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Discussions

Interactive comment on "On surface fluxes at night – the virtual chamber approach" *by* Bruce B. Hicks et al.

Anonymous Referee #1

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The authors propose a novel approach to estimate fluxes exchanged between the surface and the atmosphere in nocturnal conditions. The method is based on a so-called Âń virtual chamber approach Âż, which consists basically to estimate the storage term every five minutes and to infer its value in the absence of advection and turbulence. Under such conditions, it is supposed to equal the flux.

As estimating fluxes during nocturnal conditions remains a big challenge, there is a clear need for new approaches. The one proposed here has the advantage to not require additional measurements as those already provided by current flux towers. Only a recalculation of tracer concentrations and wind velocity means and variances on a five minute basis is required. It could thus be implemented at many different sites on existing data sets. However, I see two big weaknesses in the approach, which let

me think that it is not yet mature and that a strong reappraisal of the method is needed.

First, if I understand well, the method relies on the hypothesis that vertical gas concentration profiles are flat (in the closed chamber option, Eq. 7), or linear (in the vented chamber option, Eq. 8). This is not realistic, vertical concentration gradients are generally much higher close to the surface than upward (see, for instance, profiles in Aubinet et al., 2005). This suggests that the method cannot be applied as it is in forests (where the problem of night flux estimate is the most critical) and is also probably questionable over shorter vegetation. Replacing a single point concentration by a profile measurement could certainly improve the method (Nicolini et al., 2018).

Secondly, the method is not validated. The authors present flux estimates based on three measurement campaigns but they recognize that their method clearly underestimates the fluxes in the third campaign, due to a too low data acquisition frequency, and that the values they propose in the two first campaigns are only orders of magnitude. I think that a better validation could be provided by comparing their estimates with eddy covariance flux estimates captured during turbulent nights at the same site.

In addition, I'm surprised by the numbers they propose: at the Ohio site, their approach provides a flux estimate of 1-2 μgm-2s-1 which is 15-30 times lower than the average respiration rates observed at Fluxnet sites (Baldocchi et al., 2018). This is not totally impossible but appears in contradiction with another publication by the same team: O'Dell et al (2015) indeed reported that, at the same site, one plot emitted 146 g CO2 m-2 on 104 days, which would correspond to an average of 16 μgm-2s-1. At the Zimbabwe site, the authors obtained an average of about 20 μgm-2s-1 , which is a rather realistic order of magnitude ; however, here again, the same team (O'Dell et al, 2018) reported at this site emissions of 197 and 235 g m-2 over 139 days, which corresponds to a three times higher average. It is clear that these values could not be compared directly as they are not taken at the same time scale. However, in view of the differences in the orders of magnitude, no indication is given that the virtual chamber approach provides reasonable flux estimates but I'm rather pushed to think that it underestimates the fluxes, which would be logical if vertical concentration gradients are not taken into account. A more detailed validation and, possibly, refinements of the method are thus needed, which seems possible in view of the available datasets from the Fluxnet or the ICOS networks.

I have also a remark concerning the statistical treatment: partial correlation coefficients are computed and their time course is presented in the results but there is no mention on how they are taken into account in the study. Are they used as quality criteria? If yes, which thresholds are considered? Moreover, how do the authors justify the choice of these specific criteria (in place, for example of the standard error of ax and ay coefficients, which would provide a direct estimate on the flux uncertainty)?

I have no specific comments on the paper structure and writing, which are both good. Just one remark concerning figure 1b: I suppose that the black line represents the CO_2 concentration evolution with height and not its gradient as indicated on the line (the gradient is the slope of the line). This should be clarified.

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