

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

Anonymous Referee #3

Received and published: 5 April 2017

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).

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 et al. (2001) or the later book from Aubinet et al. (2012) are useful in that regard.

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.

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.

Some detailed comments:



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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.

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

Page 6, line 169: is G calculated from the soil temperature measurements at 2, 5, 10, 20 and 50 cm depth? Please specify.

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.

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?

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 Q_{10} is probably not stable and depends on changes in e.g. soil moisture and substrate availability.

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.

Page 8, line 231: 'dramatic' is a subjective term. Perhaps this is normal in this area?

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 R_n is also lower?

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.

Page 14, line 345-347: This sentence is unclear. Are you talking about this in general

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terms or are you referring to this site?

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.

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

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.

Page 16, line 388: You cannot say 'apparently' since you are not reporting the course of vascular plant leaf area development.

References

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