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Interactive comment on "The role of snow cover and soil freeze/thaw cycles affecting boreal-arctic soil carbon dynamics" *by* Y. Yi et al.

Anonymous Referee #1

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The authors propose different simulations performed with a hydrology-and-C-coupled model, aiming at pinpointing the role of snow (SWE, SCE) and air temperatures in shaping the ground thermal regime and carbon fluxes at different depths.

The authors do a great job in using their detailed model to highlight specific temperature and snow controls. They even manage to derive general conclusions though retaining the specificities of temperature zones and deep vs shallow ground layers. Their assessments are very well supported by the figures, among others the ones provided in the Supplement, which bring interesting additional information. The paper is very well written and therefore reads most agreably, which I thank the authors for. Furthermore, model and data are well documented and model-to-data comparisons are provided, which enhences the reader's confidence in the model and highlights its limitations ;

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these are also discussed in the manuscript. Links are made to experimental studies that put these model results into perspective.

Overall, I want to congratulate the Authors for this well-balanced and enlightning manuscript, that really serves the scientific community in pinpointing dominant controls for soil carbon decomposition and thermal regime, related to air temperature and snow and differenciated between warm and cold season. I feel that the knowledge acquired here as to these mechanisms can be valuably used for local and regional studies. I therefore believe the manuscript is suitable for publication, pending the (minor) modifications suggested below:

**** Main comments :

A huge part of the manuscript relies on correlations analysis to derive dominant controls. However, the methods behind these are not described: which time-frame is used for these correlations, which simulation, etc... ? Please add a detailed Methods section on this.

Also, my feeling is that the paper's title doesn't fit the paper's content, in that the "freeze-thaw cycles" are not specifically delt with. A title like " Snow- and temperature-related controls on boreal-arctic soil carbon dynamics over the recent decades" would probably be more faithful to the content. The abstract should also be modified accordingly.

At some places in the manuscript, (p5: "The objective of this study is to assess how northern soil thermal and carbon dynamics respond to changes in surface temperature, snow cover and freeze/thaw conditions indicated by satellite observations."; also p 11 l 5 and 6) the reader gets the feeling that satellite observations related to snow cover and freeze-thaw cycles are somehow used to drive the model and derive the highlighted controls. My understanding however is that the only satellite data driving the model are the GIMMS3g NDVI data. Please clarify this point or correct me if needed.

In the "T & P varying simulation" the model provides estimations of changes in snow

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cover extent, duration, SWE, etc., that could be compared to satellite (and other) observations like the one cited in the introduction (Brown and Robinson, 2011; Kim et al., 2012; Dyer and Mote, 2006). A short comparison of model results to these observations would strengthen the confidence in the model results with respect to the representation of snow and freeze-thaw processes. It could fit into the 3.1 "Model validation" section.

**** Minor comments :

- There are some important studies regarding the thermal (and biological) impact of snow that you did not cite, though your work somehow "outperforms" them. May I suggest Sullivan, 2010, and Gouttevin et al., 2012? They focus on different snow properties induced by vegetation and their impact on the ground thermal regime and carbon stocks. Your paper focuses on other snow-related controls and goes farther than these older studies by highlighting the impacts of different controls at different depths into the ground, explaning the potential consequences for ALT and old permafrost C remobilization.

Sullivan, P. F. (2010), Snow distribution, soil temperature and late winter CO2 efflux from soils near the Arctic treeline in northwest Alaska, Biogeochemistry, 99, 65–77, doi:10.1007/s10533-009-9390-0.

Gouttevin, I., Menegoz, M., Domine, F., Krinner, G., Koven, C., Tarnocai, C. and Boike, J. (2012), How the insulating properties of snow affect soil carbon distribution in the continental pan-Arctic area, Journal of Geophysical Research, 117, G02020, doi: 10.1029/2011JG001916.

- p 12 : wouldn't it have been more appropriate to adopt for T the same "rescaling" methodology as for precipitation in the "P-varying" sensitivity scenario ? I would suspect that using a mean T-climatology based on 1979-1981 can induce inconsistency, like having high temperatures during a rain event when in reality cooling occurs..

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- p12 l 24-25-26 : "representing major vegetation types across the pan-Arctic domain, and having at least one year of observations available. For the validation [..] meteorology" : this belongs to the Data Section rather than here.

- p13 l 1 to 5 : please explain how uncertainty in R and RMSE are computed. Adding the relative RMSE or a main value for daily GPP or NEE would comfort your findings

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