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Comment

Interactive comment on “Annual carbon gas budget for a subarctic peatland, northern Sweden” by K. Bäckstrand et al.

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General Estimates on annual total mire C balances are still most limited and therefore all attempts to fill this gap are important scientific contributions. Bäckstrand et al. accomplish this effort by using data from an automatic chamber system at the Stordalen mire in northern 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 shortcomings 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 assess whether estimates

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are different or not. Throughout the paper, from Abstract and onwards, the authors emphasize the importance of including winter time measurements when estimating total annual C exchange budgets, and they present this contribution as one of very 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 started 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 autumn/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 parts of the discussion. The very limited use of uncertainty estimates and statistical tests also constitute an important 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 uncertainty 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 uncertainty estimates as well as statistical tests on whether different averages differ 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.

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p. 5706 - Abstract, it would be 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.

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 uncertainty in the estimates. Without uncertainty estimates it is principally not possible to judge which values that are different or not.

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.

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.

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

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!)

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.

p.5711, section 2.3 according to table 1 the earliest start of measurement is day 84 and the latest day of ending is day 341. For five of the years measurements are started at day 120 (~1 May) or later, and for four of the years it is terminated at day 305 (~31

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October) or earlier. This really contrasts to the presentation in abstract and introduction where much space is given to importance 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 interpolated as representative for the time period of November – April.

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.

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 dry site?

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 uncertainty estimates are more or less meaningless. At least you need to present arguments to why you choose not to add uncertainty estimates.

p. 5751, l.19-24 this just repeats the same information twice, reformulate.

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

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

p.5718, l. 28 remove “site”

p.5721, l. 10-15 this is a good example on the problem with not using uncertainty 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 uncertainty estimates and then test, or at least try to judge if the estimates are different or not.

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

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underestimation of the change (assuming that the gap filling of winter time measurements used in this study can be justified).

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 proportion of wet areas to the total mire area, is it not?

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.

p. 5727, l. 18 – 21 This is maybe a little too strong statement, the importance 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.

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?

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. Converted to GWP it is about 8 which reduces the presented value from 30 to

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22. It is something I do not understand in this presentation.

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.

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