Please see Author comments below.

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Review repor t on bg-2009-119 Bäckstrand et al BGD 6, 5705-5704, 2009
General Estimates on annual total mire C balances are still most limited and therefore all attempts to fill this gap are impor tant scientific contributions. Bäckstrand et al. accomplish this effor t by using data from an automatic chamber system at the Stordalen mire in nor thern Sweden, installed in three different plant communities. The major improvements compared to many other studies are the high temporal resolution, still measuring at defined plant communities, and also measuring during the winter time period. I see two major shor tcomings with the current presentation. One concerns the winter time period measurements and the other is the nearly non-existing use of uncertainty measurements and accordingly no use of statistics to access whether estimates
C2063
are different or not.

Throughout the paper, from Abstract and onwards, the authors
emphasis the impor tance of including winter time measurements when estimating total annual C exchange budgets, and they present this contribution as one of ver y few achieving this goal. However, when looking into data in some detail no single measurements from the time period between day 341 (beginning of December) and day 84 (end of March) is presented, and for five out of the six years the measurement star ted at day 119 (end of April) or after. To fill data to the winter time period without measurements the authors have used average values from the rest of the winter time period. This means that the data used for gap-filling mid winter (
~4 months) comes from late au-
tumn/early winter and late winter measurements which obviously might induce a bias on the annual estimates as well as on the discussion of differences in seasonal averages and also on the comparison with winter time data from other sites. I suggest that the authors make it clear already from the beginning of the paper (Abstract, Introduction) that the focus on winter time exchange in this paper still excludes measurements from the mid winter season (December – March) otherwise it gives an unfair impression. The omission of data from this time period needs also to be addressed repeatedly in different par ts of the discussion.

**Author comments:**
We believe that we present data coverage in a straight forward way in the method section and in table 1, but it is of course true that it can be expressed even more clearly that the data collection hasn't been continuous during the whole snow season. This has now been clarified in the abstract and in the introduction, and also incorporated in the discussion where suitable.

However, it is worth remembering and mentioning (which also has been done in the new version of the paper) that even if 3 months of gap filling seems like a long period, actual measurements during snow season has been conducted over a period of nearly 2 months before, and 1 months after the gap fill.

We would also like to pay attention to the fact that we use the terms "snow season" through out the paper, which really means when there is a presence of snow (defined in Method section). We are not referring to any standard long-term climatic definitions of different seasons (and shifts between seasons) such as winter, spring, summer and fall. We considered our data availability somewhat too scarce for this.

Also, even if the snow season is short, there is a high number of flux measurements conducted, ranging from around 200 to more than 4000 for one of the sites. This is more than many other snow season (or winter season) flux studies can present and gives somewhat more weight to the results.

The ver y limited use of uncer tainty estimates and
statistical tests also constitute an impor tant limitation when evaluating the usefulness of the presentations and comparison of data with other studies. Different estimates of the variation around the central estimates of average daily exchange rates for the different sites and C-components are given in Table 2, but they are hardly referred to at all in the text. All calculations of accumulated flux estimates for different sites, seasons and C-flux components are totally missing inclusion of uncer tainty estimates, and according to me, this makes comparisons of average values both within this study and with other studies more or less meaningless. I think that both inclusion of uncer tainty estimates as well as statistical tests on whether different averages different or not. Much of the discussion in the paper concerns differences between sites, C-flux components and seasons but without statistical tests it is not possible to evaluate how relevant these discussions are.
C2064

**Author comments:**
When it comes to differences of daily averages between the sites, these analyses and results were presented in more detailed manner in Bäckstrand et al. (2008b). We agree that this result has not been communicated in a satisfactory way in this paper, we are grateful for this notice and we have added this information into the result/discussion section. The results analysing the differences between the sites (and between years), showed that the overall average flux numbers from the three sites were significantly different from each other, whereas the chambers within a site were not significantly different between each other.

We have added standard deviation for daily average of CH4 flux in the text. When it comes to uncertainty measurements of average CO2, they are not representative to show variation around the mean with the aim to compare the average fluxes between the sites because fluxes are 24 hrs average and include both day time (with high C uptake) and night time (with high C). We have added a note about this in the text, that uncertainty measures are to be find in Table 2, and the case for not so representative standard deviation numbers for CO2. We refer in the text that in Bäckstrand et al (2008b), the differences between the sites were confirmed to be significant for both $CO_2$ and THC.

When it comes to uncertainty measurements of the accumulated values, we have taken an approach of presenting the coefficient of variation (CV) based on the standard error of all measured fluxes for each site and each C flux component, and the mean for each site and each C flux component. The CV derived in that way ranges between 1 and 11 % for these different accumulated fluxes (Table 3). Using the CVs in this way indicate the uncertainty in the range of accumulated fluxes that can be derived from the uncertainty in the measured mean fluxes which is useful for everyone to interpret the results.

p. 5706 - Abstract, it would bee much more informative to have the species names and not just the genus. The specific species tells much more about the site conditions than just the genus, the environmental requirements of the genera can be quite broad.

**Author comments:**
We have added a note in the method section/site description that all different plants being present at the three sites are defined with species names in the references of Bäckstrand et al. 2008ab. This saves space in this manuscript and avoid repeating too much of the same information in three following publications. We suggest keeping the text as it is in the abstract because out opinion is that the level of detail is well suited for an abstract.

p. 5706, l. 14 Use accurate number of digits that reflect the precision in the estimates. E.g. I doubt that 0.52 mean that you have much higher precision in this estimate than in e.g. 32. I also miss any estimate of the uncer tainty in the estimates. Without uncertainty estimates it is principally not possible to judge which values that are different or not.

**Author comments:**
We fully agree on that the use of accurate numbers of digits hasn't been adequate. This has been revised though out the manuscript.

p. 5706, l. 16-18 I assume that the whole mire estimate is based on area weighted estimates for each of the three plant communities, add that information.

**Author comments:**
True. We have added a clarification in the abstract.

p. 5708, l. 2 the use of "remineralization" normally not includes autotrophic respiration and therefore tends to give the view that only photosynthesis and heterotrophic respiration makes up the CO2 balance, please reformulate.

**Author comments:**
Reformulated to include auto- and heterotrophic respiration processes.

p. 5708, l. 24-25 do not understand the meaning of this sentence, or you mean average winter time fluxes or what?

**Author comments:**
This sentence has been reformulated as we more clearly describe the extent of snow season flux measurements.

p. 5709, "Study site" add information on the dominating species, just giving info on the dominating genera do not allow the reader to compare the site with other sites. (saying that there are Sphagnum spp. and Carex spp. is about as informative as saying there is trees in the forest!)

**Author comments:**
From above: We have added a note in the method section/site description that all different plants being present at the three sites are defined with species names in the references of Bäckstrand et al. 2008ab. This saves space in this manuscript and avoid repeating too much of the same information in three following publications.

p. 5709, l. 5 Use the standard long term 30-year reference period (1961-1990) for climate description. That allows direct comparison of climate conditions to other sites. In addition you can use other time periods also, e.g. to indicate if you have trends in the climate, but not other time periods alone.

**Author comments:**
The period of climate data is what was available through ANS and we suggest keeping it like

this to be able to show some kind of long-term climate definition.

p.5711, section 2.3 according to table 1 the earliest star t of measurement is day 84 and the latest day of ending is day 341. For five of the years measurements are star ted at day 120 (
~1 May) or later, and for four of the years it is terminated at day 305 (~31
C2065
October) or earlier. This really contrasts to the presentation in abstract and introduc- tion were much space is given to impor tance in this paper by the inclusion of winter time measurements. I realize that some of the late and early months included can be considered winter time, but I am really questioning that these data can be inter polated as representative for the time period of November – April.

**Author comments:**
Please see longer comment in the beginning of the document.

p.5712, l. 9-10 it is more informative to know if the CO-component can be assumed to be important or not, I assume it is not, but just concluding that it is not captured by the analytical system used do not make anyone happy.

**Author comments:**
We know, from the work of Miller and Zepp and Conrad and others, that CO is both consumed and emitted from these surfaces. It certainly doe not make us happy either but our analytical instrumentation (and probably our chambers) cannot measure CO. Of course it is possible that CO emissions will make a small contribution under some particular conditions but it is also true that it can be consumed so we feel the conservative position is that it does not impact our budget significantly.

p.5712, l. 27 just say you have used the GWP of methane instead of limiting it to the wet sites, or do you really mean that you did not use GWP conversion for the dr y site?

**Author comments:**
Because there is no CH4 emissions at the dry site, we have not used and GWP factor on the emissions at this site. So, in fact, the use of GWP is limited to the wet sites. As we describe, it is too uncertain to adopt the GWP factor to the non-methane VOCs because we have not defined specific non-methane VOC species.

p. 5714, Result section – why do you not present a single estimate of the precision in the central estimates? According to me central estimates without any uncer tainty estimates are more or less meaningless. At least you need to present arguments to why you choose not to ad uncer tainty estimates.

**Author comments:**
See above comments.

p. 5715, l.19-24 this just repeats the same information twice, reformulate.

**Author comments:**
Ok.

p. 5716, l. 26 change from “as Stordalen” to “at Stordalen”

**Author comments:**
Great, thanks.

p.5717, l. 25 I am not sure to what extent the incoming light during the winter time at all influences the CO2 exchange.

**Author comments:**
”Little or no diurnal light variation…”

p.5718, l. 28 remove “site”

**Author comments:**
Ok. Thanks.

p.5721, l. 10-15 this is a good example on the problem with not using uncer tainty estimates. You can not tell whether the numbers 6 and 9 are different or not. If you should discuss similarities or dissimilarities you need to add uncer tainty estimates and then test, or at least tr y to judge if the estimates are different or not.

**Author comments:**
We would like to point out that, as the reviewer is well aware, this is a difficult problem because of the differences in the temporal sampling frequency and understanding of the areas sampled. Footprint analyses are always qualitative indices of the surface that is sampled and different methods have different strengths and weaknesses. We know precisely what surface is being sampled with the chamber while E.C. can give us an indication of short term temporal variation in the flux. Presenting the numbers that we do is not so much to exactly compare similarities or dissimilarities, but rather to show preliminary results from two different methods. A deeper analyses of EC and chamber fluxes, both co2 and thc are in preparation. If suggested though, we could delete the whole paragraph starting from line 3.

p.5723, l. 2-5 I would not call the change from 0.95 to – 2.6 a confirmation of a 16% increase. I would merely say that only using growing season data results in a severe C2066
underestimation of the change (assuming that the gap filling of winter time measure-ments used in this study can be justified).

**Author comments:**
Sentence revised. We formulate that if only using green season data, there is a risk of underestimating the decadal change as the snow season has been found to be an important C source period. Also, we say that the results from this study and the Johansson et al study are similar to each (instead of that our results confirms the other).

P.5723, l 25-28 It is not the rate of CH4 flux that has increased, it is the are integrated amount due to increased propor tion of wet areas to the total mire area, is it not?

**Author comments:**
This is true. Sentenced revised.

p. 5727, l. 11 I assume you mean near a change between source and sink, not just near to change. This needs to be reformulated.

**Author comments:**
This is true. Sentenced revised.

p. 5727, l. 18 – 21 This is maybe a little too strong statement, the impor tance of reduced carbon gases (often only CH4 is measured) for the total annual mire C-budgets has been highlighted in several papers before, see e.g. Nilsson et al. 2008, GCB and references therein.

**Author comments:**
”Proven once again” might be a better choice of words. Sentenced revised.

Figure and tables

Fig 2. it is doubtful how informative one year of data from a six year period is. To be able to relate the variation in annual fluxes to climate I think it much more informative to have climate data on all years. One commonly used way to do this presentation is present monthly averages of air temperature and precipitation respectively for the entire measurement period together with the standard period (1961-1990) monthly averages. From such an presentation you can easily judge how the each of the measurement years relate both to each other and to the long term averages. WT and AL can also easily be included in such a figure. I am also curious to way you use ANS data instead of data from Stordalen. That should have been measured at site from the entire time period, or?

**Author comments:**
The aim with the climate data that has been used and showed in the paper was not relate it to the variation in annual fluxes. This was on the other hand presented in a more comprehensive way in Bäckstrand et al 2008b. The aim was to present overall long-term climate conditions for the site for the reader to be able to place Stordalen and Abisko in a bigger picture with its environmental and climatic conditions. Also, the water table and active layer data is there to give the reader a feeling of the differences between the sites when it come to permafrost presence and moisture. We suggest keeping the information as it is presented at the moment.

Table 3. In M&M it is stated that GWP conversion is conducted just for the wet sites. I the table it is said (suffix c) that a conversion factor of 25 is used for the entire column. Looking at data, it is clear that no conversion has been conducted for the palsa site. Assuming 25% of the THC being NMVOC′ s, leave us with an emission of about 0.37 units. Conver ted to GWP it is about 8 which reduces the presented value from 30 to
C2067
22. It is something I do not understand in this presentation.

**Author comments:**
As described in a comment above, there are no CH4 emissions at the dry site and therefore we do not adopt any GWP factor on the emissions at this site. The use of GWP is limited to the wet sites. The CH4 contribution to THC at the three sites are stated in Methods/Data analyses.

Table 6. The heading states that the table presents data from subarctic mires. Siikaneva and Salmisuo can not be regarded as subarctic mires and should be excluded from the table as they are not at all representative for subarctic mires nor is the climate of the sites.

**Author comments:**
It is correct that the Table 6 caption is misleading saying ”subarctic mires”, whereas the left most column in the table indicate that the columns decsribe ”Mire/Vegetation type” and here it clearly states there that Siikaneva for example is a ”Boreal fen”. We have updated the table caption to say ”northern mires” and suggest leaving the data for Siikaneva and Salmisuo for comparion between northern mires, still clearly indicating mire type and long/lat position.

Interactive comment on Biogeosciences Discuss., 6, 5705, 2009.
C2068