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Interactive comment on "Spatial variability of CO_2 uptake in polygonal tundra – large overestimations by the conventional eddy covariance method" *by* Norbert Pirk et al.

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The manuscript of Pirk et al. presents interesting analyses of the spatial variability of topography and land-atmosphere fluxes of CO2 within a high-arctic polygonal tundra.

The small-scale spatial variability of topography was analyzed by photogrammetry of aerial photographs, which was used to produce a visual map and a digital elevation model. For an assessment of geomorphological changes of the polygonal tundra in the last decades, the new map was compared with historical aerial photographs. The study shows that no such geomorphological changes due to permafrost degradation could be detected at the high-arctic study site on Svalbard although the mean annual air temperatures on Svalbard have strongly increased in the last decades. This interesting

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Discussion paper

result suggests a rather strong resilience of polygonal tundra to climate warming.

The small-scale spatial variability of land-atmosphere fluxes of CO2 was analyzed by separating the flux time series in periods with either wind directions from a drier landscape sector or in periods with wind directions from a wetter landscape sector, and separately analyzing the respective flux controls and flux balances for the two different sectors. The conclusion of this part of the study is also scientifically interesting and relevant as it indicates that drying of polygonal tundra, which might happen in many polygonal tundra areas due to permafrost degradation, will lead to a decrease of the CO2 sink capacity of these tundra landscapes.

Furthermore, the authors aimed at a better understanding of "how the spatial heterogeneity and larger-scale disturbances affect eddy covariance flux estimates by investigating the spectral composition of the eddy covariance signal". For this objective, they apply the ogive optimization method, which was only recently introduced by Sievers et al. (2015). Generally, I find the application of this new method and its comparison to the conventional eddy covariance method presented by this manuscript highly valuable and of great relevance for the eddy covariance flux community. However, I think that the study does not provide enough evidence and deep-enough discussion to substantiate their claim that the ogive optimization method produces more trustworthy results than the conventional method. I discuss this in more depth in the list of specific comments below.

The language of the manuscript is clear and easy to follow. The figures are of high quality.

I recommend the manuscript of Pirk et al. for publication in Biogeosciences after major revisions considering my comments above and below.

Specific comments:

(1) Page 1: Title: I suggest weakening the rather strong and general statement in the

second part of the title: “large overestimations by the conventional eddy covariance method”. I think that it is not clear enough at this point, which of the two methods – the conventional or the ogive optimization – delivers more trustworthy results. It is definitely an important finding of this study that the two methods lead to such strongly deviating results, but for a decision which method should be preferred, a better understanding of the atmospheric flow or experimental set-up effects potentially causing these biases would be needed. Furthermore, if the title suggests that the main message of the article is that the conventional eddy covariance method overestimates the CO2 uptake, the existing theoretical knowledge about eddy covariance measurements over heterogeneous landscapes and complex terrain must be more extensively reflected both in the introduction and the discussion. If the main message of the article is on the biases of the eddy covariance method, it is not enough to just refer to the work of Sievers et al. (2015). Then, the authors have to discuss their findings in the light of the extensive work on eddy covariance measurements over heterogeneous landscapes and in complex terrain (e.g., Mahrt et al. (1994), Finnigan et al. (2003), Inagaki et al. (2006), Aubinet et al. (2010), and others) in the current manuscript.

(2) Page 2, lines 21-22: The paper of Kutzbach et al. (2007) reports an annual net ecosystem CO2 exchange (NEE) of -71 g CO2 m-2, which equals to about 19 g C m-2, for polygonal tundra (Kutzbach et al., 2007). Please correct this.

(3) Page 3, lines 7-9: I think that it would be important to more thoroughly describe and discuss the patterns of prevailing wind directions and the microclimatic situation in general. The investigation site is located in a valley surrounded by rather high mountains, and it is near to the sea (fjord). Therefore, sea and land breezes, katabatic or anabatic winds as well as gravity waves may have important effects on the air movements analyzed by the eddy covariance system. This could be relevant for the discussion of the observed frequency mismatches in the co-spectra and ogives, respectively.

(4) Page 3, lines 12-14: Please give here more information on the soil properties in this polygonal tundra. In particular, organic carbon contents in the different soils of

polygonal tundra would be of interest. Spatial variability of soil organic matter contents is likely pronounced in the polygonal tundra (Zubrzycki et al., 2013).

(5) Page 3, lines 14-16: Please give more detailed information about the vegetation composition within the polygonal tundra. How does vegetation differ between low-center polygons of different degradation/drainage conditions? Please give information on (approximate) ground coverages of shrubs, sedges and mosses at polygon rims and polygon centers of different water levels. The coverage of mosses is of high interest since they can start photosynthesizing directly after snowmelt (or even earlier) (Oechel , 1976, Tieszen et al., 1980). When discussion the early CO2 sink function suggested by the conventional eddy covariance method, coverage of mosses is of interest.

(6) Page 3, line 23: This sentence is confusing. You need the pressure and temperature inside the cell to convert from molar densities to mixing ratios. You need water vapor measurements to convert from mole fractions (referred to wet air) to mixing ratios referred to dry air. Please write this in a clearer way.

(7) Page 6, lines 4ff: How did the footprint extents differ before, during and after snowmelt? The snow cover could have a significant effect on footprint extents due to its lower roughness length. Could this affect the flux co-spectra during snowmelt?

(8) Page 6, lines 13ff: When considering the pronounced spatial variability within the footprints of the eddy covariance measurements, I wonder how much you can be sure that the frequency mismatches are due to local and non-local flux contributions. Could this mismatch also be caused by flux heterogeneity within the (local) footprint? The position of the flux tower appears to be at a drier patch compared to the surroundings in the studied polygonal tundra. When moving from the tower in both main prevailing wind directions, the first wet polygons are found some 30 m to 50 m away from the tower. Could it be possible that the observed frequency mismatches (commonly sign of covariance different for eddies larger than about 30 m than for eddies smaller than 30 m) are due to positive CO2 fluxes from the drier polygons near the tower (reflected better

in the high frequencies) and negative CO_2 fluxes at the wetter tundra at larger distance from the tower (reflected in the low frequencies)? If wetter tundra has more mosses, this could lead to earlier negative fluxes than at drier sites with less mosses since mosses can start photosynthesizing directly after snowmelt (or even earlier) (Oechel , 1976, Tieszen et al., 1980). Since the strongest frequency mismatches were observed during the snowmelt period, it would be also very interesting to have more information on the snow distribution: Was there the same snow coverage near the flux tower in the drier polygons than further away (30-50 m) in the wetter polygons?

(9) Page 6, line 30: What do you mean with “better performance”? How did you assess “performance”? I think that CO_2 uptake during the snowmelt period (as it is illustrated in in Figure 2b) would not be as implausible as suggested by the authors since mosses can start photosynthesizing directly after snowmelt (or even earlier, see above).

(10) Page 7, line 6: What is exactly meant by “combined footprint”? Just using the original eddy covariance flux time series without separating periods of different wind directions? Or have you applied some sort of spatial weighing of the contributions of wetter and drier polygonal tundra to the whole are of interest? If you do the former, then the CO_2 balances for the “combined footprint” would depend to a large degree on the frequency distribution of wind directions.

(11) Page 7, lines 9-10; Page 8, lines 1-2: It does not become clear why you can calculate eddy covariance fluxes without having mixing ratios referred to dry air by using the conventional EddyPro method but not by using the ogive optimization method. Couldn’t you apply the classic WPL approach (Webb et al. (1980) as refined by Ibrom et al. (2007)) to fluxes calculated by both methods?

(12) Page 12, lines 7-8: The observed annual CO_2 uptake appears indeed very large. However, I think that such an uptake is well possible. For example, high CO_2 uptake was also observed at coastal wet sedge tundra near Barrow, Alaska, by Harazono et al. (2003).

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