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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.

Anonymous Referee #2

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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 (CO_2 & CH_4) 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)

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act as a sink one year while acting as a source the following year.

All 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.

- "Arctic" should be written with capital letter 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.

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

- 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 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.

- Figure 6: please change the colour of the mean lines mainly in column b as they are not visible.

- 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).

- 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 CO2 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)!?!

- 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.

Good luck!

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