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

Anonymous Referee #3

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The paper by Alekseychik et al. presents the first data collected by an eddy covariance tower near the Mukhrino field station in West-Siberia. Since there are very few eddy covariance towers in this part of the world, I'm confident that the data collected will be of great interest to a wide audience, including modelers that wish to test their simulations. However, the station has not yet been operational for a long time and this study therefore only presents the first four months of data. Unfortunately, this also means that little new scientific knowledge on the processes governing carbon exchange is presented in this paper apart from adding an extra data point on the map (although data from understudied areas is valuable in itself, obviously).
We thank you for you appreciation our work and acknowledging the novelty of the study. We certainly are planning to continue the measurements in as continuous manner as it will be possible.

As the first paper from a new field station, it is important that the description of the data collection is complete and accurate, since it will be the reference paper for future studies from this location. But in order to achieve that, several improvements need to be implemented. Many of such remarks were already made by the other two referees, and I will therefore not delve to long on the areas where our reviews overlap. First of all, it is not clear why the gapfilling and partitioning of the data has not been done by more common methods provided by the Fluxnet community. I suggest to do the partitioning and gapfilling of NEE according to Reichstein et al. (2005). The scripts to do so are freely available on the Fluxnet website. Following common Fluxnet methodology is important to include this new station as a valuable point of reference, and it would be helpful to point out where calculations are similar to previous studies, and where they diverge. Reference to Aubinet al. (2001) or the later book from Aubinet et al. (2012) are useful in that regard.

We understand the concern. The modeling/partitioning method has been updated in the revised manuscript, and the explanation can be found in the response to a comment by Reviewer 1. The new method improves the models by accounting for the seasonal change in the respiration parameter Rref.
However, given such a challenging dataset, the strict application of the Fluxnet methods seems problematic, as those methods function more optimally with more complete datasets. This was acknowledged by Reichstein et al. 2005:
"To sum up, the algorithm introduced here was able to find a short-term temperature response of Reco at all studied sites and is a significant step forward towards less biased estimates of Reco and GEP. Nevertheless, important limitations should be noted: <u>It is not guaranteed to work at all sites since whether one can find a reliable short-term relationship between Reco and temperature depends on the noisiness of the eddy data and the range of temperatures encompassed during the short period</u>. <u>At sites with very stable temperatures and noisy eddy covariance data, it might be possible that within a year no short period can be found where a temperature–Reco relationship can be established at all</u>. Seasonal changes in the temperature sensitivity that have been hypothesized are hard to detect from eddy covariance data, since in many cases not enough shortterm periods with a good correlation between temperature and Reco were found to make up a seasonality."
We have to admit that our data is noisy in exactly this sense, and, in addition, it is full of gaps, especially during the nights.

Nevertheless, we believe that our approach to Re and GPP modeling works towards the same ends as that of Fluxnet, while being at the same time optimized for our dataset.

Also, as pointed out by the other referees, the paper does not present vegetation data and assumes too much about the phenology of the vegetation. If this data is unavailable, that would be a pity, since it would help a lot more to explain the data. Care needs to be taken to acknowledge that data gap, if it exists, and to not over-interpret the data.
Unfortunately, LAI has not yet been determined at the site.

Figure 1d shows that the area within the footprint has quite a bit of variance. The heat fluxes are integrated over this footprint, while soil temperature and net radiation measurements are taken in one point. An energy balance closure of 90% is then very high, given the fact that the different energy fluxes are not measured on the same area. The one place where some wiggle room remains is in calculation of the heat flux, which is highly dependent on the volumetric heat capacity of the soil in equation 4. Yet, soil properties are not mentioned in the paper and simply assumed to be 95% water and 5% peat, according to a reference from 1999. How realistic is this assumption and would your energy balance be worse if it was 80% water and 20% peat, to name just a number? Some uncertainty assessment of the assumptions behind the calculated soil heat flux would be preferable and show how this relates to the energy balance closure.
The 95% porosity in the top 50 cm of peat was originally adopted from Yurova et al. 2007, whose model was used to predict the profile of volumetric water content based on water level, as we lacked direct observations of water level. The Yurova et al. model was parameterized for the porosity values ranging from 92 to 98% between catotelm and acrotelm, so we assumed that 95% would represent the mean conditions well enough. However, at the time we were preparing this manuscript, the new results on the physical properties of peat at Mukhrino were not yet available. Szajdak et al. (2016) summarized their measurements in six representative micro-landscapes around the site and found an average porosity in the top 50 cm of peat to be 93%. Therefore, our earlier assumption of 95% porosity in surface peat layer was realistic. To be consistent with Szajdak et al. (2016), we will update the porosity value.
The other reviewers suggested to recalculate soil heat flux as an area-weighted average of the fluxes calculated for hummocks and hollows, which we do in the revised MS version. As Szajdak et al. (2016) do not present microform-specific results, we will be forced to use the same 93% porosity for both microform types.

Page 2, line 61: It would be good to include a more precise location of the tower, rather than these rounded coordinates, for future reference and model work.
Will be done. The precise coordinates of the EC tower are given on the line 126 in the revised MS.

Page 4, line 115: please mention the exact dates here also, and not only later in the document.
Done.

Page 6, line 169: is G calculated from the soil temperature measurements at 2, 5, 10, 20 and 50 cm depth? Please specify.
It was calculated from the temperature measurements at 5, 10, 20 and 50 cm depths. This will be specified. The 2 cm level was omitted because there was no certainty that it shows the peat (moss) but not air temperature, i.e. that the sensor was constantly in a good contact with the vegetative parts or soil.

Page 6, line 179: how well would this equation work for this site? Seems to me that volumetric water content would vary a lot between ridges and hollows.

The Yurova et al. 2007 model was constructed for a Swedish fen, which closely resembles the lawn microsites of Mukhrino. Lawns-hollows being the dominating microform in the areal sense, the equation probably describes the average water content over most of the area with satisfactory precision. In the revision, the hollow and ridge WTD data are used separately to model water content profile for these microsite types.

Page 7, line 206: as mentioned by the others, why not simply look at measured PAR as a threshold? Was the sensor shaded by trees?

The CO2 flux distributions at the two night definitions are more or less identical (Fig. R1). These data are u*-filtered. As with the of the open-path sensors in general, our nighttime flux is characterized with high random uncertainty. Both definitions yield realistic mean nocturnal CO2 flux of about +1 µmol m-2 s-1, have similar shapes and kurtoses. However, the definition based on solar angle leads to more nocturnal periods than the PAR definition (610 versus 455).

In any case, we are using the PAR definition of nighttime periods in the revision, as the new CO2 flux model is not as sensitive to the amount of nighttime data, as its previous version.

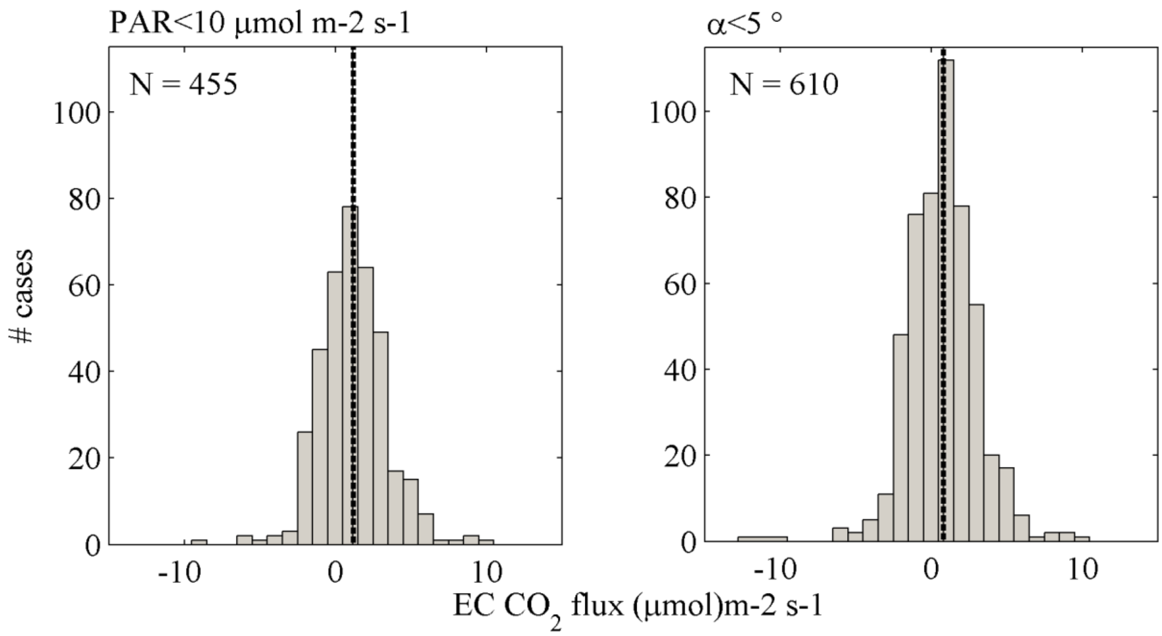

Fig. R1. Histograms of eddy-covariance CO2 flux for the two alternative night definitions, based on PAR (left panel) and solar angle (right panel).

Page 7, line 210: fitting this equation on all data at once leads to a very uncertain fit, as is clear from Figure 3a, due to temporal variation in the base parameters. The method by Reichstein et al. (2005) therefore shifts short optimization windows throughout the season. Something similar should've been applied here, since Q10 is probably not stable and depends on changes in e.g. soil moisture and substrate availability.

As discussed in the response to the previous comment, the number of nocturnal data is very low, and this prompted us to use that simplified approach. Representation of the seasonal course in Q10 seems challenging for this dataset, however. However, we found a way to circumvent the problem by representing the seasonality in the Rref parameter. This is done in a way similar to the Reichstein et al. 2005, although not down to fine detail. The limitation of the data coverage has played a major role in the choice of the gapfilling method, however we hope that we reach similar targets as Reichstein et al. 2005.

Page 7, line 216: same as previous remark. Why fit this to the entire dataset when the phenology of the plants, and therefore base parameters, is changing throughout the summer? There are better partitioning methods out there.
We must have described the gapfilling scheme in a confusing way, apologies for this. In fact, the other reviewers were similarly puzzled about this point. Fig. 3 shows a fit to all data together just for demonstration. What actually was done, is the estimation of the GPP model parameters (Pmax and k) in a 1 month-wide moving time window moved in 1-day steps. Smoothened timeseries of Pmax and k were then obtained from these daily estimates. So, our approach does capture the vegetation phenology and other seasonal effects. Please see Fig. R1, which was also included in our response to Review 1. The updated CO2 flux gapfilling method is described in the revised MS section 2.6.

Page 8, line 231: ‘dramatic’ is a subjective term. Perhaps this is normal in this area?
We wished to underline the dramatic nature of snowmelt in the year 2015, as it really was beyond the normal – early and rapid. In the revised manuscript, we rephrase it as “an unusually early and rapid snowmelt in April and the beginning of May”.

Page 10, line 259: ‘probably’? how would you know if you haven’t measured this? Isn’t the lower amount of incoming PAR the reason that Rn is also lower?
At the time of this response’ writing, we can confirm this suggestion with data. We do observe a summer minimum in PAR albedo in early July (about 4%). After that, PAR albedo climbs to 6-7% by mid-August. However, while the short-term variations in PAR albedo are correlated with the heavy rain periods, the absence of strong seasonal trend in WTD does not allow to view it as a driver of PAR albedo in that year. Instead, the late summer increase in albedo must have been due to the senescence of vascular vegetation. Although albedo for global radiation was not measured, one could surmise that it followed similar trends.
Finally, Rn reduction solely due to change in albedo was meant here (line 259). Of course, this occurs in parallel with the downward trend in insolation after the summer solstice.

Page 10, line 260: incoming solar radiation is logically lower in August, since it’s further removed from midsummer. So this would also happen if there was no difference in cloud cover.
We fully agree. As in the previous point, the individual effect of cloudiness was meant, as an addition to the astronomical component in solar radiation variation. We are sorry that this was not stated clearly, and it will be rephrased.

Page 14, line 345-347: This sentence is unclear. Are you talking about this in general terms or are you referring to this site?
If we understand this comment correctly, yes, we are discussing the effects of the fronts in terms of the *in situ* observations. The water table dynamics discussion in Price (2003) is of general relevance, and only used as a theoretical framework for the particular case of Mukhrino.

Page 15, line 364: The landscape in your footprint doesn’t look homogenous at all, with all the variation between ridges and hollows. It’s just that this variation is similar within different areas of your footprint, but that’s not the same as homogeneity.
We agree with this note. This landscape is homogeneous on the length scales of 100 m and more, but it could be incorrect to call it homogeneous in a common sense (as in the Degerö which is devoid of any small or large scale features, e.g. Peichl et al. 2013).

Figure 10: how was does normalizing done? Please explain.

The observed flux was simply divided by the model. We will make this more specific in the text.

Page 16, line 384: Are these observations really in these IPCC reports? Surely, there's a peat synthesis product out there that can be cited instead.
It actually the "Supplement to the 2006 IPCC Guidelines for National Greenhouse Gas Inventories: Wetlands" we are referring to. IPCC themselves suggest referring to it as "IPCC 2013, 2014".

Quoting the IPCC website (http://www.ipcc-nggip.iges.or.jp/public/wetlands/):
"Please cite as: IPCC 2014, 2013 Supplement to the 2006 IPCC Guidelines for National Greenhouse Gas Inventories: Wetlands, Hiraishi, T., Krug, T., Tanabe, K., Srivastava, N., Baasansuren, J., Fukuda, M. and Troxler, T.G. (eds). Published: IPCC, Switzerland"

Page 16, line 388: You cannot say 'apparently' since you are not reporting the course of vascular plant leaf area development.
LAI was not measured and we can only surmise about its effects. However, the Pmax course (Fig. R2) should approximately correspond to the LAI seasonal curve. But we agree that using "possibly" or "likely" would suit this sentence more than "apparently".

References

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