance: Does the manuscript represent a substantial contribution to scientific progress within the scope of Atmospheric Chemistry and Physics (substantial new concepts, ideas, methods, or data)?		Good		
Scientific Quality: Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?		good		
Presentation Quality: Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)?		good		

Does the paper address relevant scientific questions within the scope of ACP?

1. *Does the paper present novel concepts, ideas, tools, or data?*

The paper compares evaluates different methods to estimate regional fluxes, none of them novel, although applying the EQ method to CH₄ and N₂O is a first ever. The comparison does have considerable value however.

2. *Are substantial conclusions reached?*

Yes, the relative uncertainties and merits of the various methods are well discussed. However, since the top down methods are presented here as an independent means to verify bottom up reported missions, I believe it is a missed opportunity that the authors did not include the emissions reported in the national GHG inventory system that must be available for the region. I would love to see also the bottom-up NIR estimates according the IPCC guidelines, because its verification is the main motivation for the work presented here.

3. *Are the scientific methods and assumptions valid and clearly outlined?*

If any, this is the only weak point in the paper. The methods are not always exhaustively described. I would prefer to learn more about the details perhaps in the form of supplementary material. See 5 below.

4. *Are the results sufficient to support the interpretations and conclusions?*

Yes. Although after the methods/results sections many questions remain, these are largely answered by the very good section 4 discussion.

5. *Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?*

No. A few examples:

Tall tower EC: it is unclear how exactly the storage term is computed (F_s in eq 1). which observation heights? interpolated?. Why the (arbitrary?) limit of $-4\text{umol.m}^{-2}\text{s}^{-1}$ for discarding F_s in the morning transition? was the EC system ever above the night time SBL? how exactly monthly mean composite diurnal variation (equation)? effect of this compared to gap filling (in discussion section)? in method section it is claimed no gap filling is done, in section 3.1 suddenly there is gap filling (but only for one specific month?)

EQ method: how is time averaging done of c_+ and c_m ? what does 'composite diurnal variations' here mean (equation)?

CT give more detail on the version/product used

The FA method: the regional flux is a simple area weighted average of the fluxes of the respective land cover types, or is it footprint weighted. Either way give equation. Either way give a table in section 2.1 with the fractional cover of all significant land cover classes. Now, it is unclear how much of the area is covered by grass/pasture and or forest, only that together they make up 40%. Fires are implicit in CT; any estimate from FA?

How exactly is the footprint map used? Why only for EQ and not for FA? is this footprint the same as for EC? (the footprint is first mentioned only in section 3.1; move this to methods section; give footprint map (e.g. in SM)

in 3.3 somehow an annual $\text{CH}_4/\text{N}_2\text{O}$ flux is computed? it remains completely unclear how that is done. details! equations!

I suggest to add a supplementary section describing such details.

6. *Do the authors give proper credit to related work and clearly indicate their own new/original contribution?*

yes

7. *Does the title clearly reflect the contents of the paper?*

No. The title suggests the paper is only about PBL methods, implicitly assuming their bottom-up method (Flux Aggregation) or the direct Eddy covariance at larger elevations can and should be taken as references. The title should better reflect the fact that there is no a-priori best method to determine regional fluxes and that this paper is about a more general methods comparison including both bottom-up and top-down methods.

8. *Does the abstract provide a concise and complete summary?*

Yes

9. *Is the overall presentation well structured and clear?*

Generally yes, but see my point 4 above: quite some elements now very well discussed in section 4 come a bit late in the whole narrative. Now section 4 is really the core of the paper. Considerable parts could be integrated in the results section, which would make that part stronger

10. *Is the language fluent and precise?*

Yes

11. *Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?*

yes but... see above

12. *Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?*

I suggest to add a supplementary section describing some of the methods in more detail see 5 above.

In table 1 do not only present r^2 and NSE but also the (annual) flux magnitude of each method and the RMSE. The high values for the first two only show that the seasonal cycle is followed by each method.

13. *Are the number and quality of references appropriate?*

Generally yes

14. *Is the amount and quality of supplementary material appropriate?*

No see 5 and 12 above

Miscellaneous

p3239, l 6 water vapor mixing ratio on the tall tower...which level?

p3241, l12-17 This is too simple a discussion of the fetch of the EC observations. More sophisticated methods to determine the footprint exist e.g. Kljun et al 2002/2004, Vesala et al various reviews. Bring this in line with the better footprint estimate made a few paragraphs later for EQ

p3244, l11-12 '...no prevailing wind direction...' strange statement, the more so because on p3248 l10-15 an explicit advection estimate is made considering a northwest prevailing wind

section 4.2 I am not an expert on this EQ method but eq 2 to me suggests that the flux at the PBL top considered here is a vertical advection term only, subsidence, $W \neq 0$. It neglects turbulent entrainment due to shallow cumulus, convection, $W=0$, $w' \neq 0$. However, you even confuse it more by combining eq2 with 3 and 4 respectively. In the first case, eq2+3, implicitly you account for all transport terms that cross the PBL top. Combining eq2 with 4 you do not!

This justifies more discussion at least!

p3246, l22-23. 'If the EQ method ...' Bullshit statement. Delete whole sentence

p3247, l20-end nowhere in the paper is the effect of PBL dynamics discussed wrt the 200m observations; are these always in the mixed layer? can they be above the SBL at night? etc

p3248,l22-23 Direct measurements of these terms.... How ? Suggestions?

fig 2. the plotted error bars appear to span the range of the three annual estimates (the lines pass through their end points) this cannot be the standard deviation. More the stdev is not a very meaningful parameter for $n=3$. Better show this figure as a bar graph plotting the bars for each of the 3 years

fig 3 since you use FEC as reference throughout the paper I suggest you make that line thick

Fig4 why show the lines for NIWOT ridge no variability for CO₂ and CH₄ and so little for N₂O