Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC3
https://doi.org/10.5194/acp-2022-504-RC3, 2022

Comment on acp-2022-504

Anonymous Referee #3

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-RC3, 2022

Overall, this is very nice paper presenting important results. The authors conduct challenging measurements, analyze the data intelligently and combine their own data with ancillary datasets in a clever way to extract interesting values. They focus on the O2/CO2 exchange ratio for a boreal forest (unprecedented) and then extend their work to seperately assess the exchange ratios associated with respiration and assimilation. The work is valuable, the paper is generally well organized and it definitely deserves publication.

That said, I do have some concerns that need to be addressed prior to full acceptance:

- The authors use α_b, ER and OR somewhat interchangeably in the introductory part of the paper. Each of these symbols really does have a distinct and specific meaning. Although the use of these terms in the literature has been somewhat sloppy, as our field matures it becomes more important to use the right word in the right context.
- The authors assume ERr is constant day and night. This may well be true, but it's possible it isn't true. Since this assumption is central to the subsequent analysis, there should be more discussion of this assumption and its validity.
- The data were compromised at times by the failure of some MKS pressure/flow controllers. The authors apply a correction to the data, but there are a few points with (apparently) anomalous values where we're told that the correction simply wasn't adequate. Since we aren't told any of the details of the correction, I'd like to see evidence that the other (non-anomalous and corrected) data are valid, and not just because their values are close to what we expect.
- The authors attribute differences between ER_{atm} and ER_{forest} to "boundary layer dynamics and entrainment" or the unique nature of boreal ecosystems. I think the first explanation misses the point and the second if very likely wrong. Whenever you see O2 and CO2 changing with time with a slope more negative than -1.2, this indicates the influence of fossil fuel combustion.
- There appears to be circularlity in some of the analysis. For example, the EC data are used to set a value of the free parameter K (a transport coefficient) for getting fluxes from O2 gradients. Then the O2-based fluxes are assessed by comparing them to the EC data. Similarly, NEE is split into GPP and TER using the O2 and CO2 data. Then the

O2 and CO2 data are further interpereted by taking GPP and TER as if they were known a priori.

It's quite possible (particularly for #5) that the authors have done nothing wrong and I have simply failed to understand their work. If that's the case, then my comments should be taken as a plea for clarification and explanation in the text.

All of these concerns, along suggestions/corrections on word choice, punctuation, sentence structure and grammar, and covered in the attached "marked up" PDF. The markings are in three colors: Red - add/delete/move text, to be taken verbatim Green - questions/directives for the authors Yellow - highlighting text for which I have typed a "sticky note". Be sure to open the note and read to the end. Scrolling may be required.

Finally, I would like to acknowledge that the writing quality is very high. Even though I have made numerous editorial markings, as a native English speaker (with a modest proficiency in German) I am in awe of the authors' ability to write so well in a second language. Well done!

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2022-504/acp-2022-504-RC3-supplement.pdf