Response to the Anonymous Referee #1.

We thank very much Anonymous Referee #1 for the helpful and constructive comments and recommendations. The manuscript has been revised in accordance with reviewer's comments and suggestions to produce an improved version of the article.

[comment 1]

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.

Response

We asked Copernicus English copy-editing service if we need to correct English before the submissions, they replied us that they usually recommend submitting the paper as is, and that English copy-editing is a standard procedure for final revised papers accepted for final publication in ACP.

Uncertainty estimates and statistical analysis has been added to the revised version of the manuscript.

Our research is focused on the CO_2 fluxes in 2015-2020 period. The droughts were not observed during this period but such events can potentially influence CO_2 exchange of the peatlands in the future. We decided to include only discussion of this effects with corresponding link on the paper where influence of drought on CO_2 fluxes was described (Kurbatova et al., 2013).

Kurbatova, J., Tatarinov, F., Molchanov, A., Varlagin, A., Avilov, V., Kozlov, D., Ivanov, D. and Valentini, R.: Partitioning of ecosystem respiration in a paludified shallow-peat spruce forest in the southern taiga of European Russia, Environ Res Lett, 8(4), 045028, doi: 10.1088/1748-9326/8/4/045028, 2013.

[comment 2]

Line 47: The following reference could be relevant here too: Helbig et al., 2019;

https://doi.org/10.1029/2019JG005090

Response

We've cited the reference and added it to the reference list (P.2, L. 42-46): "It is suggested that growing air and peat temperatures especially under raising frequency of droughts in boreal regions can significantly increase decomposition rates and switch peatlands from CO_2 sink to CO_2 source for the atmosphere (e.g. Alm et al., 1999; Moore, 2002; Lund et al., 2012; LaFleur et al., 2015; Helbig et al., 2019; Loisel et al., 2021)."

[comment 3]

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

Response

We've cited the reference and added it to the reference list (P. 2-3, L. 53-56):"Previous studies reported that NEE of the peatlands is susceptible to water table depth (WTD) dynamics, air and peat temperature variations, changes in global radiation, timing of the snowmelting and peat layer thaw (Moore et al., 2006; Dunn et al., 2007; Lindroth et al., 2007; Sulman et al., 2010)."

[comment 4]

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

Response

We've cited the reference (P. 3, L. 63-66): "For instance, the forest peatlands can sequestrate atmospheric CO_2 before the snowmelting and peat thaw in spring, while a thawing is necessary for the beginning of the CO_2 uptake at non-forest peatlands (Tanja et al., 2003; Euskirchen et al., 2014; Helbig et al., 2019)."

[comment 5]

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

Response

We've added a several results from SPRUCE experiment to the discussion (P. 31-32, L. 609-616): "While warming and moistening in winter could increase GPP more than TER especially

at paludified forests, the extreme hot and dry conditions are able to increase heterotrophic respiration significantly and switch peatlands from CO_2 sink to a consistent CO_2 source for the atmosphere as well as alter NPP of the ecosystems. For example, SPRUCE experiment in Minnesota (USA) showed a significant carbon loss rates at black spruce stands on the bog (higher than its historical accumulation rates) under the warming treatment which was connected with increased heterotrophic respiration, decreased *Sphagnum* and tree above ground NPP (Walker et al., 2017; Hanson et al., 2020)."

[comment 6]

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)

Response

We've cited the reference (P. 3, L. 75-80): "Unfortunately, in spite of a numerous experimental studies focused on ecosystem-atmosphere CO_2 fluxes in different peatland types in high-latitudes in North America (e.g. Roulet et al., 2007; Gill et al., 2017), Europe (e.g. Kurbatova et al., 2002; Lindroth et al., 2007; Minkkinen et al., 2018) and Asia (e.g. Tchebakova et al., 2015; Alekseychik et al., 2017; Park et al., 2021) there is lack of studies considering the ecosystem CO_2 fluxes at the forest and non-forest peatlands located in the same landscape and undergo the similar weather conditions (e.g. Euskirchen et al., 2014; Helbig et al., 2017; Zagirova et al., 2019)."

[comment 7]

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

Russian peatlands.

Response

We have't cited this paper here because the experimental data used in the research was obtained in Asian part of the country.

[comment 8]

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

Response

We listed the coordinates of the stations to (P. 4, L. 96-99): "This study was conducted at paludified spruce forest (56.4615°N, 32.9221°E, 265 m a.s.l.) and adjacent ombrotrophic bog (56.4727° N, 33.0413° E, 240 m a.s.l.) located on the territory of the Central-Forest state natural biosphere reserve (CFSNBR) in the south-western part of Valdai hills in Tver region of Russia (Fig.1a)"

[comment 9]

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

Response

We've introduced the growing season definition to (P.6, L. 109-114): "Soil surface is typically covered by snow from mid-November to late March - early April (Desherevskaya et al., 2010) and the growing season calculated as the number of days between the first 5-day period with mean daily air temperatures above 5°C (Leaf-on day) to the first 5-day period with mean daily air temperatures below 5°C (Leaf off day) following (Urban SIS, 2018; Buitenwerf et al, 2015; Donat et al, 2013; Mueller et al, 2015) lasts 182 days on average (since 12 Apr. to 11 Oct.)."

[comment 10]

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

Response

We've deleted the word "design" from the (P.7, L. 155): "Flux stations at PF and OB sites have a standard instrumentation for FLUXNET network."

[comment 11]

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.

Response

We've updated the information about tree heights and also, added DBH. According to the last survey at PF site (November 2021) the mean tree height around the tower is 16.9 m, but several trees in the ecosystem reaches 27 m (P. 6, L. 130-131): "The mean tree height is 16.9 ±6.4 m (±SD) with mean diameter at breast height (DBH) 21.6±8.9 cm (±SD) and undergrowth is about 0.3 m.".

[comment 12]

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

Response

We haven't included VPD in the GPP-Rg response analysis due to the lack of GPP data obtained under high VPD. VPD at the ecosystems changed in the narrow range: in early spring and late autumn day-time VPD at PF and SB sites didn't exceed 5 hPa, even in the driest summer 2018 about 90% of the 30-min day-time VPD values were less than 10 hPa and about 75% of the day-time data were less than 5 hPa.

[comment 13]

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.

Response

We've added the standard deviations of the long-term mean air temperature and precipitation values to Table 2 and Table 3.

[comment 14]

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

Response

As it shown in Tab. 1 mean annual WTD negatively correlated with annual precipitation, but parametrization the relationship between precipitation and water table depth on shorter scales is challenging due to the lagged response of the WTD on precipitation.

[comment 15]

Line 273: This is one example where the claim that "strong dependence ... was not found"

should be backed up with statistical methods.

Response

We've decided not to consider the dependence between the annual sums of the CO_2 fluxes and GSL due to the small number of samples. We expressed the sentence in another way (P. 14, L.290-292): "Maximal and minimal annual NEE, TER and GPP at PF site were not correspondent with maximal and minimal GSL."

[comment 16]

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

Response

We've removed this sentence from the text.

[comment 17]

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

Response

We've added uncertainties to Table 3 and Table 4.

[comment 18]

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.

Response

We've replaced the words "leaf on" and "leaf off'' with "the start" and "the end" of the growing season everywhere in the text.

[comment 19]

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).

Response

We've considered the short-term variations of the temperature sensitivity and tried to find the difference between the years, months, periods and WTD classes, but these differences were very small. Thus, we've decided to include only best fitted models to show the difference between the sites. We also found it challenging to demostrate the difference between Q_{10} models fitted for short-term periods due to the lack of original nighttime NEE data obtained at the sites.

[comment 20]

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

Response

We've added the new version of conclusions to the text.