Reply to comments of referee 2

Manuscript: Scaling and balancing carbon dioxide fluxes in a heterogeneous tundra ecosystem of the Lena River Delta

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Colour legend: Referee's comments are written in red
Our responses are written in blue

General comments

The paper "Scaling and balancing carbon dioxide fluxes in a heterogeneous tundra ecosystem of the Lena River Delta" introduces new experimental results in estimation of carbon fluxes of tundra ecosystems in Lena river delta (Russia). It is known that the large areas of Northern Eurasia near the Arctic cycle are still very poorly investigated in respect of both spatial and temporal variability of GHG exchange and contribution of different plant communities into global atmospheric GHG budget. It makes the results of the study very interesting for scientists working in ecology, biogeochemistry and micrometeorology. The paper is well written. It contains detailed descriptions of experimental site, design of field experiments, developed model algorithms. Discussion chapter includes close examination of obtained results. Before publishing however several points of the paper should be clarified.

We thank the reviewer for the comments, which helped us to improve our manuscript.

1. The chapter 2.3 "flux processing" has not information about procedure or method that has been used for gap filling. The percentage of gaps in flux time series is not quantified.

There was no classic gap-filling that involved filling gaps in a time series of observed values with modelled values. The reason is that we were interested in the flux budgets of our area of interest (i.e. the entire flood plain) rather that the footprint (i.e. only a part of the flood plain). For this purpose, we only used modelled (instead of a mix of observed and modelled) values to estimate the final budgets given in Table 3. Therefore, the percentage of gaps in the time series (roughly 40 %) was of less importance.

2. The LAI ranges of different vegetation classes should be indicated in chapter describing the surface and vegetation structure. Information about surface topography should be also presented.

We did not characterise the vegetation classes with the LAI parameter as LAI values are not available for this site. In fact, we conducted multiple measurements with a LAI-2200C Plant Canopy Analyzer in 2015, but unfortunately the results were not satisfying. The problem was that this kind of measurement is not applicable for such little biomass (sedges, mosses, lichen) as at our tundra site. It was not possible to create a discrete time series of objective measurements over the growing season, as the LAI measurement strongly depended on the position of the device. Consequently, the LAI values largely varied while sampling the vegetation at the same spot.

The topography around the flux tower exhibited a slightly undulating relief ranging from 7.8 m to 10.7 m above sea level. This ancillary information is available in Rößger et al. (2019), which is referenced for further information in section 2.4. The essential information, however, are given directly in the text. Please see "...a sandy ridge aligned in the north-south-axis. The elevated area..." and "...located in depressions around the dry ridge...".

3. Figure 2 illustrates the vegetation map of the flood plain on Samoylov Island and shows the tower location. The tower is situated close to the boundary between bushes and sedges. They have different height and, probably, different density. It can be expected that the air flow disturbances at the boundary between these vegetation types can influence the wind and turbulence patterns at tower location and as a result the measured fluxes taking into account the height of eddy covariance equipment installation (2.8 m).

The vegetation cover of the flood plain on Samoylov is heterogeneous, which served as a starting point for the flux decomposition in this study. As far as the air flow is concerned, our tundra site constitutes – in comparison to other ecosystems or urban sites – a fairly homogeneous site, thereby fulfilling the eddy covariance assumption of a smooth and uniform surface. This statement is based on scrutinising the turbulence with the aid of the integral turbulence characteristics (ITC): during 93 % of the measurements, a well-developed turbulence was present. The remainder accounts for periods, when the wind approached the anemometer (CSAT3) from the back, causing a self-sheltering effect. Consequently, the possible air flow disturbances at the boundary between bushes and sedges could not be registered at the flux tower.

4. The photos of Samoylov Island, that can be found in Internet, show a very nice landscape and, at the same time, a non-uniform surface topography of the study area. Did you estimate the possible effects of non-uniform surface topography on measured fluxes? I guess the possible uncertainties in flux estimation due to complex topography should be discussed in the paper.

We disagree that our site is characterised by a "complex topography" as the flood plain has a slightly undulating relief, shaped by the annual spring flood that has been smoothing the topography for a long time. On the other hand, the flood plain is not fully level and has a vegetation cover with a varying roughness. These aspects impact on the data quality. However, we performed a data quality assessment, which included an ITC test plus a stationarity test, with the result that the assumptions of the eddy covariance theory were fulfilled for the very most part (and the fluxes were thus hardly biased by the topography and the varying surface roughness). What we expect from both the slightly undulating relief and the varying vegetation roughness is an increased uncertainty in the footprint modelling. However, the employed footprint model includes a wind direction-dependent set of roughness lengths, and moreover, it is a widely applied tool within the flux community (see p.11 l.24)

Specific comments

Page 8 line 3 " The mean air temperatures during the measurement periods in 2014 and 2015 ..." I guess the periods of flux measurements have to be indicated in the paragraph... e.g. from June to October 2014 and from June to September 2015.

The sentence states that these mean air temperatures respectively refer to the measurement periods, whose lengths are defined in section 2.2. We think that these information are not important enough to be repeated as, moreover, the measurement periods cover a very similar period of the year anyway.

Page 11 Line 8-10 "While the entire temperature sensitivity of NEE is manifested through changes in TER, the effect of temperature on the biochemical reactions in GPP is neglected (Haraguchi and Yamada, 2011)." I' m not sure that it is a very good assumption for accurate NEE parameterization. It is well known that GPP is strictly depended on temperature and the influence of air temperature changes on GPP rate is actually comparable with effect of temperature changes on TER.

Indeed, in the nature, the air temperature impacts on NEE through affecting both TER and GPP. For estimating NEE with our model, however, the impact of air temperature on GPP is neglected. We have modified the text in order to make clear, that the missing effect of air temperature on GPP concerns our model – not the reality in nature.

Page 11 line 19 ... direct and diffuse solar radiation ...

This change was performed as suggested.

Page 14 line 1 ... seasonal and interannual carbon flux variability ....

For specifying the type of flux in that section headline, we had to write "carbon dioxide flux" instead of the suggested "carbon flux" (in order to exclude methane fluxes). However, the focus of this study clearly is the carbon dioxide flux; so we deem that the reader automatically refers to carbon dioxide flux (variabilities) when reading this headline similar to other headlines including the word "flux" such as "2.3. Flux modelling", "3.2. Dynamics of observed fluxes" or "4.2. Validation of the decomposed fluxes".

Page 14 line 20-22 "However, it is possible that mosses did not fully photosynthesize throughout the growing season due to their tendency to lower their photosynthetic capacity under high irradiance ". What is the reason of such effect? May be it is the result of moss overheating and deficiency of internal water content?

A good explanation for the reason of photoinhibition can be found in Zona et al., 2011:

"Drying and high temperature could decrease moss photosynthesis (Oechel and Sveinbjörnsonn 1978, Murray et al. 1989a). However several experiments showed that mosses are generally not water stressed in wet tundra ecosystems in the high Arctic, as their bases are embedded in a peat that tends to retain water (Oechel and Collins 1976, Hickleton and Oechel 1977, Harley et al. 1989, Murray et al. 1989a). Moreover, elevated temperature generally is less damaging than high irradiance as the temperature optima for photosynthesis of mosses often exceed ambient temperatures in the Arctic and the temperature optima adjust through rapid acclimatization (Oechel 1976, Oechel and Collins 1976, Harley et al. 1989). While bryophytes can adjust to temperature, they cannot acclimate to high light due to the low nitrogen (N) levels in their tissues (Clymo and Hayward 1982, Murray et al. 1993). N deficiency inhibits the protein synthesis necessary to recover from photochemical damage (Ohad et al. 1984, Murray et al. 1993, Huang et al. 2004). In fact, a decrease in photosynthesis due to high irradiance has been observed in mosses even under optimum temperature and fully hydrated conditions (Oechel and Collins 1976, Harley et al. 1989, Murray et al. 1993, Hajek et al. 2009)."

Page 14 line 22-23 What is it, "sun angle"? Do you mean sun elevation?

Yes! The word has been modified.

References

Rößger, N., Wille, C., Veh, G., Boike, J. and Kutzbach, L.: Scaling and balancing methane fluxes in a heterogeneous tundra ecosystem of the Lena River Delta, Agric. For. Meteorol., 266-267, 243-255, https://doi.org/10.1016/j.agrformet.2018.06.026, 2019.

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