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Discussions

Interactive comment on "Carbon dioxide balance of subarctic tundra from plot to regional scales" *by* M. E. Marushchak et al.

Anonymous Referee #1

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In this study measurements of carbon dioxide fluxes over subarctic tundra in northeast Russia were made via chamber and eddy covariance (EC) techniques; fluxes from the two measurement systems are compared and then scaled with remote sensing to produce estimates of the regional fluxes. As in previous studies, the researcher found that the chamber and EC systems agreed well and that scaling techniques based on land cover classes or leaf area index (LAI) produce slightly different results. Overall the study was well constructed. The measurements appear to be carefully made and data handling (e.g., partitioning of ER and GEP and gap-filling) follows acceptable published procedures. The CO2 measurements will be of some interest to Arctic researchers as they illustrate the variability among tundra types and add to the sparse availability of such measurements. The greatest asset and most novel contribution here is the results of scaling exercise. The results are intriguing and potentially valuable for future

studies, however some additional clarity and explanation is needed. As well, I have some concerns about the modelling work. The authors should consider the following points before publishing this work.

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

Other minor points: 1. pg. 13, lines 2-5, this sentence should be moved to Methods section somewhere 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. 3. pg. 16 line 13, the 0.98 vs. 0.83 comparison should be noted which value is region and

which is EC footprint. 4. through out the paper you use modelling and modeling, be consistent. 5.pg. 19, line 25, where should be were and line 28 remove the words "for the observed" 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. 7. pg. 25 line 26. The author list for this citation is incorrect. 8. Are Table 1 and Figure 2 both needed? Perhaps drop Fig. 2? 9. Fog. 6 does not have labels on the x-axis. 10. Figure 8 mention the year of data in the caption.

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