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Interactive comment on "Annual CO_2 budget and seasonal CO_2 exchange signals at a High Arctic permafrost site on Spitsbergen, Svalbard archipelago" *by* J. Lüers et al.

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Dear Reviewer 2,

thank you very much for your very helpful and thoughtful comments.

Reviewer 2 Specific comments:

"P1536 L25: Add references for the few studies that actually exist." The two sections on P1537 between L6-21 is not very clear to me. I think you should combine them and describe how and why the different periods differ for the different sites. You mention that casual processes affect the spatial variability in effects of the transition periods.

Which are these casual processes, and how and why do they differ for the different sites. Add reference to the sentence claiming that it is growing season and moisture conditions that mainly matters for the growing season fluxes.

Our remark: Thanks! We have added some references. We think, how and why the different periods differ for the different sites is later described in the chapter 4 (discussion). You are right: The "casual processes" means here, e.g. different climate or bio-climate or vegetation zones, different impact of synoptic weather events, islands or continental regions . . . As already pointed out by Lloyd et al 2001, it is common knowledge that the moisture conditions (precipitation during snow-free periods, summertime) have the most important impact to vegetation and soil life and thus to CO2-fluxes. So we did not think about a specific reference. Reference added.

"Add ref to "Several studies have shown. . ." P1537 L 22"

Our remark: Thanks. They are all listed a few sentences later.

"The studies from Zackenberg are based on different ecosystem types. Groendahl is a heath tundra ecosystem, whereas the studies by Nordstrøm and Soegaard are from a wet tundra ecosystem. There are newer references for both these sites: for the wet tundra ecosystem (Tagesson et al., 2012), and for the heath tundra: (Lund, 2012)."

Our remark: Thanks for that! We have added the references and changed the text accordingly.

"You mention the water fluxes at several places in the introduction, even in the aims section in the end. But there is no water fluxes presented anywhere in the paper. I would either include results regarding the energy fluxes, or remove everything about the water fluxes."

Our remark: Thanks for that. We have removed most of this. But at very few cases we would keep the text unchanged, because it helps to interpret the CO2-flux quality.

"P1539. L 23 You have a description of the terrain in the river catchment area. Is this the terrain which is measured by the EC tower? In case it is not the terrain in the footprint, I suggest remove this description, as it is not important for your results. It is also easily misinterpreted as the terrain measured by the EC system. If it is the terrain in the EC footprint, be more specific about it."

Our remark: Thanks! Our terrain description is directly related to the footprint area of our EC-System. Text adjusted.

"P1540 L5 Did the shipping of the LI-7500 to the factory results in a huge gap? This is not mentioned in the description of the gaps."

Our remark: During the study period Mar 2008 to Mar 2009 the device was not sent to the Manufactory for calibration. It was calibrated by the Ny-Ålesund Station personnel on side.

"P1540 L5: The measurement height is 2.9 m."

Our remark: Yes, thanks! It's corrected now!

"Maybe you could mention the general tundra type measured. I assume that it is a heath and not a wet tundra ecosystem."

Our remark: Thanks! As far as we think, it is definitively not wet-land or heath tundra. After Uchida et al. 2009 it is a semi-desert ecosystem with vegetation as described by them and us. We have added an according sentence.

"I have no experience of the TK2 software. But I think you should mention what settings you used in the flux calculations. Did you apply any despiking (which range was used)? What detrend method was used? How was the time lag checked? Did you apply any frequency corrections? What method was used for compensation of the density fluctuations?" You write that you preferred the quality classification procedure used as these tests removed less data. Why is this preferable? The reason for the filtering is to get rid of conditions when the eddy covariance method is not applicable. By including many of these data points, you get an incorrect estimate of the fluxes. For

me it seems like very few data points were rejected. Normally when I work with open path sensors I reject around 40% of the data. In your manuscript, the filtering resulted in a lower rejection rate than 10%. Why is this? I also usually use turbulence integral characteristics and the steady state test as recommended by (Vickers & Mahrt, 1997). But I never have had so many data points left.

Our remark: We have changed the text, including the requested information. Here a more detailed description: TK2 is capable of performing all of the post processing processes and automatically produces quality-assured values for turbulent fluxes for a station in one single run. It includes all corrections and tests, which are state of the art e.g. detection of spikes (Vickers and Mahrt 1997), application of the coordinate rotation methods, correction of spectral loss (Moore 1986), determination of the time delay between sensors, correction for density fluctuations (Webb et al. 1980) and a sophisticated quality assessment. The latter follows a procedure proposed by Foken and Wichura (1996, short "FW1996") and further developed by Foken et al. (2004). Two special quality tests were applied campaigns flux data. The quality flag system developed by Foken and Wichura (1996) classified nine classes. In summary, the classes 1 to 3 are good quality, the classes 4 to 6 are usable quality, class 7 and 8 are only for orientation, and 9 has to be neglected. The first test is a steady state test. It is designed to detect non steady state conditions during the chosen perturbation timescale which violate the assumptions of the eddy-covariance method. This test compares the covariance of a 30-minute integral with several shorter intervals of the same 30-minute interval. The common procedure is to subdivide six 5-minute intervals and to compare the covariance of each of the shorter to the related 30-minute interval. The agreement between each of the six parts with the 30-min value is a measure of stationarity. The second test is based on the flux-variance similarity, which means that the ratio of the standard deviation of a turbulent parameter and its turbulent flux is nearly constant or a function of the stability. These normalized, non-dimensional standard deviations (ratio of sigma(w) or sigma(Ts) to their dynamical parameters u or T) are called Integral Turbulence Characteristics (ITC). This ITC-test compares measured integral turbulence characteristics with modeled ones. The agreement between both measured and modeled values is a measure of well-developed turbulence during the whole integration time of, for example, 30 minutes We only kicked out values with the bad quality flag "9" and values during heavy snow or rain events (be aware that there were only very few such events 2008/2009). Due to the FW1996 approach and the experience we have made by Lüers and Bareiss (2010 and 2011) we could keep many data. Regarding ustar filtering: The scientific background of ustar filtering (Goulden et al., 1996) is to exclude all those data which do not indicate turbulence and where EC assumptions are not fulfilled, so that the EC method cannot be used (Foken et al., 2012). The FW1996 approach we use in our study is the same kind of test as the ustar filtering. But many authors use only the steady-state test without the integral turbulence characteristics (ITC) test of the FW1996 approach, so that the ustar filtering should be applied as well. Specifically, ustar is a test which tries to guarantee that non-turbulent conditions are not considered. Nevertheless, turbulence still exists even for low ustar (probably up to 0.1 m s-1) under steady-state conditions and during intermittent turbulence. These cases are excluded by the simple ustar filtering. Ruppert et al. (2006) shows that more data can be used to parameterize e.g. the Michealis-Menten light response function by applying the FW1996 approach than by applying the ustar filtering. In our specific case, ustar filtering will exclude too large a fraction of data (due to a lot of intermittent turbulence). Therefore, the FW1996 approach has a significant benefit. (Goulden ML, Munger JW, Fan S-M, Daube BC, Wofsy SC (1996) Measurements of carbon sequestration by long-term eddy covariance: methods and a critical evaluation of accuracy, Glob Change Biol 2, 169–182.)

"P 1541 L 10 You never present results of the momentum and sensible heat fluxes, so I do not think that you should mention them in the method."

Our remark: That is just to show the overall quality of the total flux measurements. It helps to interpret the CO2-flux quality.

What did you do to test the impact of the heating of the open-path sensor? This effect

has huge impacts on the NEE estimates by open-path sensors under arctic conditions. I know that it is a problem applying the Burba et al correction when doing measurements when tilting the sensor. But I still think you should give it a try. If not used in the final budgets, I think you should at least use the results in an uncertainty analysis. See comments below.

Our remark: You are right. We added a longer part to address this topic. Using a LI-7500 open-path gas-analyzer, a discussion is still ongoing since (Lafleur and Humphreys, 2007) about the heat generated by the sensor body. Such artificial heating can potentially generate convection within the sampling volume (the infrared pathway) and thus influence the WPL-correction for density fluctuations (Webb et al., 1980). But how efficiently this effect can be avoided or corrected remains uncertain (Grelle and Burba, 2007; Burba et al., 2008; Järvi et al., 2009; Burba and Anderson, 2010; Burba, 2013; Oechel et al., 2014). One way is to follow the suggestion by Foken et al. (2012), where the sensor head should be tilted by at least 45 degrees to one side so that the artificial heat is more than less rising away from the sensor's infrared pathway. This minimizes or even eliminates the possible error due to the heat generated by the sensor. Aware of the suggestion by Burba et al. (2008) and Burba (2013), we tried to apply the only method suitable for correcting of our previously collected data, when in-path fast temperature measurements were not available (method 4). As result (see Figure 1), we obtained totally unrealistic NEE values accumulating to an annual C-budget of around +190 gC m-2 (expected and confirmed are values around zero gC m-2 and year). What are the reasons? It turns out, that the unavoidable positive offset produced by the correction equations of around 0.03 mgC m-2 s-1 added to the NEE-fluxes (same as found by Euskirchen et al., 2012) seems to be suspect, esp. if the CO2-fluxes are generally very low and fluctuating closely around the zero-line, like in our case (or typical for an high-arctic ecosystem). This bias produced by the Burba-equations and apparently induced by only 15 J s-1 heating power, resulting in an unrealistic and wind speed and air temperature independent addition of around 10 to 15 W m-2 to the standard WPL-correction term for a vertical oriented LI-7500. (Be aware that the sensor's window area is only around 1 cm-2 and that at already 1 m s-1 wind speed the air volume in the sensor's open pathway is replaced 100 times per second!) As a consequence, too many NEE-values were shifted above zero, leading to an incorrect aggregation of the bias while accumulating the C-budget. This unavoidable, consistent bias seems to be not much of relevance dealing with productive ecosystems and strong NEE-fluxes, but it is apparently unacceptable if the CO2-fluxes are small (+/- 1 or 2 gC m-2 d-1) and fluctuating around the zero line (causing false flux direction changes). It is further questioned if this consistent bias is an overestimation, especially if the sensor is strongly tilted. In agreement with Oechel et al. (2014) there is currently no adequate method to apply the correction for inclined sensors. Another question arose. At some of the LI-7500 test experiments done by Burba and colleagues (e.g. Grelle and Burba, 2007) the size (diameter) of the used fine-wire thermocouples inside LI-7500 path were unknown or too thick and therefore probably affected by a misleading (short) wave radiation load. That's why we made the decision not to apply this correction.

See Figure 1: original NEE values (blue) before correction of heat produced by the LI-7500 gas sensor and with heat correction (red). Above: time series of 30-min NEE values. Below: accumulated NEE between Mar 2008 and Mar 2009.

"Would the Multi-step error Filtering be necessary in case you would have applied a different filtering method where more data would be rejected?"

Our remark: Yes, definitely! The automatic spike detection and the other plausibility text implemented in our EC-Software dealing with 20 Hz data is not able to detect all possible outliers within produced the 30-min flux values. See Papale et al. (2006). So we expanded Papale's approach and developed this multi-step error filtering for the 30-min flux and the needed meteorological and soil/snow physical time series. This filter doesn't alter any values, just detects outliers. That is also the case if filling gaps! The original data is not altered; just the gaps are filled with the most adequate available values.

"I would not mention the gaps for the periods when you could restore the data. It was unclear to me which data that you could restore and thereby which gaps was not an issue. I suggest removing the sentence about the restored data and remove the gaps when the data could be restored."

Our remark: As said before: We do not "restore" data. If there is a gap no data exists. We have changed the text but want to keep the parts about the gaps to address and explain the gap periods.

"P1543. I suggest giving the equation of the Mihchaelis-Menten function. I do not follow how it was fitted using wind speed and air temperature?"

Our remark: We think to give that world wide well known equation is in our case not necessary. The Michaelis-Menten (1913) light response function is just a relation between NEE-fluxes and incoming shortwave radiation. We do not fit the Michaelis-Menten light response function itself with other parameters. We also tried to find a simple correlation between NEE-fluxes and meteorological parameters during polar night, but there is none. We changed the text accordingly.

"How was the smoothing done? P1543 L18"

Our remark: Running average.

"My main point of concern regarding the methods is that you apply many different corrections to the raw fluxes, and they all change the final budgets that you calculate. I suggest including some sort of uncertainty estimate of the budgets. How would the budgets differ if you had other settings for the filtering of the data, how the budgets would differ if you applied the Burba correction for the heating of the open-path sensor. How did the gap filling method affect the fluxes? What are the random errors? In the end, it might be that these things did not change the budgets considerably, but at least you would have set a number on it. There are many ways of doing an uncertainty analysis, I generally follow the method in the appendix of (Aurela et al., 2002), but you can do any that you find appropriate."

Our remark: First: the typical total error range of EC-flux measurement varies between 5 or 10 % but not necessarily, most of possible "errors" or better "assumptions" applying the EC-method can be corrected (spectral loss, WLP-correction etc.), see Foken et al. (2012) and other chapters of this book. In the EC-community there are no big questions open anymore, dealing with the high-frequent data, e.g. 20 Hz. You said: "How would the budgets differ if you had other settings for the filtering of the data?" You also asked: "Fig 2a. I do not understand why you show both the NEE error filter and the NEE with the gap filling. What is the point with the NEE without gap-filling?" The reason is, to show the reader how much outlier filtering and gap-filling affect the accumulation (not the single data point). We tested some settings applying the multi-step filter, using finally sensible settings. But we think to stress this point in the paper would shift the focus out of our intension. Burba-correction: see our statement above.

"I suggest adding a table with the different budget values, there are very many numbers in the text. It would be easier to follow, if they were mentioned in a table."

Our remark: We thought about that too. But finally we decided against it since it is really only five numbers (summer, autumn, winter NEE) which are of key interest to most readers.

"It would also be nice with a graph that shows the 30 minute NEE fluxes without gap-filling."

Our remark: We thought about that too. But the major gaps are already shown in Fig 2a (black dots). A more detailed graph marking all gaps would lengthen the article without much additional information. Furthermore, such a 30-min-plot would be very "spiky" (as in the figure above) so that not too much can be gained by the reader.

"The Qh and QE is mentioned for the first time in the results at P1546L6. I suggest removing it completely from the paper."

Our remark: Thank you! Sentence is deleted.

"I do not understand how the snow can have an uptake of CO2 during the winter time. Are you absolutely certain that this is not just the effect of the heating of the openpath sensor itself?"

Our remark: Yes! We are definitely sure that this is NOT related to any artificial heating effect of the gas-sensor! Regarding the part of a possible uptake of CO2 during winter into the snowpack: We have rewritten this part and we want to try to give an explanation.

"P1546 L24. Be consistent on the number of significant digits used."

Our remark: Thanks. Corrected!

"P1547 L20 This is incorrect. The Groendahl paper presents data from a heath tundra ecosystem with hardly any vascular plants. It is a heath ecosystem."

Our remark: Thanks. Corrected!

"Figures: Fig 2. I do not think that it is necessary to mention the software in the figure caption."

Our remark: Thanks. Corrected!

"Why is the uptake so big during April and June 2008? It is even bigger than for the peak of the growing season? The pattern during June is really strange to me. Can you try to explain it. Why is there a big uptake in the middle of June, which then suddenly shifts to a large release? Then the growing season starts first after this."

Our remark: We have changed the text to address most of your points. Yes, June is remarkable. The considerable downward CO2 flux occurred between June 13 and June 19 (snow height ca 70 cm). The week before and the days after June 20 were characterized by a strong snow melt, air temperature around +2° to +5°C, but very low wind speed. Within the period June 13 and June 19: no snow melt, air temperature shortly below zero degree, very strong wind and a very strong positive evaporation flux (latent heat flux) but weak neg. sensible heat flux. This situation is quite complicated and not yet fully understood. So we decided not to put this into the article.

Thanks for your great help!

Interactive comment on Biogeosciences Discuss., 11, 1535, 2014.

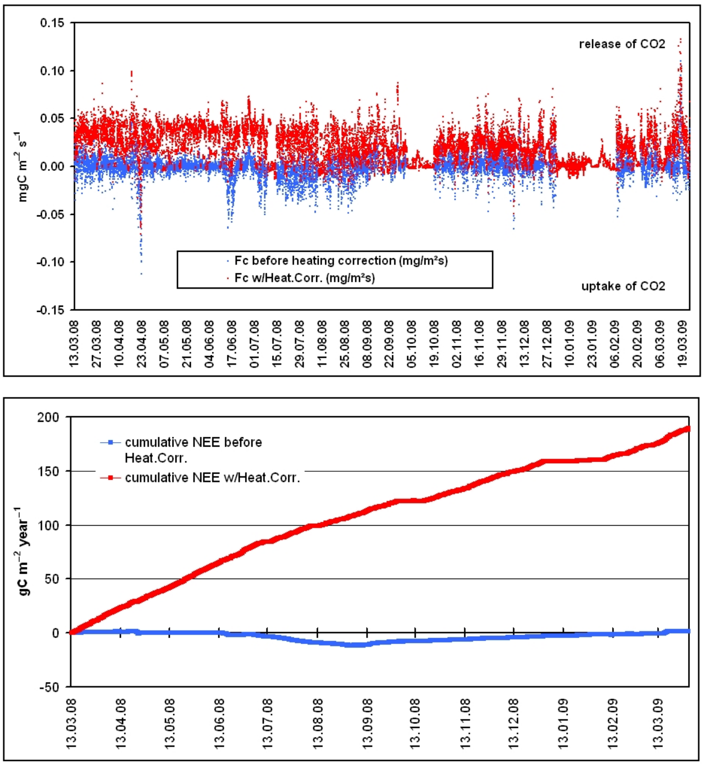

Fig. 1. original NEE values (blue) before Li-7500 heat correction and with heat correction (red). Above: time series of 30-min NEE values. Below: accumulated NEE, Mar 2008 to Mar 2009