

Review

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Title: Inferences from CO₂ and CH₄ concentration profiles at the Zotino Tall Tower Observatory (ZOTTO) on local summer-time ecosystem fluxes

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General evaluation

This study represents vertical profiles of CO₂ and CH₄ concentration measurements from the ZOTTO tall tower site in Central Siberia. An attempt is made to estimate ecosystem fluxes on intermediate scales (10⁴ km²) applying the modified Bowen Ratio method, which assumes equal eddy diffusivities for sensible heat, carbon dioxide and methane referring to similarity theory. Additionally it is explored how much information on local carbon fluxes can be extracted from the CO₂ and CH₄ concentration profile at ZOTTO.

Considering the remoteness of the site and the obviously excellent data quality, the paper, lacks in originality. In fact, at the end it presents averaged diurnal fluxes during summer-months, but simultaneously relativized the quality of the results. (MBR method, "our approach has certain limitations", "advection can alter the signal especially during sunrise or sunset", "measurement uncertainty is another restriction for our turbulent flux estimates", "in summary, the MBR method can give only limited information about the turbulent fluxes", and many more). At the end the reader wonders what the in the title mentioned "inferences" are.

MBR requires a) simultaneous measurements of T and the scalar of interest at identical levels and b) that measured H is representative for the same source area. Further on, no horizontal transports are considered. Regarding the large footprint of the top level, sources/sinks of heat and CO₂/CH₄ are not very likely to be the same. Most MBR studies show differences between EC fluxes and MBR derived fluxes which are attributed to different source areas and/or advection. This fact should be addressed more in detail, also regarding the large footprint.

I agree that the recently installed eddy covariance (EC) system can help to extend the flux estimates back in time. However, since EC data in 2012 was obviously influenced by the great Siberian forest fires, the presented comparison relies on a poor data base, again regarding the different footprints. The statement that "the derived CO₂ fluxes exhibit reasonable diurnal shape and magnitude" is rather vague. A comparison with data from 1996 may be possible, but the time lag of 15 (!) years has to be addressed.

There is no information about stratification. Nothing is said about stability nor the height of the boundary layer. Since the most reliable fluxes refer to nighttime, it would be of highest interest to have an estimate of the height of the nocturnal boundary layer, i.e. if the top level is above the NBL. This is essential when comparing with the local fluxes from EC towers.

There is an ongoing discussion in the FLUXNET community about the correct form of the mass conservation equation and (storage) flux calculation (see Finnigan (2006), comment by Kowalski (2008) and response by Finnigan (2008) in Agr. For. Meteorol., also Kowalski JAS 2012), regarding the physical conservation principles. This issue should be addressed at least

in the introduction in the context of tall tower measurements and the sampling times of the six level tubing system.

As the paper heavily relies on storage flux calculations between different levels I wonder why the authors do not present these values. In my opinion this could significantly improve the originality of the paper.

I agree that it is difficult or even impossible to “judge on the contribution of horizontal advection to the measurement signal”. Referring to Finnigan (1999) the authors do not present estimates of vertical advection. However, since there are sonic anemometers installed at all six levels, it would be of highest interest to see estimates of total vertical advection at the top of the tower and between these levels since vertical advection may account for a significant portion of total advection. Further on, as the authors apply the modified Bowen ratio method, it could be of interest to see the propagation of the sensible heat flux and the exchange coefficients with height with regard to the height of a “constant flux layer”.

The paper would generally profit from presenting derived data for the different height levels.

Footprint: this is an essential topic when trying to attribute signals from tall towers to local ecosystems. As this is a central part of the paper I would expect much more details about the differences of concentration and flux footprints and how the tall tower signal is interpreted in terms of local ecosystem fluxes. The authors state that “it is explored how much information on local carbon fluxes can be extracted from the CO₂ and CH₄ concentration profile at ZOTTO”. I cannot find the section in the text that really addresses this topic. I further miss the relation of footprint and land use/land cover. I think there is much more to say than “the fluxes of the surrounding boreal forests ... have the largest impact on the measurement signal”, and, for the EC towers, “the two towers represent well their surrounding local ecosystems”. Where is the link?

Boundary layer budget methods (e.g. Denmead, 1996; Eugster & Siegrist, 2000) were obviously applied earlier at the site (Lloyd et al., 2001), though with large uncertainties. Nevertheless, this method would be an easy to apply additional control for comparison with the presented fluxes.

Considering this general evaluation I recommend rejecting the paper.

minor remarks

P..40L21: please provide an approximate mean height of the taiga forest in the surrounding of the towers

P..40L28 ff.: mixing time of 37 min., frequency 0.0004 Hz (P..44L19). According to Winderlich et al. (2010) one cycle for the 6 levels is 18 min. How are these measurement used in the 30 min. framework (all calculations are done with a temporal resolution of 30 min.)?

P..41L21: “...we make use of ... and vertical wind measurements”! Where?

P..43L3: time step 30 min.

P..43L19: photosynthesis reduces the CO₂ concentration at all heights?

P..44L11: please add chapter reference in the reference section

P..45L12: please provide references for “models”

P..45L55: It would be of interest how many data points were omitted.

P..47L8: please add chapter reference in the reference section

P..49L8 ff.: I am sceptic about these comparisons considering the completely different footprints. “The general shape of the datasets compare well” is not an argument.

P..45L15: abstract Huang et al. : “...eddy diffusivities differ among the three scalars, by up to 10–12%, in the surface layer”.

P..55L11: please correctly cite chapters 5 and 8 of this book