

Atmos. Chem. Phys. Discuss., referee comment RC2  
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## Comment on acp-2021-944

Anonymous Referee #2

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Referee comment on "Interannual variability in the ecosystem CO<sub>2</sub> fluxes at a paludified spruce forest and ombrotrophic bog in the southern taiga" by Vadim Mamkin et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-944-RC2>, 2022

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In "Interannual variability of the ecosystem CO<sub>2</sub> fluxes at paludified spruce forest and ombrotrophic bog in southern taiga", Mamkin et al. present CO<sub>2</sub> flux data and analysis at two taiga peatland sites in western Russia. They highlight the interannual variability in the CO<sub>2</sub> fluxes and driving meteorological and environmental conditions at and between both sites, with implications for the future net carbon balance of this region and ecosystem due to climate change.

Overall, this is an important topic and the study presented here is, for the most part, thoroughly and completely introduced, described, and discussed, with results placed in a proper context. The study data are great for long term climate trends in a sparsely monitored region, and the paper shows well how ecosystem warming has varied impacts depending on seasonal timing. However, many English-language errors greatly hinder the paper's readability and must be corrected. Additionally, all discussion of uncertainty in the CO<sub>2</sub> flux measurements and partitioning methods is absent and must be included prior to publication in ACP.

More specific comments and suggestions are listed below:

Figure 1a: This figure would be more useful with country borders, lat/lon descriptions, and more contrast in colors between the different land cover types.

Line 100: I wondered why air temperature was not used from MS site when introduced here. It is later mentioned to be not available, perhaps move this mention earlier.

Line 108: Add additional context for CMI range of values, for those not familiar.

Line 110: At which site or sites is this regional trend detected?

Figure 1b: Not cited in text.

Line 165: This paragraph continues description of OB site, but the paragraph break without further mention of OB makes this unclear.

Line 176: Should this be "2015-2020"?

Line 179: What about the "2" quality flag makes that flux worthy of being removed?

Lines 176-184: As mentioned above, this section must be expanded to include description of error and uncertainty associated with eddy flux measurement, calculation, and partitioning of GPP and TER from observed NEE. Perhaps a comparison of the derived TER and GPP from isolated NEE alone (section 2.4) with the automated partitioning would be useful. Further discussion later on should reference how the results could differ based on the potential errors and uncertainty.

Figures 2 and 3: It may be more effective to convey interannual variability in meteorology and CO<sub>2</sub> flux as anomalies from a mean set of values, rather than a timeseries. This is especially true when referring to differences on a monthly scale, such as early snow-off in a particular year.

Line 269 and elsewhere: Considering add in mention of processes when referring to numbers such as NEE. Rather than or in addition to saying "NEE decreases", one could say "net CO<sub>2</sub> uptake increases".

Line 370: Why does GPP determine the parameters between the sites? Because of relative constant  $R_g$ ?

Lines 373-379: Was there similar (any?) interannual variability in the TER parameters as for GPP shown here?

Line 473: The predictive relationships between environmental drivers and CO<sub>2</sub> fluxes mentioned here are not shown. A figure or statistics that illustrate these would be useful.

