Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2022-504-RC2, 2022

Comment on acp-2022-504

Anonymous Referee #2

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-RC2, 2022

Diurnal variability of atmospheric O_2, CO_2 and their exchange ratio above a boreal forest in southern Finland

Faassen et al. present a highly novel dataset of O_2 and CO_2 measurement in the surface layer over a boreal forest. Such measurements are technically very challenging making this study one of the very few so far that have succeeded to apply O_2 in micrometeorological land surface flux measurements. Typically, the signal to noise ratio in O_2 gradient above forests is very small which limits the application of the flux gradient methods. Here the authors make use of a 125 m tall tower to increase the O_2 gradient.

A major challenge in this study is that the measurement uncertainty of the O_2 system is below comparable systems. This limits the interpretation of the data. Nevertheless, in my view the authors found a suitable way forward by aggregating the data to a "representative day".

While the experimental design and analysis is well done, there are several aspects that need to be addressed before publication.

Major comments

- Footprints: A major question regarding the study is that the ERatmos values are much higher than in previous studies. Some potential reasons are discussed in lines 481 to 490. What I am, however, missing is a proper treatment of the concentration footprints. Firstly, they differ between heights, particularly if the height difference is 100 m. This could lead to situations where the bottom height sees the local land surface whereas the top height sees air influenced at a regional level. Secondly, right next to the towers (roughly 200 m) is a large lake. Given that lakes have different O_2:CO_2 exchange ratios, I am wondering how this would influence the observed signal. Some of the co-autors have published articles on eddy covariance flux measurements over that lake. For the manuscript it would be help to add a footprint analysis and evaluate and discuss the influence of these two aspects on ERatmos and ERforest.

- Flux partitioning: It I understood correctly, the exchange ratio of assimilation (ERa) is calculated based on equation 8 assuming a constant ERr and ICOS data of NEE, GPP and TER (line 276). Once ERa and ERr are retrieved for one representative day, these values are used to calculate GPP and TER on other days. For me it is not clear what we learn from this exercise as GPP is used to constrain ERa and then ERa used to constrain GPP. Other studies such as Wehr et al. 2016 Nature have shown that NEE partitioning with an independent method using 13C in CO_2 resulted in lower TER and lower GPP compared to the temperature-based function following Reichstein et al. 2005 possibly indicating a Kok effect. If now the temperature-based GPP is used to calculate ERa, the O_2 based method does not provide additional and independent information. While I understand that the authors have no independent measurements of ERa at hand, I still miss a more careful discussion including Wehr et al. 2016 and addressing the limits of this approach.

Minor comments

Line 23: better “net uptake” than “uptake”

Line 23 and 24: better be consistent using ether land biosphere or terrestrial biosphere

Line 27: Add a citation for last sentence in first paragraph.

Line 30-32: here I am missing a mentioning of Wehr R et al. 2016 Nature where they showed that fluxes partitioning using 13C differ from fluxes partitioning following Reichstein et al. 2005.

Line 36/7: fluxes of O_2 and CO_2 are opposite. Here a positive ER is used. It might be helpful to indicate this by saying “indicates the amount of moles O2 consumed per mole of CO2 produced (or vice versa)‟.

Figure 1: in the text of the introduction the term GPP and TER are used and in figure 1 respiration and assimilation. Please use consistent terms.

Line 90 to 94: personally, I prefer if the given objectives are presented with the term "objectives" for allowing speed-reading. Maybe a matter of taste

Line 113: what is the influence of the nearby lake on the exchange ratio. The footprints at 23 m and at 125 m are very different. How does this influence the results?

Line 129/130: It seems that the sampling lines are alternatingly flushed with 120 ml/min and 2 l/min. Has it been evaluated whether these changes in flow rate lead to any effects on the O_2 signal? Or are all these effects removed by discarding the first 4 minutes after switching.

Line 210: I find it confusing that in equation 5, eddy covariance terms for the turbulent fluxes are presented, but the turbulent fluxes are obtained from flux gradient measurement. Why are not equation 5 and 6 combined?

Line 218: In my view it is not the stability that characterised if in a period respiration or assimilation dominate, but it is the radiation regime. Why was here stability used and not nighttime vs. daytime?

Line 255: unit is missing. Should be "0.4 m s-1".

Line 271: here it is referred to ICOS NEE and GPP from EC measurements. It would be good to say how ICOS partitions NEE into GPP.

Line 304: why was a fixed calibration time during the day selected (20:00 - 22:00). An alternative could be using a moving calibration time.

Line 307: 0.70 ± 0.65: the unit is missing.

Fig. 4a: for the height 23 m, the CO_2 concentration varies with a range of 15 ppm, whereas the O_2 concentration varies with a range of 35 ppmEq. Wouldn't we expect to see a similar range of variation? What is the role of the nearby lake?

Fig. 4b: at night we see a vertical gradient in O_2 concentration (roughly 10 ppmEq) that exceed instrument precision (roughly 4 ppmEq), but during daytime the gradient is – even averaged over multiple days – lower than instrument precision. To me it is unclear how the uncertainty of the measurements is propagated to the final fluxes and ERforest.

Line 318: in P3b: O_2 and CO_2 concentration changes show the same sign, instead of the expected opposite sign. This is related to an instability of the MKS pressure regulator. It is unclear why this effect should only affect P3b and no other times of the day. How was this evaluated?

Fig. 5: Which regression type was used to calculate the regression?

Line 339: Given that the measurement uncertainty is so high compared to the variation during P3a, I am wondering how the uncertainty could be included via error propagation when calculated the slope and its uncertainty.

Fig. 6: The units of the fluxes are given in ppm m m-1. This is very unusual for the flux community. Typically, the fluxes are reported in µmol m-2 s-1. Also, I find it confusing that the y-axis label is the covariance, but the fluxes are calculated from a flux-gradient approach and not from eddy covariance.

Fig. 6b: Could please describe in the caption what are the error bars. Could just be moved from the main text (line 380). Also here, it is unclear to me if an error propagation incl. measurement uncertainty was carried out.

Fig. 7 : It is surprising to see ERforest values at -2 to -2.5. This is much more negative that other reported data and it is unclear what this could mean physiologically. It is also surprising that the fluxes with the most negative values are also the largest fluxes, where we would expect to see large gradients and thus robust flux calculations.

Appendix: Personally, I prefer that the units are shown as well.