Comment: *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 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.

Reply: We thank Prof. Lars Kutzbach for his thorough review and his comments to further improve our manuscript.

<u>Specific comments:</u>

Comment: *(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.*

Reply: OK, we propose to weaken the statement in the second part of the title to take some weight off the eddy covariance calculations. So the revised title would be: "Spatial variability of CO2 uptake in polygonal tundra – assessing low-frequency disturbances in eddy covariance flux estimates"

We furthermore propose to extend the introduction including the mentioned literature by adding the sentences: "Large-scale surface heterogeneity has been observed and simulated to induce thermal circulations on the mesoscale that can impede the turbulent flux estimation (Mahrt et al., 1994; Inagaki et al., 2006), while complex terrain may lead to horizontal advection of gases and thereby biased flux estimations (Finnigan et al., 2003; Aubinet et al., 2010). Finnigan et al. (2003) showed that the averaging operation and coordinate rotation commonly applied in EC flux calculations can lead to co-spectral distortions and a loss of flux."

In our discussion, we propose to make specific mention of the CO2 co-spectra given in Figures 15 and 16 of Finnigan et al. (2003), which show an indication of a similar frequency mismatch below 10^-3 Hz:

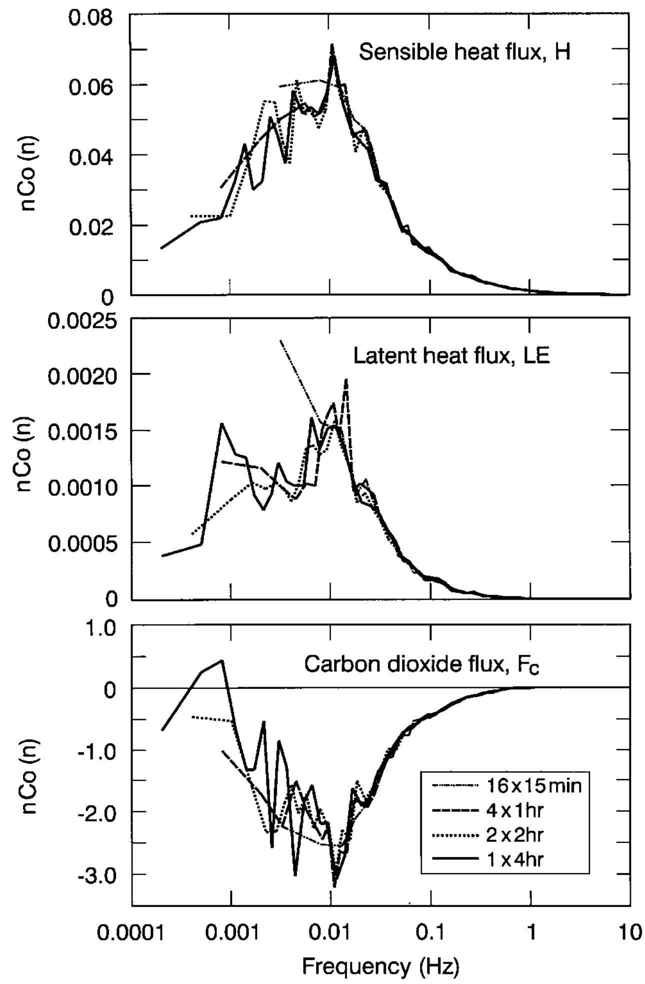

Figure 15. Ensemble averaged scalar cospectra for the period 0800–1200 EST from 9 days of Tumbarumba data. The cospectra are plotted in area preserving form. Each plot consists of four curves corresponding to averaging and rotation periods of 15 min, 1 h, 2 h and 4 h. The curves extend to successively lower frequency as the averaging period increases. (a) Sensible heat, H. (b) Latent heat, *LE* from the closed-path Licor instrument. No high frequency correction has been applied. (c) Carbon dioxide flux, F_C from the closed-path Licor instrument. No high frequency correction has been applied.

The fact that the low frequency shift is here only observed for CO2 may have something to do with the low covariance of CO2 observations, relative to sensible and latent heat fluxes.

Comment: *(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.*

Reply: OK, we corrected this.

Comment: *(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.*

Reply: OK, we propose to extend the site description with the following sentences: "The surrounding mountains feature plateaus of around 450 m a.s.l., as well as peaks and ridges of up to 1000 m a.s.l, which are still partly glaciated. Wind directions are generally oriented along the valley, with dominating easterlies in wintertime (coming from inland Spitsbergen), and an approximately even distribution of easterlies and westerlies in summertime (westerlies coming from the fjord). Long-term statistics indicate that wind speeds in Adventdalen are below 5 m s-1 for about 70% of the year (and below 10 m s-1 for about 97%), with a most frequent wind speed of about 3 m s-1."

Comment: *(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).*

Reply: OK, we propose to also extend this part of the site description with what has been reported by other studies: "The measurement site is located on a river terrace on the flat part of a large alluvial fan, where the ground is patterned by ice-wedge polygons. These coarse alluvial deposits are covered with a few ten centimeters of organic material and fine-grained eolian deposits (loess), which typically stem from wind erosion in the braided riverbed when it dries out in autumn (Bryant et al. (1982), Oliva et al. (2014)). The site's soil organic carbon content in the uppermost 100 cm soil is about 30 kgC m-2 (personal communication with Peter Kuhry)."

Comment: *(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.*

Reply: Due to the patterned microtopography, there is considerable variability in the vegetation cover. A dedicated vegetation analysis has not been conducted, so we can unfortunately not estimate the overall moss cover. Our general assessment is that the three vegetation layers (shrubs, sedges, mosses) clearly overlap in most areas. Drier areas are dominated by shrubs and sedges, while wetter areas are dominated by sedges and mosses. So we can assume sufficient

moss coverage for the discussion of the early onset of net CO2 uptake indicated by the conventional method (details given in reply to comment 8 below).
To give some more information about the specifically relevant moss cover, we propose to add these sentences to the site description: "The moss cover is sparse in drier polygons where shrubs dominate the vegetation community, while the wetter areas at local depressions feature an almost continuous moss cover. Within individual polygons the moss coverage typically increases from the drier rim to the wetter center."

Comment: *(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.*

Reply: We acknowledge that this sentence needs clarification (as also pointed out in comment 5 by reviewer #1). We propose to change it to: "CO2 concentrations collected in 2013 were only recorded as molar densities and without the cell pressure necessary for a sample-by-sample conversion to mixing ratios according to the Webb-Pearman-Leuning correction proposed by Sahlee et al. (2008), which is currently the only option implemented in the ogive optimization software. Hence, we only report 2013's fluxes from EddyPro as supplementary support for our findings"

Comment: *(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?*

Reply: In our footprint estimation we kept the roughness length constant at 1 cm throughout the year. This value might be slightly too high for snow (which is typically assigned 0.5 cm) and maybe slightly too low for open tundra vegetation (typically assigned 3 cm). We have however not undertaken dedicated efforts to quantify this parameter more precisely. Therefore, we might overestimate the footprint extent a little bit during the snow-free season, and underestimate it a little during snow-covered conditions.
The surface roughness could indeed affect the flux co-spectra -- in principle at any time of year. One conceivable mechanism is that a greater roughness length could break down larger turbulence into smaller turbulence, thus shifting some of the co-spectrum toward higher frequencies. We propose to add this consideration to our discussion, stating: "Snowmelt also entails a change in the typical surface roughness length, which is slightly smaller for snow than open tundra vegetation. A greater roughness could break down larger turbulence into smaller turbulence, thus shifting some of the flux co-spectrum toward higher frequencies. However, such spectral shifts would be no problem for the functionality of the used flux calculation schemes and cannot readily explain bi-directional fluxes even if the surface roughness is spatially heterogeneous."

Comment: *(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 CO2 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?*

Reply: When we first noticed the systematic frequency mismatches during snowmelt we were thinking along exactly the same lines as Prof. Kutzbach describes here, namely that different frequencies represent different areas, of which some are CO2 sources and others CO2 sinks. However, due to the comparably large scale at which is mismatch occurs (around 25 sec, corresponding to more than 30 meter), we largely discarded this explanation again. We don't have a detailed vegetation map or a good estimation of the snow coverage throughout the snowmelt period, but we generally estimate the patchiness during snowmelt to be smaller than the size of the eddies corresponding to the lower frequencies.
Moreover, we observed the frequency mismatches from both wind directions, and since there are likely less mosses in the east than the west, the moss mechanism for this mismatch becomes even less likely.
However, we cannot fully exclude the mechanism outlined by Prof. Kutzbach, so we propose to add the following sentences to our discussion: "However, we cannot fully exclude that the frequency mismatches are caused by flux heterogeneity within the local footprint. It could be possible that the drier areas near the EC tower (reflected better in the high frequencies) are net CO2 sources, while wetter areas at larger distances from the tower (reflected in the low frequencies) are net CO2 sinks. A heterogeneous vegetation composition might cause such flux heterogeneity during snowmelt, because unlike shrubs and sedges, mosses have photosynthetically active tissue that may overwinter so that they can start photosynthesizing at low rates already during snowmelt (Oechel et al. (1976), Tieszen et al. (1980)). While some degree flux heterogeneity is certainly present at any time of year, its effect might be too small to explain the large frequency mismatches observed particularly during snowmelt. Nevertheless, our observations might incentivize future studies to investigate the frequency dependency of EC footprints."

Comment: *(9) Page 6, line 30: What do you mean with "better performance"? How did you assess "performance"? I think that CO2 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).*

Reply: We acknowledge that "better performance" is not the best formulation here and propose to change this sentence to: "Since the frequency mismatches cannot be resolved in conventional calculations, we focus on the NEE fluxes calculated by the ogive optimization method for the ecosystem characterization."

Comment: *(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 CO2 balances for the "combined footprint" would depend to a large degree on the frequency distribution of wind directions.*

Reply: Yes, we simply mean using all fluxes without separating wind directions, and we agree that the derived balances will depend on the occurrence of the two wind directions. We still chose to report these CO2 balances because this seems to be quite common for EC studies. To clarify the meaning of "combined footprint" in this case, we propose to change this sentence to: "The annual balance of the combined footprint (using all fluxes without separating periods of different wind directions) was -82 gC m-2 in 2015."

Comment: *(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?*

Reply: We acknowledge that our formulations of this issue weren't clear enough (as also noted by reviewer #1). While it is in principle possible to use the WPL approach in ogive optimization, it is not implemented in the current version of this software. So this task remains to be solved in future studies.
We propose to clarify this part of the text, as noted in our reply to comment 5 of reviewer #1.

Comment: *(12) Page 12, lines 7-8: The observed annual CO2 uptake appears indeed very large. However, I think that such an uptake is well possible. For example, high CO2 uptake was also observed at coastal wet sedge tundra near Barrow, Alaska, by Harazono et al. (2003).*

Reply: We thank the reviewer for pointing us toward this publication by Harazono et al. This paper only reports budgets from spring to autumn, i.e. not the full annual CO_2 budget including possible wintertime release of CO_2. Still, we propose to mention this large growing season uptake in our discussion by adding the following sentence: "The summertime CO_2 sink of Adventdalen is comparable to that of a coastal wet sedge tundra ecosystem at Barrow, Alaska (-105 to -162 gC m^{-2}) (Harazono et al. (2003))."