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Comment on acp-2022-504

Anonymous Referee #1

Referee comment on "Diurnal variability of atmospheric O_2, CO_2, and their exchange ratio above a boreal forest in southern Finland" by Kim A. P. Faassen et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-504-RC1, 2022

Review of "Diurnal variability of atmospheric O_2, CO_2 and their exchange ratio above a boreal forest in southern Finland" by Faassen et al.

In this paper, the authors present diurnal variations in $\delta(O_2/N_2)$ and CO_2 observed at two heights above the boreal forest. They calculated ER_{forest} and ER_{atmos} based on flux and concentration measurements, respectively, and found ER_{forest} and ER_{atmos} cannot be used interchangeably. The authors also applied the observed ER_{forest} to separate the NEE into GPP and TER, and they found comparable results to the commonly used eddy covariance approach. These findings supported and refined the discussion by Seibt et al. (2004) and Ishidoya et al. (2013, 2015) who reported differences between ER_{forest} and ER_{atmos} and its application to forest carbon cycle. There are only a few data sets of continuous measurements of both $\delta(O_2/N_2)$ and CO_2 over forests, and accurate estimate of ER_{forest} at various forests is highly important for not only forest but also global carbon cycle. This paper makes a valuable contribution in this respect. However, I find some issues in the observed variations in $\delta(O_2/N_2)$ which should be addressed before publication.

Main Points

The authors ascribed the temporal decreases of O_2 and CO_2 between 13:00-20:00 (P3b) in Figure 4 to a remaining artefact that could not be corrected for with the pressure correction associated with the instability of the MKS pressure regulator in 2019. If so, I think the artefact also superimposed on the O_2 data during the other periods (P1, P2, and P3a), and I am concerned about the unrealistic values of ER_{atmos} of 2.28±0.01 and 2.05±0.03 found in Fig. 5 are also attributed to the artefact. In my experience, larger ER_{atmos} than 2.0 has never been observed in a diurnal cycle at a forest in a growing season. I recommend the authors to create the aggregate day based on the periods other than 7-13 July, 2019, and calculate the ER_{atmos} for the average diurnal cycles. Especially,

the ER_{atmos} in 2018, when the pressure correction was not applied, will be useful for comparison. If larger ER_{atmos} than 2.0 is also found in the average diurnal cycles in 2018, then the value will be reliable. However, if larger ER_{atmos} than 2.0 is found only in the diurnal cycles in 2019, then it may be due to the artefact and the ER_{forest} may also be affected by the artefact. To discuss differences between ER_{forest} and ER_{atmos} properly, it is important to rule out the possibility of the significant effect of the artefact.

Other Specific Points

1) Line 175-178 and Table 1: What does "our own calibration" mean? Did the authors calibrate the target cylinder using the primary Scripps cylinders by themselves? I think the declared value with calibration in Groningen is based on SIO scale. Therefore, the values of target cylinder based on "our own calibration" should also be on SIO scale to calculate the mean of the difference.

2) Line 190-192: Related to the main points, the period of 7 through 12 July 2019 to create the aggregate day is shorter than that by Ishidoya et al. (2015). I am concerned about the artefact during this period considering the very high ER_{atmos} found in Fig. 5.

3) Line 223-226: The authors calculated the ER_{forest} from means of the O_2 and CO_2 flux during night, day, and entire day. I think it can also be calculated by applying a linear regression between O_2 and CO_2 flux (or ΔO_2 and ΔCO_2) on the points as Ishidoya et al. (2015, 2020) did. Wouldn't this method reduce the uncertainty on ER_{forest}?

4) Figure 4: Do the error bars indicate standard error? Please specify.

5) Figure 7: The ER_{forest} is negative value in this figure, although it is defined as positive value throughout the paper. Please be consistent with the terms you use.

6) I think it would be better to add the references and/or brief description of the EC method and temperature-based function used in this study, since comparison of EC method and O_2 method in Fig. 8 is an important topic.

7)The words "Eddy Covariance (EC)" appears repeatedly at line 30, 131, 227, and "Eddy Covariance fluxes" and "eddy-covariance CO_2 flux" also appear at line 429 and 634, respectively. I think it's better to use "EC" throughout the paper after the definition at line

30.