Interactive comment on "Net ecosystem exchange and energy fluxes in a West Siberian bog" by Pavel Alekseychik et al.

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General Comments

The manuscript presents energy and CO2 flux data from the West Siberian Taiga. This is valuable data, as the West Siberian Lowland is a vast understudied region. The presented 4-month data set is the first data of what is to become a permanent flux measurement site. Thus is can provide a base line for comparison with other sites and with data that will be collected at the same site in the years ahead.
Generally, the style of the manuscript and the presentation of data is adequate. However, the data analyses lag behind the state of the art and the discussion of the results is often weak. Extensive revisions are necessary before publication of the manuscript.
We express our gratitude for the time invested in deep analysis of our manuscript and for the many useful comments that have resulted from it.

Main Critique

(1) No information on the gap-filling of energy fluxes is given. Was gap-filling not performed for H and LE? Monthly means of these values could be seriously biased if they are calculated based on non-gap-filled time series. Gap-filling should be performed in order to derive sound estimates of mean or cumulative fluxes, and the methods used should be clearly presented in the methods section.
You are correct, the original non-gapfilled energy fluxes were presented in the original MS draft, as we considered that the data series were complete enough (at least in May-July) for the means not to be biased. However, we understand the concern and have done gapfilling of the energy fluxes. Soil heat flux is calculated from gapfilled soil temperature and water level data, therefore, it's gap-free. The other fluxes are gapfilled according to the accepted routines, e.g. Falge et al. (2001). The more in-depth explanation can be found in the revised manuscript (section 2.5). However, the changes after gapfilling, in terms of average or cumulative values (including Bowen ratio) were small.

(2) How have the authors addressed the heterogeneity of soil and hydrological properties and hence ground heat flux (G)? Was soil temperature measured and G calculated for only one microform, hummock or hollow? As G could be expected to vary strongly between hummocks and hollows, a weighted average (based on surface area fractions) of G calculated for both microforms should be used. If G is available for only one microform, an estimate of the error induced by this approach should be added (which could also serve as a justification for this approach).
The originally presented soil heat flus was calculated for hollows, as they are the dominating microform within the EC footprint. To account for this comment in the revision, we calculate G as an area-weighted average for hummocks and hollows (using two replicates of temperature profile in each microform). At the same time, the hydrological differences are tackled by using the ridge and hollow water level measurements in the calculation of the corresponding heat fluxes.

(3) In my opinion, an instationarity test (e.g. Foken and Wichura, 1996) is start of the art and should be applied.

We have applied the instationarity test in the new version of the manuscript and it did not introduce a statistically significant change in the observed fluxes, because it mainly remove very small fluxes (those close to zero). For instance, only a fraction of the negative nocturnal CO_2 flux values were removed by this filter. However, it appears that the new method of Re and GPP model parameter estimation is robust enough so that non-stationarity filtering can be implemented without significant increase in parameter uncertainty. This is done in the revision with a non-stationarity threshold of 1.

(4) No information on the seasonal vegetation development is given in the manuscript. Even if assessments/measurements of GAI or LAI are not available, a general description of the vegetation development is indispensable in order to put the observed flux data in context to the annual cycle of fluxes and drivers.

1)_ Unfortunately, no LAI or snow monitoring has been done during the study period. Therefore, only approximate qualitative estimation of the two parameters can be offered.

Additionally, as the measurements started directly after the end of snow melt, information on the snow/soil conditions immediately before the beginning of the measurement period should be added, if possible (snow height, snow water equivalent, beginning of snow melt, depth of frozen peat layer, beginning/end of peat thaw). This could be very helpful to understand the temporal development of fluxes at the beginning of the growing season.

2) The snowmelt is shown by the steep downward trend in PAR albedo, indicating the presence of patches of snow until about 3 May. However, all profiles, except one in the hollow, indicate freezing at -5 cm until 3rd May, and until about 6th May at -20 cm. Therefore, the snowmelt and peat thaw proceeded only over the first few days of the study. The quantities such as snow pack depth or snow water equivalent are, unfortunately, currently unavailable.

(5) The partitioning of measured net ecosystem exchange (NEE), particularly the modelling of ecosystem respiration (Re) appears to be not sound. In Detail: (a) Why are there significant negative fluxes in the Re vs. peat temperature (Tp) plot (Fig. 3a)?

After careful QA/QC I would ideally expect to see only few and small negative night time CO_2 fluxes (predominantly at lower temperatures). Maybe, the application of an instationarity test could help removing these conspicuous data points? (b) The fit of eq. 6 to the Re vs. Tp data set (Fig. 3a) seems to have a low Rˆ2. I'd like to see the Rˆ2 and p values for this fit. (d) Generally, combining data from the period May-August in one fit of Re vs. Tp is likely to confound the seasonal development of Rref with its temperature dependence. This is reflected in the large temperature sensitivity (Q10 value) obtained by the fit. Fitting Re vs. Tp in a moving window of length 10: : :30 days would be more appropriate. If this would lead to unrealistic variations of the reference respiration (Rref) and Q10, the authors could constrain Q10 to a value around 1.5 (cf. Mahecha et al., 2010). This way, at least the variation of Rref could be assessed, which could give valuable insights into the seasonal vegetation development.

We faced challenges with energy supply at the Mukhrino station. With many cloudy and low-wind periods, frequent blackouts occurred, especially in the nighttime periods. Eventually, this led to the nocturnal data being scarce as it is. The existing nighttime data were only sufficient to construct one general fit of Re vs. Tp (Fig. 3a). Therefore, unfortunately, recalculation of both Re model parameters (Re_ref and Q_{10}) in a moving window does not seem possible. However, a different modeling/partitioning method was used instead. It offers a tradeoff between robustness, precision and the ability to resolve the seasonal course of the parameters, incorporating the following steps:

The updated modeling/gapfilling approach
a) The complete NEE equation, NEE = Rref*Q10^((Tmoss-12)/10) - (Pmax*PAR)/(k+PAR)), is fit to the data at PAR<300 W/m2, in the region where exchange is dominated by respiration. This fit yields Q10=1.99, 95% CI [1.42; 2.57]. This value of Q10 is fixed for the entire May-August period. Regarding the Reviewer's remark on Q10, we would in return suggest that it mainly stems from the choice of the driving temperature, and, as such, does not carry much biological meaning.
b) The Rref, Pmax and k parameters are evaluated in a 30-day wide moving time window.
c) The Rref, Pmax and k series are spline-interpolated to produce the 30-min series, after which the models can be calculated at the original data resolution.
The overview of the gapfilling method is included in the revision (section 2.6).

The superior performance of the described method, compared with its older version, is revealed by a smaller model-measured intercept (see e.g. the figure with the mean diurnal CO2 flux course).

Flux nonstationarity filtering.
Filtering the CO2 flux for high nonstationarity does remove a number of nighttime data in addition to the u* filter; however, the data points so excluded are randomly distributed over the entire nighttime Re range (i.e. both negative and positive values are affected). The negative values in nocturnal Fco2 likely result from high random uncertainty, i.e. they are counterweighted by similarly high positive values. In the revised version of the manuscript, the FST filter is used with the threshold of 1.

To sum it up, we believe that the mean flux-temperature regression, in both its old and new versions, is realistic. This is supported by the fact that the general Re model of Mukhrino does look similar to those found in the other sites. For example, in a similar Siikaneva-2 site (Southern Finnish bog), an ensemble of data from 4 growing seasons showed Rref = 0.8 µmol m^{-2} s^{-1} and Q_{10} = 3.5.

Specific Comments

Line 123: On which micro-form was soil temperature measured (hummock/hollow)?
See (2) in Section "Main Critique" above.
The presented soil temperature and the derived heat flux were measured at a hollow microform. However, in the revision we use a total of four profiles, in hollow and ridge. The area-weighted average ridge-hollow temperature time series are shown in Fig. 4a.

Line 169: For which micro-form was ground heat flux calculated (hummock/hollow)?
See (2) in Section "Main Critique" above.
In the original MS version, at the hollow microform. However, the difference between the updated and old G is not great, in correspondence with the dominance of hollows.

Lines 193-194: Why was only CO2 night time data of August excluded from analysis? It is hard to imagine that only CO2 fluxes are compromised by technical problems of the gas analyzer but not LE fluxes.
The source of the problem affecting the August nocturnal CO2 flux is not known. The Rref parameter increases notably in August, and it is difficult to say if this is a natural dynamic or a technical problem. The objective quality criteria do not remove those data. However, LE seems not

to be affected, as its August nighttime values are close to zero as in the previous months. In any case, we decided to keep the August data, but be tentative in its interpretation.

Lines 205-206: Why was night-time defined as periods with a solar elevation angle below 5_ and not by a PAR threshold (e.g. PAR < 20 _mol/m2/s)? Using a local PAR threshold may allow additional data points to be included into the night time data set (e.g. during cloudy conditions), which could improve the data coverage and hence the modelling of Re.
The night definition was updated as proposed, as the periods with PAR<10 umol m-2 s-1.

Line 206: Have you tried to use peat temperature from other depths or air temperature for the modelling of Re? Information on the performance of the model with other temperatures could give valuable insights into the source of respired CO2.
Yes, we did try to use the other temperatures as drivers of respiration. The hummock temperature measurement had been previously used for that purpose. This is consistent with much higher density of vegetation and low water level in hummocks, which probably makes them major contributors to ecosystem respiration despite representing a smaller area fraction than hollows. However, in consistency with the revised soil heat flux, we are now using the area-weighted soil surface temperature also in Re modeling.

Lines 219-221: In which time steps was the 30-day window moved? Please add this information. Further, the time series of the fit parameters Pmax and k (or the often used alpha = Pmax/k) should be presented. This could deliver valuable information on the seasonal development of the vegetation and could be compared to other studies.
The time window was moved in 1 day steps. We will present the Pmax, k and alpha parameter timeseries in a new figure or subplot. Please also see the requested data in Fig. R1 below.

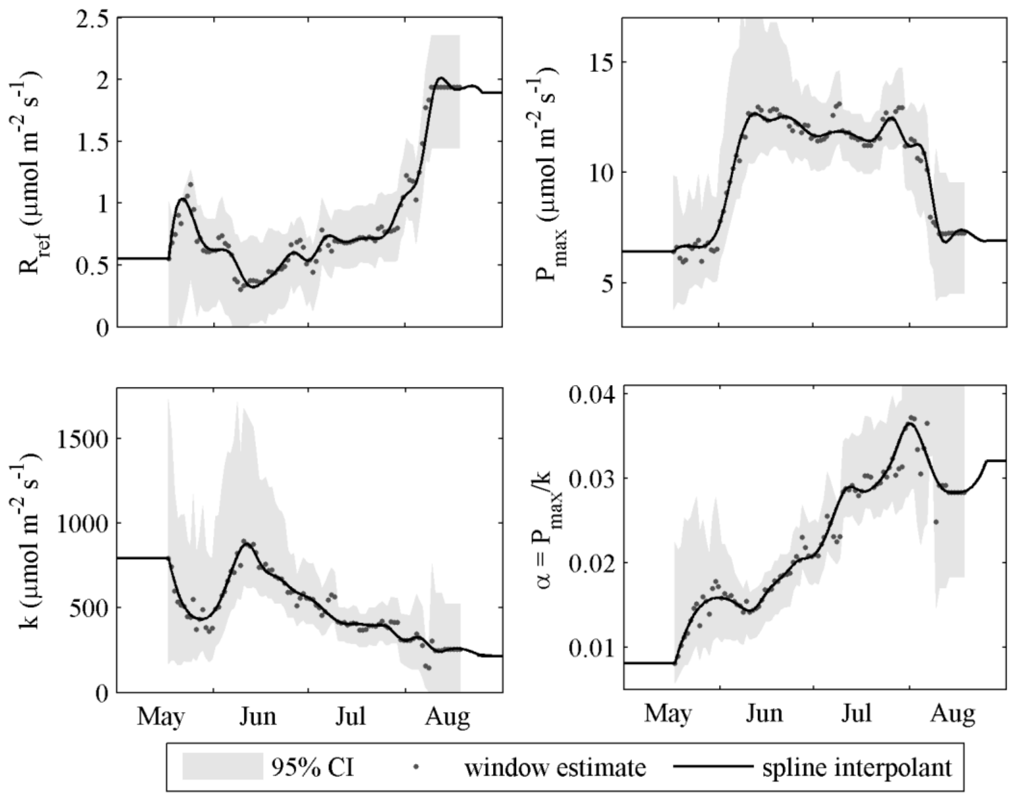

Figure R1. The timeseries of the CO2 flux model parameters. The dots are the daily values estimated in a moving window 30 days wide; the solid lines are the spline interpolants, and the shaded area is the 95% confidence interval (calculated in each time window). This figure will be included in the revised manuscript.

Lines 234-236: Soil temperature at depths 20 cm and 50 cm is discussed here, but this data is neither displayed in Fig. 4a nor used in the analyses. I suggest to either add this data in an additional subplot of Fig. 4 or concentrate in the text on the data already displayed in Fig. 4, i.e. Tp at 5 cm depth.

This mismatch between the text and the plots in Fig.4 is confusing indeed. We will add the 20 and 50 cm temperatures in the subplot (a) of Fig.4.

Lines 258-259: The statement ": : :later on during the summer the water level decreases: : :" contradicts what is shown in Fig. 4e, and is stated in lines 344-345, "The regular and ample precipitation helped sustain water level at a nearly constant level: : :". Hence, the authors' assumption that albedo is reduced due to drying of the vegetation is ill-conceived. Still, it could be checked by simply calculating an albedo from incoming and reflected PAR.

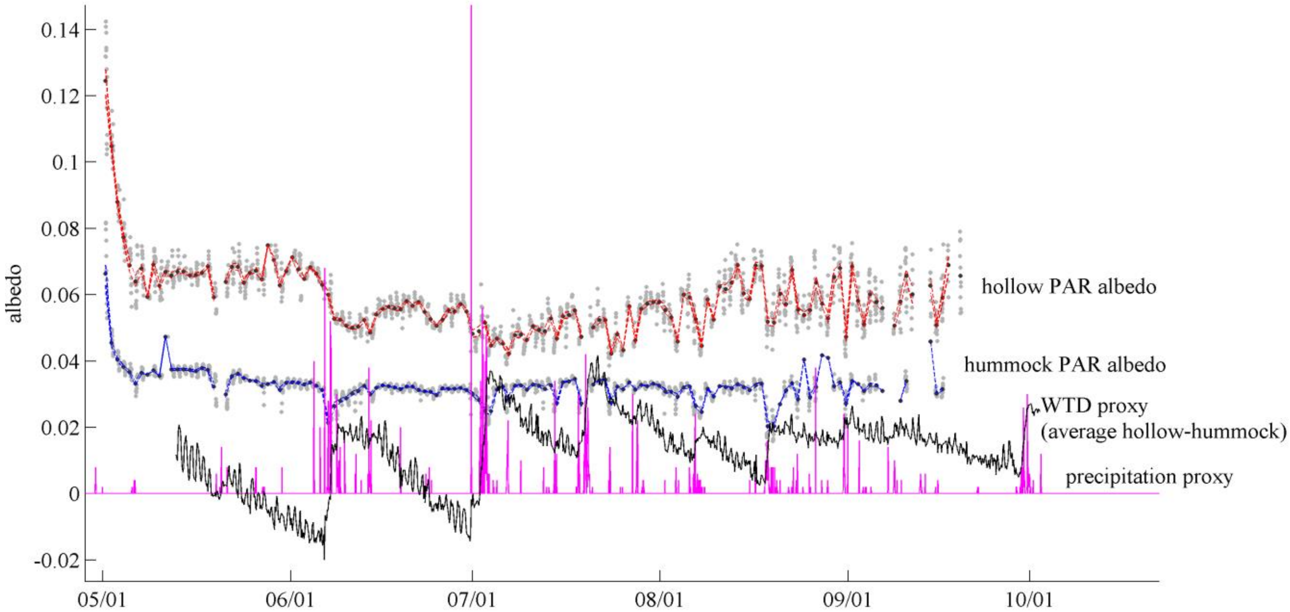

Figure R2. PAR albedo for the hollow and hummock microforms calculated as diurnal medians of the midday (10AM-16PM) periods. The grey dots are the original 30-min albedo averages. WTD (black) and precipitation (purple) proxies are also shown for reference.

Our original expectation was to see an <u>increase</u> in albedo during the dry spells (lines 258-259), which is commonly observed in other peatlands worldwide. However, the year 2015 being unusually wet, the water table did not follow its typical downward trend, nor did dry spells last long. In fact, there were at least seven periods of WTD drawdown and subsequent recovery during heavy rainfalls. On average, WTD had maybe remained constant throughout the growing season. The time-series of albedo are shown in Fig.R2 above. Albedo was rather stable at about 0.06 in hollow and 0.04 in hummock, although small variation correlated with WTD and precipitation can be seen. Similar peatland PAR albedo values were found in other studies (e.g. 5.5% in Frolking et al. 1998). In general, the correlation with rainfall seems to be higher than that with WTD, which is

consistent with the expectation that surface wetness is a stronger controller than WTD. WTD may be decoupled from surface wetness, which is especially probable in hummock, meaning that WTD is probably an inferior predictor of surface wetness and, hence, albedo. In this sequence of frequent rewetting events and drying periods, the surface wetness and albedo shows simultaneous peaks, which is illustrated well by the hummock measurements (Fig. R2).
Of course, the phenology (course of LAI, etc.) in sedge in other vascular species must have affected albedo in a way that is difficult to estimate for the lack of observations. Qualitatively, the dark-colored living vascular plant biomass should lower the ecosystem albedo around the peak of the growing season.
Also, note the steep albedo plunge in early May, indicative of the final snowmelt stage.

Line 302: The spatial heterogeneity does not seem to serve as a good explanation for the low value of the energy balance closure in May, as the surface heterogeneity does not change during the course of the measurement period. Or does it change? How?
The change in the area of open water pools is mentioned as one of the possible sources of heterogeneity in May (line 302), which, in turn, may affect the ground heat flux (line 303). This was the month when the snowmelt ended, which is typically accompanied by scattered open water pools. Their locations were not recorded. We do not know how the surface energy balance measurements could have been affected by those spring conditions. The ground heat flux in hollow was very variable during May, reaching the highest levels for the whole growing season in mid-May with a subsequent reduction to the average annual levels (Fig. R3), suggesting some rapid changes in the thermal properties of the ground.

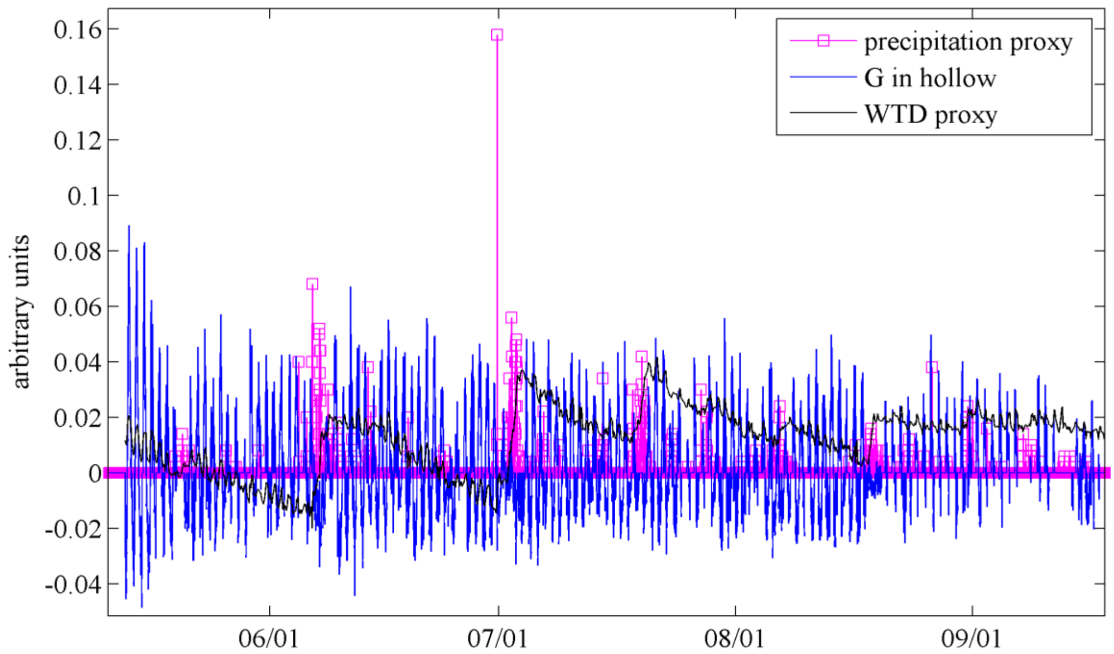

Figure R3. Timeseries of the ground heat flux in hollow, precipitation and WTD. The units are arbitrary.

Line 310 and Fig. 8a caption: The data displayed is surely modelled NEE and not measured NEE?
We apologize for the inconsistency - this is actually gapfilled NEE, i.e. the quality-controlled and u*-filtered original NEE record with the gaps filled by the NEE model.

Lines 311-312: Could the lower amplitude of NEE in May also be due to a not fully developed foliage of the vegetation? Snow melt had only ended a few days before and below-zero temperatures still seem to occur during May. Time series of the parameters Pmax, k, and Rref could help to explain the variations in observed NEE.
You are absolutely right. The ground vascular vegetation (shrubs, sedges) would have only started to recover from the winter and grow their leaf area. While Rref cannot be resolved on a finer timescale for the reasons discussed above. Exactly as suggested, Pmax shows a steep upward trend between May and June, the period of green biomass accumulation. The time-series of the model parameters are now shown in a Figure 3 in the revised manuscript.

Lines 213-314: I see a systematic difference of measured and modelled NEE in Fig. 9. In the afternoon hours of July, measured NEE uptake is smaller than modelled NEE uptake. Hence, either Re is underestimated or GPP is overestimated. What could be the reason for this? Furthermore, why is August night time CO2 flux data displayed if it should have been excluded from analysis due to technical problems (line 194)? In fact, this data does not look completely unrealistic to me. Gažoviˇc et al. (2013) has observed the highest Re during August, while GPP peaked in July. The discrepancies between modelled and measured fluxes could be caused by the fact that Re is poorly modelled by the approach chosen by the authors.
The general observation relating to the new modeling results is that this mismatch is mainly gone. It seems that, just as you have suggested, the model-measured NEE mismatch had been caused by the suboptimal Re modelling method and/or the choice of the driving temperature. In any case, the updated version of the NEE model is probably good enough for the gapfilling purposes.
We also agree (as in the response to an earlier comment) that the August respiration data might be correct. The hypothesis about the technical problems in August has not been confirmed after a cross-check; it was established during revision that no objective filtering or quality control steps could remove the August data. It is also encouraging to hear that some studies, including Gažovic et al. (2013), found similar seasonal trends, thank you for pointing this out. However, data coverage in August is still very low, and the results from that month should be treated with caution.

Lines 352-353 and Fig. 10: Combining all data from the period May-August potentially confounds the seasonal development of Pmax and k, and hence GPPmod, with a possible short term variation of these parameters due to their temperature dependence.
For this approach, only data from the peak vegetation period, i.e. June and July, should be used. Ideally, also the window length for the fit of eq. 7 and determination of parameters Pmax and k should be reduced.
We understand the logic behind this comment. However, as the GPP model used for normalization was calculated from the seasonally changing Pmax and k series, it thus implicitly includes LAI development and other low-frequency seasonal factors, although short-term variability may be lost. We have experimented with different time windows of Pmax and k, and found that at lengths shorter than 1 month, the random variability increasingly dominated the real signal. This was due to the gaps in the original 30-min data, which can only be circumvented by using a sufficiently wide time window. Nevertheless, since May represents the spring recovery period, and August is not covered with data well, using only June and July would improve this analysis. However, for June-July, the picture remains about the same (Fig. R4). Reducing the window length to under 1 month is problematic, due to the scarcity of the nighttime data, but we believe that the current length allows evaluating the seasonal change, at the least.
The observed NEE normalized with the NEE model vs. air temperature for June-July 2015 can now be seen in Fig. 10b of the revised manuscript.

Technical Corrections

Line 66: Use same units as in line 64, i.e. kmˆ2.
Done

Line 72: Is there Permafrost at all at this site, i.e. discontinuous Permafrost? Please clarify.
There is no permafrost of any type in Mukhrino or anywhere else in the region; this is clarified in the revision.

Line 302: Replace "somehow" with "to some extend".
Done

Line 367: "GPP normalized by its model" is ambiguous. Use "(NEE - Rmod)/GPPmod".
Done

References

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