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Atmospheric Measurement Techniques Discussions

Interactive comment on "Surface flux estimates derived from UAS-based mole fraction measurements by means of a nocturnal boundary layer budget approach" *by* Martin Kunz et al.

Anonymous Referee #2

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General comments:

Overall, this is an excellent and exciting paper. It demonstrates a novel application of UAS for atmospheric science and adds to an exciting literature concerning the new horizons this sampling platform offers. It is a proof-of-concept study, intended to demonstrate the potential use of UAS in CO2 biospheric respiration measurement. It identifies the challenge and importance of nocturnal respiration measurements and the gap that EC methods (and limited spatial scale of chambers) cannot fill. It proposes a mass balancing approach suited to night-time measurement, taking advantage of the assumption of a stable boundary layer. Given that this is an initial study, intended to open up

a new direction in this field, some of the questions about the validity of the flux method itself (see specific comments) should be seen in that context, i.e. that this paper identifies a problem and suggests an innovative approach that can be built on and refined in future work. I believe the paper would be of great interest to readers of AMT and the quality of presentation, figures etc is excellent. I specifically praise the way the authors have carefully considered the specific challenges of rotary UAS sampling (i.e. the influence of downwash, instrument response time, etc) and proposes a solution to only use descent profiles to avoid disturbance and take into account response time. These factors are often overlooked and this paper serves as excellent guidance. The paper also compares UAS results with chambers and raises some interesting questions.

I do have some important comments though. These concern the UAS flux approach and the way in which surface footprint and vertical mixing scales have been derived (see specific comments). I hope that these comments can be addressed or answered in a revised version of the paper. I see this method as something that can be improved upon in future work and perhaps the most important edits to the text could highlight the remaining uncertainties and challenges to the approach.

Specific Comments:

1/ Use of STILT to define footprint: I have sympathy with the approach and I do not have a good alternative solution to accurate night-time footprint evaluation, however Lagrangian trajectories near to the surface are known to be subject to significant error/uncertainty. Surface trajectories tend to hug the surface and follow (typically) the 10 m wind vector suggested in the reanalysis met data used to drive the model (in this case ECMWF 0.1 IFS), i.e. upward/downward motions are supressed. How many vertical levels does this version of ECMWF have and what resolution in the vertical domain used in the study? The approach used here is to release 10000 particles per time-step at very small increments in height up to some assumed mixing height (see comment below). I would raise some concerns with this approach. Perhaps an improvement may be to run STILT in ensemble mode – to perturb each trajectory with some assigned uncertainty (diagnosed from the ECMWF data or obtained by drone-based wind measurement variability in future) to the wind vector to examine advective uncertainty - Section 5 nicely acknowledges the future role of wind measurement. A set of releases at different heights is unlikely to recreate any meaningful 2D footprint as the trajectories will cluster along one singular wind vector (as Figure 13 tends to show) extracted from the ECMWF model grid (0.1 deg is $\sim$ 10km of fetch after all), whereas an ensemble may at least give a better qualitative indication of the possible extremes of the fetch/footprint. This is likely to be the biggest source of uncertainty in any Lagrangian budgeting approach and I think it may be important to state this in the paper, even if an ensemble approach is not used in any revision. I realise that footprinting is extremely difficult but it would be useful to acknowledge just how difficult and error-prone it is. The same is true of EC footprints in topographically-variable environments of course.

2/ P.12 line 1 – why is it expected that "Surface fluxes are expected to be diluted into a column that extends from the surface to 1/2 this height in each time step"? This seems rather arbitrary? Why is this expected? How was it derived from ECMWF data? In a stable night-time boundary layer, what is the vertical mixing process assumed to reach this quantitative mixing-height value? In stable NBLs, vertical dilution is dominated by diffusion with some small residual vertical turbulence, e.g. the "fanning" Pasquill stability class. Given that assumed vertical mixing timescales (and horizontal footprint) are key to deriving flux per unit area in the footprint using the proposed method, these quantities are key. This (and comments below and above) cause me to start to question the overall flux method as it stands. Wouldn't a much more conceptual and simple approach simply be to look at the temporal gradient in CO2 throughout the NBL throughout the night and assume a fetch equivalent to the length scale of advection over that timescale (e.g. treating the NBL like a large-scale vented flux chamber, so long as footprint can be defined)? Such a concept would negate a diagnosis of any spatial heterogeneity in flux (arriving at a bulk net flux for a defined airmass volume) but I don't have any confidence that the proposed approach can do anything better

than this in reality (without a fleet of drones that is). In summary, I'm not convinced that any useful 2D footprint can be obtained, so averaging the accumulated NBL mass over any surface area is problematic, so a simplified NBL bulk net flux approach may be more meaningful?

3/ Other sources of flux uncertainty: These include the assumed background CO2, any variability in upwind sources of CO2 (i.e. variability in the background airmass entering the footprint over the time frame of the measurements), measurement error/precision, wind speed and direction variability etc. Section 4.2 and 4.6 addresses measurement error nicely and explores sensitivity, but not the other sources of flux error. Perhaps it would be good to note these in the paper, even if they cannot be determined or budgeted in this work, so that others following or improving on the work are aware.

4/ Use of w from ECMWF and the nature of night-time lifting or subsidence (page 11): I'm not sure that large scale vertical motions need to be considered in the proposed flux approach. The effect of subsidence is to suppress the night-time boundary layer, i.e. to move the night-time inversion lower. Lifting would act to lift the inversion and entrain air from above (diluting the NBL and therefore XCO2). Since this approach treats the NBL as a flux chamber (in effect), this motion seems not to be important and implicit (i.e. manifest) in the concentration measurements themselves. Or have I interpreted this incorrectly?

Technical comments:

Remember to add spaces between quantities and units (e.g. 100kmˆ2 on line 11) and other instances.

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