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Biogeosciences
Discussions

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 1,

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

Reviewer 1 General comments:

"The LI-7550 artificial heating effect and Burba-correction."

Our remark: We have added part of this answer to our text. 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.

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.

A possible interference of sunlight with the LI7500 mirrors temperature does not exist anymore since the very first LI-7500 prototypes.

"I invite the authors to show also diel patterns of (mean_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."

Our remark: We thought about that too. But we would not like to additionally extend our article. The diurnal variation does not much differ from other ecosystems with a longer snow/ice covered season. We would like to point to our dataset (complete 30-min NEE time series) uploaded to the PANGAEA Open Access data library (Lüers and Boike, 2013, doi:10.1594/PANGAEA.809507).

Reviewer 1 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..."

Our remark: Thanks! "affected" replaced by "characterized" We not fully agree that [. . .

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the gains – and losses – are temporary and do not affect the annual carbon budget. . .]. The snowpack as a buffer zone can gain and loose CO_2 or other trace gases Due to the "pumping" effect related to a sharp, rapid air pressure drop or increase, big parts of air is exchanged between snow/soil and atmosphere. These fluxes will affect the C-budget for sure.

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

Our remark: Thanks. Publ. is included.

"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."

Our remark: That's true, but they also said: "This correction was generally small, <0.025 mg CO_2 m-2 s-1, and resulted in no apparent uptake of CO_2 during the winter." Their Li7500 was tilted by 30°. See our general remarks about the Li-7500 heat-effect and Burba-correction.

"P1540, par. 2.2: It is not clear how bad data from the LI7500 were discarded. On the basis of plausible ranges of CO_2/H_2O 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."

Our remark: Due to the very low absolute humidity esp. during winter time or polar night, there is no such icing of our instruments observable. We used state of the art spike detection and quality tests implemented in our EC-software-package (TK2) and the LI-7500 diagnostic feature (AGC) to remove all severe snow or rain events, and of course plausibility ranges (adjusted to the in situ weather conditions) were applied using consistency limits processing the high-frequent 20 Hz data and our multi-error-

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filter routine to detect all kinds of outliers or possible wrong values processing the 30-min fluxes.

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

Our remark: Thank you very much! We have fixed this!

"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."

Our remark: We rewrote the relevant text to make that point more clear. The 2 tests from Foken and Wichura 1996 (FW1996) are doing the same as the simple friction velocity threshold criterion, but with a different - and more universal - approach. 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-

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term eddy covariance: methods and a critical evaluation of accuracy, Glob Change Biol 2, 169–182.)

“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.”

Our remark: All this is published in: Westermann, S., Lüers, J., Langer, M., Piel, K., and Boike, J.: The annual surface energy budget of a high-arctic permafrost site on Svalbard, Norway, The Cryosphere, 3, 245-263, 2009. We think, to include this whole topic would be a not necessary repetition and would shift the readers mind out of focus.

“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.”

Our remark: See our statement above. Please note: Thomas Foken (Foken et al., 2012) never suggested an upside down mounting of the LI-7500. His suggestion is a 45° tilt. That is the case for our instrument at Svalbard.

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 (RËĘ2)?

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Our remark: Michaelis-Menten 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. That would make no sense. We try to find a simple correlation between NEE-fluxes and meteorological parameters during polar night, but there is none. We changed the text accordingly.

"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¿'

Our remark: We agree that it is possible that the CO2 storage in the snowpack – or better the CO2-gas-exchange between soil and snow and snow and atmosphere – tends to zero over time regarding respiration. But the fact is – as we want to show in this article – that depending on the frequency and intensity of the meso-scale synoptic weather events of rapid and strong air pressure changes in both directions combined with air mass changes and strong wind forcing, there is an "input" to the annual C-budget. Indeed, it would be nice to quantify the different C-flux contributions (soil, snowpack, atmosphere, vegetation). We have tried that in the "Summery and conclusion" chapter. But until now, we do not have enough data esp. regarding the impact of that "pumping effect."

"P1545. par. 3.2: 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."

Our remark: As we said just before, we do not have the "right" data yet to do this. Simple correlations check between NEE and snow height and pressure variations didn't

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show any visible relation, but as you said, there may be one.

“Figure 2: right y axis title: [gC m-2]; legend: 1) daily NEE, 2) cumulated NEE-Error filter, 3) cumulated NEE all gaps filled”

Our remark: Thank you! It’s corrected!

Thanks for your great help!

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

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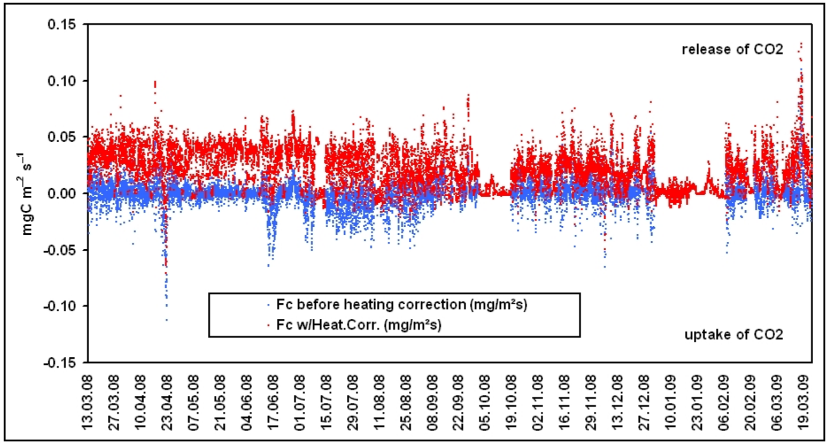

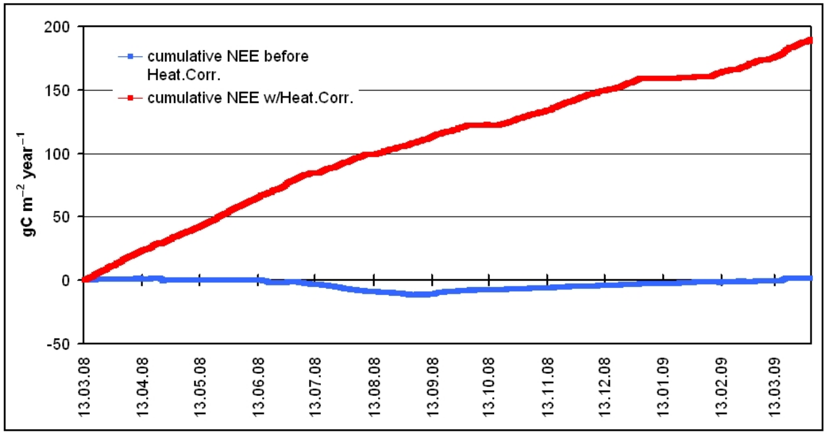

Fig. 1. original NEE values (blue) before Li-7500 heat correction and with heat correction (red). Above: time series of 30-min NEE values. Below: accumulated NEE, Mar 2008 to Mar 2009

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