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Comment on egusphere-2022-384

Ankur Desai (Referee)

Referee comment on "Eddy-covariance carbon fluxes of a heterogeneous forest: one tower - two heights" by Alisa Krasnova et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-384-RC2, 2022

Krasnova and colleagues evaluate sources of differences in eddy covariance fluxes made at two heights, 30 and 70 m on a tall tower at the SMEAR Estonia tower site. They conclude that the lower height is more affected by nearby clearings, leading to a smaller sink or larger sources, especially in calm conditions. Further, use of a wind directional rectification approach did not significantly alter this effect but allowed for estimation of a carbon budget in line with expectations. Overall, the contribution is useful for understanding the importance of evaluating sources of footprint bias and approaches to account for those in making reliable, defensible flux measurements over heterogenous ecosystems.

While the concept is useful, the manuscript lacks key consideration of drivers of flux profile in the atmospheric surface and boundary-layer. An eddy flux measured at a specific height (Fc) is not necessarily equivalent to the surface flux (NEE). Rather it is the flux at that height, and most flux profiles have linear slopes in the boundary-layer beyond the lower surface layer, of which 70 m would often not be part. So it should be no surprise that the two fluxes differ by some magnitude. Without accounting for flux divergence, advection, and storage flux, it is not clear that the two heights can be compared. My recommendation is thus the manuscript be rejected now, but a resubmission that addresses this oversight would be a positive contribution to the literature.

1.) In particular, the authors appear to have failed to account for the known effect of storage flux, the accumulation of flux under sensor height. Storage flux underestimation would be consistent with the delta_NEE (30-70) term being positive under a strong surface source, and during calm wind, low u* conditions at low light or night.

This is something I showed at our own 396 m tall tower site with fluxes measured at 30, 122, and 396 m for methane and CO2 fluxes in Desai et al., 2015. I encourage the authors to take a look and see if a similar analysis would be possible here using the CO2 concentration profile, even just from the two heights. Similarly, this particular site

(Ameriflux/Fluxnet site US-PFa) has been operating since 1996 and there have been several publications looking at flux profiles and differences with height, including Davis et al., 2003, Berger et al., 2001, and Yi et al., 2000. It's too bad these were missed. Note, however, I do not require the authors to cite any work I mention - but hopefully these papers give you some guidance on how to approach the comparison here from a similar type of set up.

2.) I don't doubt that footprint differences are also a driving factor, but without an analysis of the flux profile and including storage flux (a known issues for tall towers back to foundational work on Massman and Lee, 2002), it is difficult to truly test your hypothesis. Beyond the storage flux issue, there is also the relatively qualitative way the authors try to evaluate the role of clearings and land surface with wind direction at the two heights. Since the authors have already run the Kljun 2D footprint model to construct the footprint climatology, I recommend developing a simple land cover map for both heights and evaluating the relative contributions to the footprint climatology. Also, given that the delta_NEE signal does have a wind directional signal, it would seem wise to try to look at the specific footprints during periods of max difference and identify whether the land cover fractions differ significantly (and whether the clearing is a major influence on the 30 m level).

I wonder if another way to test for this is to look at the same differences for CO_2 but for H_2O or heat flux? One might expect that lower GPP element from the clearing is driving the flux difference, one would also see differences in higher heat fluxes (more surface heating) and lower water fluxes (less transpiration). This is not a required correction, but could be a fun way to further support the hypothesis.

3.) The use of the chamber fluxes is relatively minimal. Personally, I find it provides compelling evidence that EC70 is more representative of the landscape (assuming the chambers are representative of the landscape), as both the magnitude and pattern more closely resemble EC70. I would suspect, given the forest cover here, that Rs would be more on the higher end of Rs/ER ratio report (0.3-0.8). Among other things the cited paper by Barba et al demonstrates that at many sites RSoil often exceeds REco, or is very similar in magnitude (look at Table 3 and the conclusion within it). It's not clear to me where the 0.3 to 0.8 range is actually mentioned in the citations. Davidson and Janssens makes no mention of ratios as far as I can tell. Either more work needs to be done to fully incorporate analysis of chamber fluxes, or it should be removed.

4.) The wind direction normalization from Hadden and Grelle (2017, also cited as 2016, is that a different paper?) seems like a good first test for estimating annual budgets, but there have been some more advanced approaches in recent years. For this manuscript, since the goal is just to demonstrate how consistent budgets are after normalization, probably not a major issues. But take a look at Griebel et al., 2020, Wang et al., 2006, Metzger, 2018.

Citations:

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Sincerely,

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