Comment on acp-2022-504
Anonymous Referee #1

Referee comment on "Diurnal variability of atmospheric O2, CO2 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 O2, CO2 and their exchange ratio above a boreal forest in southern Finland” by Faassen et al.

In this paper, the authors present diurnal variations in δ(O2/N2) and CO2 observed at two heights above the boreal forest. They calculated ER\textsubscript{forest} and ER\textsubscript{atmos} based on flux and concentration measurements, respectively, and found ER\textsubscript{forest} and ER\textsubscript{atmos} cannot be used interchangeably. The authors also applied the observed ER\textsubscript{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\textsubscript{forest} and ER\textsubscript{atmos} and its application to forest carbon cycle. There are only a few data sets of continuous measurements of both δ(O2/N2) and CO2 over forests, and accurate estimate of ER\textsubscript{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 δ(O2/N2) which should be addressed before publication.

Main Points

The authors ascribed the temporal decreases of O2 and CO2 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 O2 data during the other periods (P1, P2, and P3a), and I am concerned about the unrealistic values of ER\textsubscript{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\textsubscript{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 $\Delta O_2$ and $\Delta CO_2$) 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.