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Comment on acp-2021-944

Anonymous Referee #1

Referee comment on "Interannual variability of the ecosystem CO2 fluxes at paludified spruce forest and ombrotrophic bog in southern taiga" by Vadim Mamkin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-944-RC1, 2022

The manuscript compares net ecosystem CO_2 exchange from a paludified spruce forest and an adjacent ombrotrophic bog in west Russia and analysed the main environmental controls on NEE and its component fluxes. The study addresses an important research question aiming at better understanding interannual variability in NEE in these understudied ecosystems. The manuscript is mainly well written but could benefit from writing improvements (e.g., grammar and wording). The methodology is sound and in general appropriate for this study. Overall, the study remains very descriptive, and, in my opinion, results should be strengthened by adding uncertainty estimates for fluxes and statistical test results when comparing bog and forest fluxes throughout the manuscript. Additionally, water availability and drought effects are discussed but observational evidence does not appear to support that these factors play a significant role. I think this could be further clarified.

I also have some more specific comments:

Line 47: The following reference could be relevant here too: Helbig et al., 2019; https://doi.org/10.1029/2019JG005090

Line 54: The following reference could be cited here too: Moore et al., 2006; https://doi.org/10.1111/j.1365-2486.2006.01247.x

Line 61-64: Helbig et al. (2019) might be relevant here too

Line 72: It might be insightful to include results from the SPRUCE experiment to the introduction and/or discussion (https://mnspruce.ornl.gov)

Line 78: Another paired flux tower study comparing forested and non-forested peatlands in the sporadic permafrost zone is published by Helbig et al. (2017; https://doi.org/10.1111/gcb.13638)

Line 83: Park et al (2021; https://doi.org/10.3390/atmos12080984) is another study on Russian peatlands.

Line 93: I think the latitude/longitude coordinates should be listed here for both sites.

Line 106: The growing season definition could already be introduced here.

Line 146: It is unclear what a “standard design” is. Please clarify.

Line 148: The tower height is 29 m, but trees reach up to 27 m. It seems as if the EC measurements could be most of the time in the roughness sublayer affecting the validity of the essential EC assumptions. Perhaps the authors could explain how this potential issue was addressed.

Line 200: Why was VPD not included in the GPP response?

Table 1 and other tables: At least for the long-term means, the standard deviation should be included in the table. It would also help to characterise how strong the climate anomalies were.

Line 262: Is there a relationship between precipitation and water table depth?

Line 273: This is one example where the claim that “strong dependence … was not found” should be backed up with statistical methods.

Line 284: It is unclear where this hypothesis is coming from and how it is backed up.

Table 3 and other flux tables: Should include uncertainties in aggregated fluxes.

Line 324: Leaf-on and leaf-off might not be the right terms for evergreen ecosystems. Start and end of growing season might be more accurate.

Line 355: It seems as if the Q10 model was fitted to the entire dataset. Did the authors consider analysing short-term variations in temperature sensitivity (see Reichstein et al., 2005; https://doi.org/10.1111/j.1365-2486.2005.001002.x).

Conclusions: In my opinion, the conclusion would be more impactful if it was shortened and if the main take-home messages were highlighted here.