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

Anonymous Referee #2

Received and published: 3 April 2017

The manuscript presents net ecosystem and energy fluxes from a West Siberian bog measured by eddy covariance technique. This manuscript provides important data from a remote and large but also understudied region which is characterized by high coverage of peatlands. Unfortunately, the data presented here is just for 4 summer months but as it was measured at an established field station more data will surely follow in the future. The topic of the manuscript is well within the scope of the journal
and the manuscript meets well the basic scientific quality. However, extensive revisions are necessary before publication of manuscript.
Dear reviewer, we express our gratitude for your detailed analysis of the current manuscript. We will try to address all the comments.

Main points of critique

1. It would be important to include some information on vegetation development over the investigated period to better understand the dynamic of the CO2 fluxes.
Unfortunately, the vegetation parameters (such as LAI, biomass or phenology) were not directly assessed during the course of the measurements. Therefore , we can only offer qualitative estimates.

2. The gap filling procedures should be described in more detail. It should be clarified in the text and in the figures if modelled or measured data has been used and also discussed how the gap filling method might influence the modelled data. If gap filling was not applied it should be discussed which influence it would have on the results.
This is correct, the CO2 flux data series have been gapfilled already in the original MS, and both the gapfilled and non-gapfilled series were displayed throughout the manuscript, depending on the aims of the individual sections. We will specify this more clearly in the text and explain the gapfilling procedure in more detail. The modeling/gapfilling method has also been revised; please see the detailed explanation in the answer to a comment by Reviewer 1. Please see the revised MS sections 2.5 and 2.6 for the description of the updated gapfilling procedures for the energy and CO2 fluxes.

3. The discussion is weak, maybe that was the reason why results and discussion were merged in one chapter. This chapter includes mainly comparison to other studies and less discussion of the influencing factors which determine the dynamics of CO2 and heat fluxes.
We understand this comment and will try to improve the discussion. We will revise the discussion of the environmental driver effects as much as it is possible with the available dataset, as much as the available data permits.

Specific comments:

L 38: Please include some examples for measurements in Europe and in Siberia.
We will mention the relevant examples of bog measurements in the region. However, very few other peatland studies have been conducted in the region – this not only including West-Siberian middle taiga, but in Siberia as a whole. The number of relevant studies becomes even more limited

if one considers only raised bog ecosystems. The revised MS lines 39-44 now list the comparable sites having eddy-covariance setups.

L 55 Please change to flux tower data
Thank you for noticing this slip of the pen, this will be corrected.

L 114 I usually include the measurement section into the methods section
It will maybe make more sense indeed to combine the 'Materials' and 'Methods' sections; this is done in the revised version.

L128-130 You do not present winter fluxes here, so you can skip that paragraph or when does the winter start?
This is in fact the first description of the measurement site and its infrastructures that has not appeared in any prior English language publications. We therefore tried to include all the information that could potentially be of interest. In fact, in 2015 the snowmelt ended in the very beginning of May, as suggested by the PAR albedo timeseries (Fig. R2 in the response to Reviewer 1).

L205 Why do you use the solar elevation angle and not the widely used PAR<10 _mol/m2s threshold to define the night-time?
The solar elevation angle criterion has also been widely used in our community. A possible issue with using the PAR threshold is that PAR is measured at about 2m height, while the eddy-covariance sensors are installed at 4m height. Furthermore, Fig. A1 suggests that there is a great degree of overlap between the two night definitions, as far as the mean CO_2 flux is concerned. As one can see, on average, the CO2 flux transits zero somewhere near +5 degrees threshold that we have defined. At the same time, when the PAR threshold of 10 umol m-2 s-1 is used, an equivalent number of negative CO2 data are marked as "night". So, we suggest that the two methods of night period definition produce approximately equivalent results. Nevertheless, we are using the PAR definition in the revised manuscript.

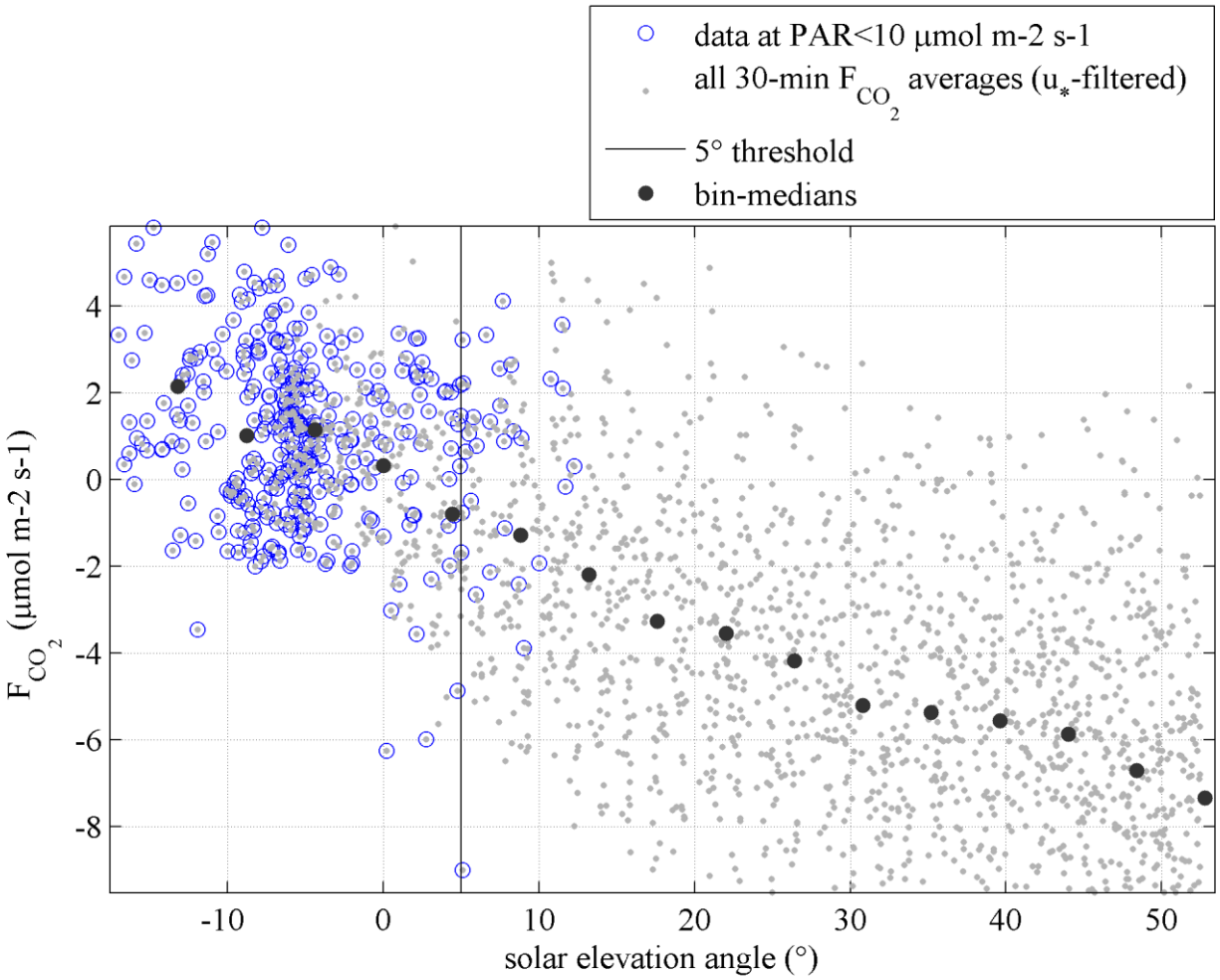

Fig.A1. EC CO_2 flux versus solar elevation angle.

L 206 Did you try to use other soil temperatures than at 5 cm depth to model the respiration? Please include R2 to the Figure 3.

Yes, we did. It was difficult to find any improvement when the other temperature measurements were tried. In the revision, we are using the area-weighted average of the ridge and hollow -5cm temperatures, so that to account for the spatial variation.

The original Figure 3 proved to be confusing and gave a wrong idea that the displayed "general" fits were used to model Re and GPP for the whole period, therefore, it was replaced with the parameter timeseries figure. We did not use the general regression of peat temperature to Re anymore, as described in the description of the revised gapfilling method.

In any case, the requested R2 of the Tp-Re fit in former Figure 3a was 0.55.

L234-236 You use just the soil temperature at 5 cm depth for modelling, please include this information to the text and skip the information on soil temperatures at other depths.

We would maybe argue that the 20 and 50 cm depths be kept, once again for the reason that it has not been published anywhere, while it might interest some as background. The 20 and 50cm depths are added to Fig. 4a in the revision.

L 310-311 So the range of the values in the Fig 8a show just values from +3 to -9 mol/m2s.

This is correct. We did zoom into the main data cluster so that to make the seasonal dynamics clearer. The extreme 30min averages in fact do not need to be mentioned, as they represent the typical random variability of the eddy-covariance fluxes rather than the typical exchange, which is bounded by about +5 and -10 umol/m2s.

L 312 The vegetation might play an important role as well.
You are absolutely right, we should mention this biological driver along with the abiotic ones, temperature and PAR. However, the LAI or other vegetation parameters have not been directly measured at the site.

L 318 Please include the gapfilling methods for the NEE fluxes.
We agree on the importance of this information; we hope that the revised section 2.6 provides sufficient detail on the gapfilling method.

L 329 What was the range of daily fluxes in Mukhrino?
For June-August, the average daily cumulative NEE was -1.8 g C m-2, so, well within the values observed elsewhere. This estimate is included in the revised text for a clearer comparison with the literature.

L 351 It might be interesting to include a figure with a typical diurnal course before and during the passage of the weather front.
It should be possible to add show a close-up of the daily course as an extra panel to Fig.8. This addition is made in the revised MS. The third front passage period having the least proportion of gaps is displayed as a case study.