

## General Comments

This is an interesting paper that takes a few steps in the right direction of where surface-atmosphere exchange work can go with the rapidly increasing availability of UAVs that can carry the relevant sensors. The paper presents data from field work that compared CO<sub>2</sub> flux data from a stationary eddy covariance flux tower in a southern Chinese coniferous forest with fluxes estimated from vertical profile and box flights above this tower with a UAV carrying CO<sub>2</sub> and turbulence instrumentation. 167 flights over a year-and-a-half were analyzed, split into seasons and time of day. Boxes of different horizontal sizes were flown in an attempt to evaluate heterogeneity and advective effects. The derived vertical fluxes appear to agree well, but the analysis of horizontal advection seems a bit more inconclusive.

Unless this is made clearer in the text, many readers may assume that part of the motivation was that the UAV approach can do what EC can't, and that is to quantify horizontal advection. The problem with that is that you are quantifying the advection between  $z=35\text{m}$  to  $z=100\text{m}$ , not the advection most flux tower operators are worried about, i.e. below the EC measurement level. I think the paper would be strengthened by a careful explanation / conceptualization early on that what is being compared when doing the tower – UAV comparison are the turbulent fluxes across the horizontal plane at  $z = 31\text{m}$  (which is what the tower gives you) and the mass budget of the box sitting on top of this plane, from  $z = 35\text{m}$  to  $100\text{m}$ , typically  $100\text{m}\times 100\text{m}$  horizontally), which contains no CO<sub>2</sub> sources or sinks, i.e. if all the fluxes across the 6 surfaces of the box are accounted for, the numbers should be the same in the limit of insignificant flux divergence/advection. And it seems to work, more or less! By explaining this, you can alleviate any worries a reader may have about the complexities below the  $z=31\text{m}$  level.

In its current form, the manuscript has a few weaknesses that need to be fixed. The theory section is somewhat sloppy, with many undefined terms, missing references/explanations, etc. – see specific comments for details. The interpretation / discussion of the discrepancies in 4.2 and 4.3 is weak and needs to be revised. One big problem is that the uncertainties are vastly underestimated (as they were in Gordon et al., 2015). There are errors due to non-stationarity and the non-instantaneous nature of the UAV flights that should be quantified and included. Perhaps running an LES simulation might shed some light on differences between actual fluxes and trying to estimate them with a single point measurement moving through space to map a box over  $\sim 20$  minutes or so.

This kind of field work would ideally be done with several fully automated UAVs flying simultaneously, and as continuously as possible – food for thought! Also, a flatter site, or one with several flux towers that can provide an independent estimate of horizontal advection, may be worth considering for future studies.

Given the lengthy list of improvements suggested below, I will categorize this as “major revisions”.

## Specific Comments

All equations throughout: make sure ALL terms are defined explicitly

Line 38: simplify to  $r^2 = 0.77$ . With such a tight range, no need for the “approximately” or the range.

Line 144: the UAV method complements the EC fluxes, but not in the way most might expect. The current approach, which looks only at the box above 35m, says nothing about advection below the eddy covariance level and therefore cannot help making the EC estimate (which relies on assuming horizontal homogeneity below the measurement height) more accurate. But, with a significant amount of UAV data, it can provide an independent measure of the forest flux.

Table 1: define the “D” in the GPS accuracy

Table 2: I presume you simply mean “ppb” for the CO<sub>2</sub> accuracy

Fig. 1: what is the LED for?

Section 3.1: very nice. This is very important & useful to future studies trying to do this.

Line 328: a horizontal  $u'$  of 0.02 m/s seems very low – it should be at least on the order of  $w'$ .

Line 329: by more than a factor of 100

Line 345, 362 & elsewhere: the term “emission” is misplaced here. What you are really talking about is advection. Emission should be reserved for processes related to the actual creation of CO<sub>2</sub>.

Eq 3.8: this is the exchange coefficient for momentum. In what follows you assume that  $K_e = K_m$ , which is only approximately true, some of the time. This needs to be stated explicitly.

Eq. 3.10: where does this stratification correction parameter come from? Please provide references or a derivation. This is not part of the Monin-Obukhov “canon” and needs to be explained.

Line 399: wind profiles “close to the forest”

Line 402: the displacement height depends on the type of forest, so state “for this type of forest”

Line 425: Eq. (3.17)

Line 426: specify what  $z_1$  and  $z_2$  you used in your calculations

Line 427: make it explicit that this section is about the (stationary) tower EC system

Line 445: remove “eddy covariance”

Lines 450-452 are misplaced and should be moved to 2.1.2.

Fig. 4: nice! However, a meaningful x-axis would improve this (time units), and a 1:1 plot along with slope,  $r^2$  etc. would also be useful.

Section 4.2. It would be good to start this section with a time series plot of all tower fluxes (May 2023 – Dec 2024) with the times of flights marked by arrows, to give the reader a feeling for the seasonal magnitudes of the fluxes.

Line 481: missing a verb... “are compared against...”

Line 483: again; “is used, which...”

Fig. 5: what does SMA stand for?

Table 3: state how you define the seasons. For ecological studies, growing seasons are often used, but I presume you are using 3-month calendar seasons.

Table 4: that the evening is so much worse than the morning is a bit surprising. A figure (perhaps in the SI) showing the typical diurnal cycle of  $u^*$  and/or stability ( $R_i$ ) (based on the tower system) might be helpful to explain the asymmetry. Fig. 8 suggests morning and evening should behave similarly.

Also, spelling of “morning”

Line 582: Rephrase this sentence. Why would low nighttime turbulence lead to vertical fluxes? I presume you’re trying to talk about the relative magnitudes of the various flux terms.

Line 597: how is nighttime defined? Same as “evening” before?

Line 604: the F term needs to be explicitly explained – not everyone knows ANOVA intimately. The sentence in 605 can be shortened or eliminated.  $F=2.50$  actually means that “among” is exactly 2.50 times larger than “within”, not approximately.

Fig. 7 is really not needed, or could be moved to the SI.

In fact, this whole section on evening/nighttime fluxes could be shortened to a single paragraph. Obviously the relative errors will become larger at night, but since the fluxes are much smaller than during the day, they should be assigned a lower importance – if what you are after is the net ecosystem exchange.

Fig. 8: ensure color consistency with Fig. 6.

Line 643: this is a coniferous forest, so I presume the seasonal variation in LAI is small. Therefore, LAI is not the explanation. The reason really is simple growing season dynamics related primarily to solar radiative input and temperature.

Section 4.3. When doing these comparisons of different box sizes, you need to be very careful to compare “similar situations”. Ideally, the 3 boxes would be captured simultaneously (and instantaneously), but that of course is not possible. So it is very important to bin together very similar conditions (time of day, winds, turbulence, EC flux magnitudes etc.), otherwise you are comparing apples with oranges. Please add an explanation of what exact datasets are compared.

Line 662-666: remove; superfluous

Line 672: not “based on”. Simply say “the means (see boxes in Fig. 9) were ...”

Line 674: summer at noon

Fig. 9: move central annotation to make it legible

Table 5: normalize the emission rates by area for easier comparability

Line 706: units

Line 711: In which upper layer? And are you saying that the footprint is homogeneous in the summer but heterogeneous in the fall? That seems unlikely. It may simply be a matter of the relative magnitudes of  $E_{CV}$  and  $E_{CH}$ , and the mornings (regardless of season) may be more strongly affected by advective issues such as katabatic flows.

Lin 721: it's not that simple: the lowest ratio is at the 100x100m scale.

Line 799: this uncertainty is unrealistically low. Significant noise is almost certainly introduced by the assumption of stationarity (partly, assuming the ~20-minute flight represents an instantaneous snapshot of a static field, and partly, that even the instantaneous snapshot would be useless unless many snapshots are averaged to capture all relevant eddies), by interpolation, etc. Look at Fig. 11 and imagine all the potential variability inside the box that you can't see!

Section 4.5. I think moving this section forward, e.g. right after 4.2, would improve the flow.

Line 865: spatial heterogeneity, sure, but also advection of concentration "blobs", coupled with the finite integration time, sequential vs. instantaneous measurements etc. In 20 minutes, a lot can change. The 200m box can be traversed by a 1m/s wind in 200s; if that is your round-trip time, you may consistently hit the maximum of a hypothetical sinusoidal wave with a wavelength of 400m on the upwind side of the box and the minimum on the downwind side (as a simplified example).

Refs: Yang et al., 2025 link does not work.