Response for Referee 1

23/11/2012

Major comments:

1. Clarity is needed to distinguish between the terms LCC and LCT. Pg. 10, section 2.5.1, and elsewhere, it seems to me that LCC seems to be a 'purely remote sensing product', while LCT is something else. This was my reading from the paper and it may be wrong. Can you make it clear what these two terms represent and how they are related to one another or not? There is confusion with other terminology – see below.

The referee is right that the abbreviations LCC and LCT were difficult to distinguish from each other, especially when LCC could stand for 'land cover classification' as well as for 'land cover class'. For clarity, both abbreviations were opened and the full terms 'land cover classification' (i.e., map showing the coverage of different land cover types in the region) and 'land cover type' (i.e., a surface with uniform vegetation and soil) were used throughout the manuscript.

The list of abbreviations was removed since the other abbreviations are well-established, and it is enough to open them in the manuscript text.

2. The error estimation (section 2.6) for the chamber scale measurements of growing season and annual balances seems to be fairly simplistic. I think the real errors/ uncertainty may be much larger than this simple statistical estimate, for example see the paper - J. Bubier et al., Net ecosystem productivity and its uncertainty in a diverse boreal peatland, J. Geophys. Res., 104, 27,683–27,692, 1999. I know these are difficult quantities to estimate, but a little fuller discussion of the issue would be helpful.

We admit that the error estimates we use in this work for CO2 fluxes are rather simple and probably conservative for both methods. Various error estimation techniques have been used for the both measuring techniques in the literature (EC: e.g., Baldocchi 2003, Aurela et al. 2007, Moffat et al. 2007, reviewed in Loescher et al. (2006) 'Uncertainties in, and interpretation of, carbon flux estimates using the eddy covariance technique'; chambers: e.g., Fox et al. 2008, Bubier et al. 1999), but there is no single well-established methodology that would be clearly preferable over the others. Use of more advanced error estimation techniques would have probably lead to even higher error ranges, especially for chamber fluxes. We have now mentioned this with a reference to Bubier et al. (1999) in section 2.6.

At the same time, a clear advantage of our study compared to many others is the use of two different measuring techniques and up-scaling approaches. The possibility to compare the results derived from different approaches allows better assessment of the uncertainties related to each method. This gives robustness to our CO2 balance estimates.

In Fig. 8 you compare measured and modelled fluxes from the chamber method as further evidence that the models can be used to extrapolate in space [and time]. Yet, this comparison is basically circular, the same data that was used to develop the models is used here to confirm their accuracy – of course they should perform well! Can you justify this approach?

Indeed, our aim with the modeling work was simply to create a full time-series of hourly CO2 flux values for different land-cover types in order to compare it with the EC results. This procedure is actually better described as temporal integration of the data than as modeling. With this respect, the terminology used was somewhat misleading and has been revised in section 2.3.2 and relevant figure captions and tables. Also, Fig. 5 showing the CO2 fluxes predicted by regression functions vs. the measured data, about which the referee is concerned, has been removed.

3. Figure 3 needs more explanation – it is not clear exactly what is being compared here. If these are some sort of means then the variation for each point should be shown. Related to point 1 above, you state (pg. 12 line 24/25) that these are values derived from the map of different land cover classes (LCC) and then in the next sentence you discuss LCT that deviate from the 1:1 line, hence further the confusion over these terms. Also the text says the p_value is <0.01 and the Figure shows <0.05. In the end, I am not even sure why this comparison is presented here. The results don't seem to be used elsewhere.

In Figure 3 we are comparing the LAI values measured at the chamber microsites to those predicted for the corresponding land cover types by the LAI map. Agreement between these measures shows that the chamber microsites were well representative for the corresponding land cover types in the whole study

region when it comes to LAI. We refer to this figure in discussion section 4.2, where we discuss the chamber plot selection as a possible source of error, and feel that this figure is important prove that we managed to select the chamber plots rather well. We hope that the figure is more comprehensible now when there should not be confusion between the terms land cover type and land cover classification.

We thank for pointing out the mistake in the p-value. The right p-value ($p < 0.05$), shown in the Fig. 3, has been now corrected to the text. Also the standard deviations of the chamber microsite LAI have been added to the figure.

4. Despite the comment above, the LAI results (pg. 13, Fig. 8) are quite encouraging, as others have suggested the importance of LAI as a driver of CO2 exchange in Arctic tundra (e.g., Shaver et al. 2007). One aspect of this not discussed here is inter-annual variability. These results were developed for a particular year, 2008, and very nicely explain spatial variation in fluxes aggregated for chamber microsites. Yet would the same regressions perform well in another year, say one where it was much cooler and wetter? Probably not, at the very least the slope of the line would be different. My feeling about this issue is that LAI can predict CO2 fluxes in a given year, but that flux varies much more inter-annually than LAI on the tundra, so in a predictive sense we need to figure out how the relationship varies between years to make it highly useful. This cannot be done in the present study, but some mention of the issue is the discussion to 'qualify' the present results would be appropriate.

It is very true that based on just one year of data, we cannot say anything about how the LAI-CO_2 flux dependence varies depending on environmental conditions. In our current research we are collecting data to investigate the interannual variability in this relationship based on climatic conditions, and this will surely make the relationship more generic. For this reason, we did not claim in the manuscript that the relationship would be similar from year to year. As suggested by the referee, we have added a phrase on this aspect in the end of the first paragraph of section 4.1.

5. The whole section 3.4.1 (Seasonal variations) needs to be revised. I am not sure the first paragraph of this section is needed, along with Fig. 10. These patterns are not new information and add little to the present study.

The relevance of the first paragraph of section 3.4.1 and Fig. 10 for the paper was questioned by both the referees. We wanted to include it to facilitate comparison with other EC studies (min, max NEE during different seasons), but do agree that it is not in the core of this paper. Our decision was to drop out Fig. 10 and delete the first paragraph, except for the sentence about maximum night-time respiration and net uptake that was incorporated into the next paragraph.

Lines 20-23 (pg. 14), sentence starting "The areal integrated. . .", it is not clear what comparison is being made.

Here the area-weighed ER derived from regression functions for different land-cover types is compared to the measured values, i.e., the point measurement at chamber measurement plots. This is an important comparison since the CO_2 estimate for the long winter period was based on few actual measurements. The modelled value is well within the range of the measured once, which gives us confidence to our cold season estimate.

Also in this section (pg. 15) you use the term "upland microsites" and again I am confused that another terminology referring to the tundra type has been introduced. Does this refer to some of the LCTs or LCCs or is it something else?

Here we changed 'upland microsites' to 'upland tundra', which term is used to refer to one of the three main land cover types (Upland tundra – Dry peatlands – Wetlands; see Table 2). There was some inconsistency in how this land cover type was called in the manuscript (tundra heath – upland tundra), but this has now been fixed.

6. The discussion section is somewhat long and could be made more relevant to the paper. For example, section 4.2 is rather monotonous and even though all of the explanations for the potential differences between the two measuring techniques are valid, the most likely reason for the differences in seasonal values is that one flux (EC) is mostly derived from measured data and the other (chambers) is largely modelled data. This is mentioned on pg. 18, line 22-25, but is not given much weight among all these other factors mentioned but which cannot be tested. Finally, pg. 19 the statement on lines 10-15 is rather obvious and not helpful here.

We revised the discussion section extensively based on the feedback received from the referees. Section 4.2 on the differences of the two methods was fully revised in order to give the focus on the most essential. However, we feel that this kind of critical assessment is very important for this kind of study using two measuring techniques in parallel, even though it is difficult to verify the relative importance of different error sources.

7. Section 4.4 is quite speculative and although I understand what the authors are trying to achieve there, it does not add to the paper and should be removed.

We understand the concern of the referee about the relevance of the section 4.4 about the climate change impacts on tundra C balance for this particular paper. However, it included some discussion of the relative importance of different land cover types for the regional CO_2 balance, not discussed elsewhere, but something very much in the core of this paper (e.g., importance of willows with the report of Forbes et al. 2010 in view). Also, the heterogeneity in the CO_2 balance across the tundra landscape was so evident in this study that it was necessary to emphasize the importance of landscape reorganization for the regional CO_2 balance, although we admit that this is not new knowledge from this study.

Other minor points:

1. pg. 13, lines 2-5, this sentence should be moved to Methods section somewhere

This sentence about the sign convention of flux data has been removed to the end of the section 2.3.1.

2. Is Figure 12 needed? These results are already somewhat confirmed by Fig. 4. If this figure is retained, please put labels on the x-axis.

Figure 12 (EC fluxes partitioned to ER and GP components by modelling ER) was combined to the Figure 4 that shows similar data for the fluxes of land cover types based on chamber measurements.

3. pg. 16 line 13, the 0.98 vs. 0.83 comparison should be noted which value is region and which is EC footprint.

This has been now indicated in the text.

4. through out the paper you use modelling and modeling, be consistent.

The spelling of this word has been checked and is now consistent.

5.pg. 19, line 25, where should be were and line 28 remove the words “for the observed”

These two corrections have been made.

6. I am not sure Table 6 is necessary and it is certainly not extensively used for comparison here. No mention of it in the discussion of LAI effects on pg. 20. Perhaps more specific comparisons between the present study and those in Table 6 are needed.

We would like to keep the Table 6 that is tightly connected to the two first paragraphs of the section 4.3. Although we do not go through specifically all of the studies listed in Table 6, it gives at one glance an idea of the numerous tundra CO_2 studies carried out earlier. A sufficient review of these studies in the manuscript text would be wordy. The whole section has now been revised, based on the comments from the referee and the role of LAI in explaining the between-site variability is included in the second paragraph of the section.

Additionally, the Table 6 shows clearly that among the many tundra CO2 studies, those ones with a nested scale design combining chambers and EC are very few. A mention on this was lacking in the manuscript, but has now been added in the discussion, in the beginning of section 4.2.

7. pg. 25 line 26. The author list for this citation is incorrect.

The author list has been corrected.

8. Are Table 1 and Figure 2 both needed? Perhaps drop Fig. 2?

We considered this suggestion and since Fig. 2 has little added value to Table 1, decided to leave it out from the paper.

9. Fog. 6 does not have labels on the x-axis.

Labels were added.

10. Figure 8 mention the year of data in the caption.

The year was added.