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Interactive comment on "Carbon budget estimation of a subarctic catchment using a dynamic ecosystem model at high spatial resolution" *by* J. Tang et al.

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We really appreciate the constructive comments. The comments from the reviewer are included in this document within "", followed by our responses to each comment.

"The manuscript titled "Carbon budget estimation of a subarctic catchment. . ." by Tang et al. is well written, though a bit long, but given its extensive scientific content also understandable. In their study they combine a process-based dynamic model with observations of terrestrial and aquatic carbon fluxes to simulate long-term carbon exchange in Stordalen, a subarctic catchment in Northern Sweden. They point out the difficulty in fully understanding the complexity of C (CO2 & CH4) fluxes in such a heterogeneous terrestrial and aquatic ecosystem comprising of lakes/streams and a variety of vegetation types, from peatlands to forests, from permafrost to non-permafrost areas of the catchment. The manuscript also highlights the difficulty in modelling such processes as each year can drastically vary from the previous, where some ecosystems (forest) act as a sink one year while acting as a source the following yearAll in all a well-executed study. Nevertheless there are some minor points the authors should address: The abstract is rather long and should be shortened where possible."

Responses: We have now shortened the abstract by rephrasing some descriptions of the results.

"“Arctic” should be written with capital letter where appropriate."

Responses: Thanks for pointing out this error. We now use ’arctic’, ’Arctic’ and also ’subarctic’ and ’Subarctic’ where appropriate.

"Please include a map showing the location of Stordalen/Abisko or at least the coordinates so readers who do not know the area can place Stordalen on a map."

Responses: The coordinates of Abisko station have been added now (see Page 9, line 8).

"The warming potential of CH4 for the 100 years horizon is 21 or 25 according to the various IPCC and UNFCCC reports, not 28!"

Responses: In the latest IPCC AR5 report (see WGI section 8.7, Table 8.7), the GWP value of methane (CH4) for a 100 years horizon was updated to 28. We therefore decided to use this figure. Here is the direct link: http://www.climatechange2013.org/images/report/WG1AR5_Chapter08_FINAL.pdf

"What does strike one when reading is the time frame chosen for the model. Why has it been projected to 2080 instead of remaining in a rather more realistic time frame? Uncertainties must be rather high, given the high year to year variability and the very short measurement time that has been included in the evaluation of the model. This

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is of concern, in particular as the authors do point out the model weaknesses and the tendency of under- and/or over-estimation of fluxes. Please include uncertainty values for the model evaluation time frame."

Responses: The choice of the projection to the year 2080 was based on the available Rossby Centre Atmosphere Ocean (RCAO) RCM output, which was used to extract monthly anomalies in the model setting (Please see section 3.2.1). The reason we looked at such a long time period was to examine the interactions of different carbon component dynamics and to try to identify the determining factors influencing catchment C sink/source functionality in a warming future, though the uncertainties are quite high for the evaluation years. To address the uncertainties of modelled fluxes during the observation period (2006-2011, a period covering all the observations used in this study), we have now estimated annual variations (presented as one standard deviation (s.d.)) of each flux component in the revised manuscript (see values in the parentheses in Table 2). Moreover, we also added the annual variability estimations for other time periods (1961-1990, 2000-2005, and 2051-2080). As the reviewer has rightly pointed out, the annual variability was quite high. From the estimated mean and s.d values, we can now clearly see the high interannual variation of C fluxes for the birch forest, for example, acting as a C sink in some years, but as a C source in others (see the revised Table 2).

"please change the colour of the mean lines mainly in column b as they are not visible."

Responses: Thanks for pointing out it. Now we have changed the colors for column b - see Figure 6 in the revised manuscript.

"Figure 7: Please rephrase the figure caption as it is not clear. Currently it appears as if it belongs to a different graph. It describes differences in simulations but the only comparison can be made between vegetation types not model runs (with and without CO2 increase)."

Resonses: The caption of Figure 7 has been clarified now, The caption of the figure 7 was revised as: Carbon flux differences (ΔC fluxes) for different landcover types with and without a CO_2 increase since 1960. Positive values of ΔC NEE imply a higher uptake or a lower emission of CH_4 in the simulations with a CO_2 increase compared to the simulation without a CO_2 increase. For the birch forest landcover type, the differences in gross primary production (GPP) and ecosystem respiration are shown in the panel (a), where the positive values indicate a higher photosynthesis rate and a higher respiration rate in the simulations with a CO_2 increase, compared to the simulations without a CO_2 increase.

"How do the authors explain the distinctive different variability in the model outcome for the period 1913 - 1970, 1970-2020 and 2020 onwards in Figure 7? The model outcome shows no variability at all until approx. 1970 with a little increase in variability until 2020 and with a dramatic range onwards. Please explain how such differences are plausible. The authors have mentioned that out of 4 measured years (forest) two were sinks for CO_2 and two were sources. They also included disturbances in the model but these seem to be completely irrelevant in the future as the graph is showing the forest not to act as a source ever again (according to the current graph)!"

Responses: In Figure 7, we compared carbon fluxes of different landcover types (where we identified different landcover types based on dominant PFTs for the period 2001-2012) from two simulations with and without CO_2 increase after 1960. The driving climate and CO_2 data are the same before 1960 for both simulations, so we almost see no differences for the period 1913-1960. After 1961, the CO_2 concentration input data starts to differ, so we start to see the differences in the modelled C fluxes between two simulations. The increasing trend of ΔC flux magnitudes can be identified for the birch forest and tundra heath, as the reviewers also pointed out. The reasons for this trend for birch forest and tundra heath are because with higher temperature and CO_2 concentration, the responses of GPP are stronger than the ecosystem respiration in the model, which is illustrated in the revised Figure 7(a). As we can see in figure 7(a), the ΔGPP rises much faster than the Δrespiration, so we see the increasing trend of

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ΔNEE for these two landcover types. However, the peatland vegetation shows smaller differences in terms of ΔGPP and Δrespiration. Regarding the C sink and source functionality of birch forest that the reviewer pointed out, we can see the dynamics in Figure 6-2a. Some years in the future are indeed seen to be carbon sources in the model output.

"Supplement information, Figure S2: There is no differences between the 2 different model outcomes. Please include statistical significance values as they do not seem to be any different."

Responses: Thanks for pointing out this. Now we add Mann-Whitney U test statistical significance values in Figure S1 (not Figure S2 though). The comparisons of birch forest, tundra heath and the whole catchment carbon budget show significant differences between the two simulations with and without a CO_2 increase since 1960.

Interactive comment on Biogeosciences Discuss., 12, 933, 2015.