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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.

**Anonymous Referee #2**

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Comments to the Author

Review of

'Annual CO2 budget and seasonal CO2 exchange signals at a High Arctic permafrost site on Spitsbergen, Svalbard archipelago' by J. Luers et al.

General remarks:

Luers et al. present a unique data set of CO2 exchange fluxes from a High Arctic permafrost site on Svalbard. I have conducted measurements under similar meteorological and logistical conditions and I am well aware of how challenging it must have



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been to maintain these measurements for the full annual cycle. The manuscript thereby contains comprehensive and very valuable data from a region where climate change is expected to have a pronounced impact on ecosystem functioning. The paper is thereby relevant in relation to the scope of Biogeosciences. There is no specific question that is tackled, but that is ok as bigger relationships of ecosystem functioning and soil-vegetation-atmosphere interactions are investigated. It is generally well written, even though there are some sections I do not fully follow, see comments below.

Introduction:

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.

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

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).

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.

Materials and methods:

P1539. L 23 You have a description of the terrain in the river catchment area. Is this the



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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.

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.

P1540 L5 The measurement height is 2.9 m.

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

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.

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.

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



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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.

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

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.

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?

How was the smoothing done? P1543 L18

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.

Results and Discussion:

I suggest adding a table with the different budget values, there are very many numbers



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in the text. It would be easier to follow, if they were mentioned in a table.

It would also be nice with a graph that shows the 30 minute NEE fluxes without gap-filling. There are very few data sets with NEE fluxes for the winter time from the high arctic, and I think that seeing the real values, not cumulative and daily sums, would be very informative for the readers.

The Qh and QE is mentioned for the first time in the results at P1546L6. I suggest removing it completely from the paper. If the data is presented somewhere else, you can keep this sentence, but then you must add a reference. Or add much more information about the LE and H.

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 open-path sensor itself? I have no problems in believing the pressure effects of CO2 fluxing toward the atmosphere. But the flux into the snow looks very strange to me. Why is the uptake of the snow so high? I do not fully follow in your arguments in the text. I would like to see some more analysis of these events. Is it possible to find any explanatory variables for the uptake of the snow?

P1546 L24. Be consistent on the number of significant digits used, occasionally you use 1 and occasionally you use 2.

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. It is the Nordstrøm and Soegaard studies that present data from a wet tundra ecosystem with a high fraction of vascular plants. (Lund, 2012) is presenting data from the heath for a much longer period as well.

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

Fig 2a. I do not understand why you show both the NEE error filter and the NEE with



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the gap filling. What is the point with the NEE without gap-filling?

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.

Thank you very much for a very interesting article...

Aurela M, Laurila T, Tuovinen JP (2002) Annual CO2 balance of a subarctic fen in northern Europe: importance of the wintertime efflux. Journal of Geophysical Research, 107, 4607.

Lund M, J. M. Falk, T. Friborg, H. N. Mbufong, C. Sigsgaard, H. Soegaard, and M. P. Tamstorf (2012) Trends in CO2 exchange in a high Arctic tundra heath 2000-2010. Journal of Geophysical Research.

Tagesson T, Mölder M, Mastepanov M et al. (2012) Land-atmosphere exchange of methane from soil thawing to soil freezing in a high-Arctic wet tundra ecosystem. Global Change Biology, 18, 1928–1940.

Vickers D, Mahrt L (1997) Quality control and flux sampling problems for tower and aircraft data. Journal of Atmospheric and Oceanic Technology, 14, 152-526.

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