

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2021-641

Anonymous Referee #2

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Referee comment on "Biogeochemical and biophysical responses to episodes of wildfire smoke from natural ecosystems in southwestern British Columbia, Canada" by Sung-Ching Lee et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-641-RC1>, 2021

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### General comments

The paper by Lee et al., aims to characterize the biogeochemical and biophysical responses to wildfire smoke episodes from a forest and a wetland ecosystems by using eddy-covariance and meteorological datasets.

Although the paper addresses a relevant scientific question, in my opinion it lacks of substantial conclusions, and the reported ones are not sufficiently robust and supported by scientific evidence. The main limits of the current manuscript - as also stated by the authors - are all related and are:

- Few observations (in their current form): despite the wildfire episodes are sporadic and limited in time (there is not much to do for this) data can be treated and aggregated in a more sophisticated way than by the simple day-to-day comparison (e.g. creating layers of data using some representative ancillary variables)
- the attributions of ecosystem response: the very likely presence of confounding effects is not treated in an accurate way so that to exclude (or limit and identify) their effect on results.
- the selection of experimental and control datasets: the simple definition of "smoky" and "non-smoky" days, with the latter being the remaining day of the month, risk to confound the picture too much. This is a challenging task, but a more sophisticated method (e.g. a data driven approach) could be explored.

Some of the literature cited in the paper could provide good methodological examples (e.g. Hemes et al., 2020; Park et al., 2018).

## Specific comments

- It would be important to have a proxy variable to scale the AOD values to the measurement sites (e.g. PAR measured at the Saturna Island) and use this to select (or verify) the smoking days (or hours).
- Why did you use different partitioning methods for the two sites? This adds further uncertainty and makes respective estimates harder to compare
- At line 284:285 you report, within brackets, that control days are "all the remaining days in the same month". This criteria must be better clarified and described reported in the method section
- 263-270: did you compare the estimated diffuse PAR fraction with measured data when available? (e.g. from 2017 at the Buckley Bay site)
- Given the small numbers of samples and their distributions, the differences between smoky and non-smoky days should be tested by non-parametric statistical methods.
- The difference in PAR between smoky and non-smoky days seems to be never significant (except for August 2017 in the wetland site).
- The same holds for the diffuse radiation
- The Section 3.1.1 should be drastically reduced, moving all the text describing Fig. 3 in the supplement and keep the most relevant info.
- 360 this is true for the smoke-free days too.
- Section 3.1.3: the described differences are never significant
- Section 3.1.2 Albedo and energy partitioning:
  - Avoid basing discussion on single days. As you actually mention and as it is visible from Fig S4, heat fluxes are extremely variables. It's hard to draw robust conclusions here
  - ll 360:362: No need to compare flux absolute values between the two sites.
- Section 3.2.1 Net ecosystem exchange
  - 392:394: you report that "Both sites became a stronger CO<sub>2</sub> sink ... These increases were statistically significant in the first two years." It is actually true that the significance (assuming its estimate is appropriate) is verified for the first two years in the bog and just for 2017 in the forest.
  - I would not discuss single-day values (e.g. ll 403-406), or base a conclusion on 2 days (e.g. ll 430-421)
  
- The same consideration as for NEE hold for GPP and Reco discussion
- Section 3.2.3 Relationship between smoke and gross primary production
  - the title is not reflected the text. In addition, the relation between PAR<sub>g</sub> and the diffuse fraction is not a result (and well known already) and should be moved in the supplement or in Methods
  - R<sup>2</sup>s reported in Fig. 6(b) do not seem to reflect the scatter of respective points, please check (however, it is not necessary to report them here)
- Discussions: it is good to compare results with similar studies, but it is better to limit the use of conclusions from other works

## Technical comments

- Fig. 1: please increase the quality, e.g., type of maps, distinguishing between EC sites and areas interested by fire.
- 143: insert AOD500 acronym after the word wavelength
- 143: please recall why you select the 500 nm wavelength
- 145-147: please provide some reference examples for AOD value (e.g. during clear sky)
- 2: please improve the axis labels, and align the two panel according to dates (days)
- 240: inconsistent formula for albedo: should be switched, reflected/incident
- 343: "Fig. S3" should be Fig. S4
- 412: add "g" to  $-5.40 \text{ C m}^{-2} \text{ day}^{-1}$
- It would be better to move Fig S5 and S6 into the main text