Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2017-43-RC1, 2017

Atmospheric Chemistry and Physics Discussions

Open Access

ACPD

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

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Received and published: 13 March 2017

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.

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Discussion paper

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.

(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).

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

(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. 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.

(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 CO2 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.

Specific Comments

Line 123: On which micro-form was soil temperature measured (hummock/hollow)? See (2) in Section "Main Critique" above.

Line 169: For which micro-form was ground heat flux calculated (hummock/hollow)? See (2) in Section "Main Critique" above.

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.

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 μmol/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.

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.

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.

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.

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.

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?

Line 310 and Fig. 8a caption: The data displayed is surely modelled NEE and not measured NEE?

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.

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. Gažovič 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.

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.

Technical Corrections

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

Line 72: Is there Permafrost at all at this site, i.e. discontinuous Permafrost? Please clarify.

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

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

References

Foken T, Wichura B (1996): Tools for quality assessment of surface-based flux measurements. Agricultural and Forest Meteorology, 78, pp. 83-105. DOI:10.1016/0168-1923(95)02248-1.

Gažovič et al. (2013): Hydrology-driven ecosystem respiration determines the carbon balance of a boreal peatland. Science of the Total Environment, 463-464, pp. 675–682.

Mahecha et al. (2010): Global Convergence in the Temperature Sensitivity of Respiration at Ecosystem Level. Science, Vol. 329, Issue 5993, pp. 838-840. DOI: 10.1126/science.1189587.

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