## Response to the Anonymous Referee #2.

We thank very much Anonymous Referee #2 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]**

*In "Interannual variability of the ecosystem CO2 fluxes at paludified spruce forest and ombrotrophic bog in southern taiga", Mamkin et al. present CO2 flux data and analysis at two taiga peatland sites in western Russia. They highlight the interannual variability in the CO2 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 CO2 flux measurements and partitioning methods is absent and must be included prior to publication in ACP.*

**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 its discussion have been added to the revised version of the manuscript.

**[comment 2]**

*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.*

**Response**

We've changed the Figure 1a.

**[comment 3]**

*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.*

**Response**

We've added this information to (P. 5-6, L. 106-109): "Long-term mean annual precipitation (1991-2020) measured at meteorological station "Lesnoy Zapovednik" (56.50° N, 32.83° E, 240 m a.s.l.) – the nearest meteorological station to the study area was 778 mm (continuous air temperature data from "Lesnoy Zapovednik" meteostation for 1991-2020 period is not available)".

**[comment 4]**

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

**Response**

We've added information about CMI index to (P. 6, L.115-118): "The climate moisture index (CMI) calculated as the ratio of annual precipitation to annual potential evapotranspiration and ranged between -1 and 1 (Wilmott and Feddema, 1992) is 0.3 - 0.4 (Mamkin et al., 2019; Novenko et al., 2015)."

**[comment 5]**

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

**Response**

We've corrected the sentence (P. 6, L.118-120): "In the recent 30 years a positive trend of air temperature (+0.73 °C per10 years) and precipitation (+3.6 mm·month-1 per 10 years) was detected at the meteorological stations "Toropets" and "Lesnoy Zapovednik" respectively".

**[comment 6]**

*Figure 1b: Not cited in text.*

**Response**

We've added the citation to (P. 4, L.99-100): "The sites are located 7.5 km apart (Fig. 1b) and characterized by very similar weather conditions."

**[comment 7]**

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

**Response**

We've added the site name to the first sentence of the paragraph (P. 8, L. 174): "Additionally, global radiation at OB site was measured with 4-component radiometer NR01 (Hukseflux Thermal Sensors, The Netherlands) at 2.5 m height."

**[comment 8]**

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

**Response**

We've corrected the sentence (P. 8,L.185-186): This study is based on eddy covariance and meteorological data obtained at PF and OB sites in 2015-2020.

**[comment 9]**

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

**Response**

We edited the sentence (P. 8, L.189-190): "All fluxes with quality flag 2 was removed from the analysis following the recommendations on the data quality assessment (Mauder et., 2013)."

**[comment 10]**

*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.*

**Response**

We've added the information about uncertainty estimation to (P. 9, L.196-201): "Uncertainty of NEE, TER and GPP associated with the random error in the measured fluxes, u*-threshold estimation, gap-filling and flux partitioning procedures was calculated using REddyProc package (Wutzler et al., 2018) as standard deviation (SD) of the flux values. The aggregated random uncertainty of the seasonal and annual sums of the CO2 fluxes was obtained considering the autocorrelation between the residuals using empirical autocorrelation function (Zięba & Ramza,

2011).” Also, a section considering the flux uncertainties have been added to the discussion (P. 29, L.537-548).

**[comment 11]**

*Figures 2 and 3: It may be more effective to convey interannual variability in meteorology and CO2 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.*

**Response**

We absolutely agree that the chart with anomalies would be more useful than a timeseries graph in this context. However, we’ve decided to use a timeseries because of several long gaps in the data obtained at OB site that makes difficult to calculate flux and meteorology anomalies comparable with the anomalies derived for PF site. At least, timeseries can show to readers the general differences in seasonal dynamics of the $CO_2$ fluxes between two peatlands in spite of long gaps.

**[comment 12]**

*Line 269 and elsewhere: Considering add in mention of processes when referring to numbers such as NEE. Rather that or in addition to saying “NEE decreases”, one could say “net CO2 uptake increases”.*

**Response**

We’ve corrected the sentence at (P. 14, L.283-284) and the similar sentences throughout the manuscript: “During the 6 years of measurements $CO_2$ uptake at PF site tended to increase.”

**[comment 13]**

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

**Response**

The difference of the parameters between the sites was mostly determined by GPP due to the almost equal Rg (difference of the daily sums between the sites was on average ±3%).

**[comment 14]**

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

**Response**

The analysis of the interannual variability in the TER parameters hasn't been included to the text due to the small differences between them. We also found it challenging to research 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 15]**

*Line 473: The predictive relationships between environmental drivers and CO2 fluxes mentioned here are not shown. A figure or statistics that illustrate these would be useful*

**Response**

As an example, we've added the relationship between the residuals of the $Q_{10}$ models (calculated using soil temperature) and WTD (Figure 5).