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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 #1

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General comments:

The paper "Annual CO2 budget and seasonal CO2 exchange signals at a High Arctic permafrost site on Spitsbergen, Svalbard archipelago" by J.Lüers et al. presents the results of one year of CO2 eddy fluxes measured continuously by eddy covariance technique over a high latitude Arctic site. Considering the still limited number of published studies reporting on the land-atmosphere GHG exchanges in Arctic tundra ecosystems and the relevant feedback that the mobilization of carbon contained in permafrost could have on global climate change, the subject of the paper is definitely of interest for Biogeosciences. Moreover the paper presents one of the very few year round dataset of net ecosystem exchange (NEE) from the Artic tundra so far available

and evidences the role of the snow cover as a storage for CO2 and the physical drivers behind relatively large CO2 fluxes observed during the winter season. In this sense the level of novelty of the reviewed study is considerable. The manuscripts is well written, clearly structured, and the cited literature is appropriate.

However I found that the analysis did not delve sufficiently into some methodological aspects of flux computation in order to ensure a robust assessment of the annual carbon budget. Here I refer primarily to the correction of the WPL term to take into account the additional sensible heat flux generated within the open path analyzer that, as the authors acknowledge, is responsible for biased results under cold temperature conditions, exactly as those characterizing the Bayelva site. The particular instrumental configuration used in the study does not prevent the measurements to be affected by this problem and although I agree that the approaches to best correct the data are still a matter of debate in the micrometeorological community, I would like the authors to discuss the topic more in depth and bring more evidence that their results are not strongly biased by spurious high frequency temperature fluctuations in the LI7500 path. I therefore suggest to: (i) apply the correction proposed by Burba et al. 2008 (method 4) and to discuss the plausibility of results taking as a reference the maximum values that the correction term may assume according to literature (Burba et Anderson, 2010). (ii) look for correlation between shortwave incoming radiation and CO2 fluxes during the snow covered period to highlight the possible interference of sunlight with the LI7500 mirrors temperature. (iii) provide an estimate of the impact of the correction on the annual carbon budget of Bayelva. In case the application of the correction (method 4) leads to unrealistic fluxes, use published values (correction term as a percentage of CO2 flux magnitude) and propagate the error in the annual sum. Moreover I found that the dataset was not presented in a suitable degree of temporal detail since only daily and seasonal sums of 30 min CO2 fluxes were shown and discussed. I invite the authors to show also diel patterns of (mean$\pm$std.dev) NEE typical of different key seasons of the year and to further illustrate how the rates of CO2 uptake/efflux vary along the day. This would more easily allow a comparison with results of CO2 exchanges,

either based on micrometeorology or chambers, from other arctic sites.

Specific comments:

P1537,L6-9: the uptakes of CO2 (NEE<=0) that occur out of the growing season cannot be related to a biological activity but rather to the CO2 storage of the snowpack. Therefore these "gains" are temporary and do not affect the annual carbon budget. It is then more correct to say that "The annual carbon budgets of arctic ecosystems are not only characterized by growing season exchanges, but also..."

P1537, L10: a recent paper on European tundra and valuable example is Marushchak et al. Biogeosciences (2013). (http://www.biogeosciences.net/10/437/2013/bg-10-437-2013.html)

P1538, L16-19: note that Euskirchen et al.(2012) accounted for the LI7500 extra sensible heat correction while producing an estimate of the annual carbon budget.

P1540, par 2.2. It is not clear how bad data from the LI7500 were discarded. On the basis of plausible ranges of CO2/H2O concentrations though the processing software? Through diagnostic variables such as the AGC? I believe that especially in winter LI7500 data might have been rejected due to snow/ice on the sensor head's mirrors.

P1541, L8. Ruppert et al. (2006) does not appear in the references.

P1541, L.5-7. I agree on the followed quality check method, however in addition I would also look for a friction velocity (u*) threshold to detect if nocturnal advection is potentially leading to unaccounted CO2 effluxes. Given the particular topography of the site, it would be important to exclude the advection of CO2 associated to catabatic wind flows.

P1540-41, par.2.2. What were the results of the energy balance closure at the site? How did they change in the different seasons? These results should be better included, if possible, in the paragraph on the quality assessment of fluxes.

P1542, L1. The suggestion in Foken et al. (2012) is actually to set up the LI7500 head upside down and not just to tilt it to 45 degrees. The adopted set up, although in theory reduces the generation of the additional sensible heat in the optical path, does not exclude it. In this regard I am aware that the formulation (method 4) by Burba et al.(2008) is recommended for tilt angles up to 20 degrees and that it may lead to unrealistic results when applied in your case, but the quality of LI7500 measurements in arctic climatic conditions is too relevant to be overlooked and therefore I expect the Authors to follow the steps that I suggested in the general comments.

P1543, L8-12. Fluxes were "fitted to the Michaelis-Menten light response function...in response of meteorological parameters..such as incoming radiation, wind speed and air temperature." Does it mean you fitted light response functions to subsets of data sorted by air temperature and wind speed classes? If so, how large was the selected range for air temp. and wind speed classes? Could the sentence be rephrased more clearly? Finally, how good was the fit of nonlinear regressions (R^2)?

P1544, L13-14. The diel pattern of 30 min fluxes should be displayed in a graph and reported more in a quantitative form in the results section.

P1545, L1-5. The cumulated NEE over the snow covered period yields the small net CO2 efflux that is fundamentally originated by soil respiration. Supposing that the sum of CO2 storage in the snowpack tends to zero as it should do ideally, can the Authors compare the amount of CO2 exchanged by the snowpack and the atmosphere with the cumulated NEE?

P1545. Par3.2. Could an analytic relation be found between winter time NEE, atmospheric pressure variations and snow cover height? I would expect the maximum magnitude of winter time NEE to scale with snow cover height (storage size) and to be driven by changes in atmospheric pressure (pumping effect). If a significant relation could be found, it would be plotted as a nice additional graph in the paper.

Figure 2: right y axis title: [gC m-2]; legend: 1)daily NEE, 2) cumulated NEE-Error filter,

3)cumulated NEE all gaps filled

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