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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.

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Response to referee's comments on "The role of snow cover and soil freeze/thaw cycles affecting boreal-arctic soil carbon dynamics"- Manuscript BG-2015-279

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Dear Editor,

We appreciate the helpful comments from the two reviewers. Our responses to the comments are provided in the following text, and the revised manuscript is enclosed as a supplement with changes highlighted. Thank you for considering our manuscript.

Review 1:

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1) 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.

Response:

We added an additional description in the methods section (Section 2.4) for better clarification of the correlation analysis (Page 10, paragraph 3):

"We mainly used correlation analysis to evaluate the climatic controls on simulated soil temperature and carbon fluxes. The outputs from the model baseline simulations (i.e. varying T and P) from 1982 to 2010 were used for this analysis. The period from 1979 to 1981 was excluded in order to reduce the impact of the spin-up process on model simulations. We first calculated the correlation coefficients between the time series of each climate variable and modeled soil temperature or carbon fluxes at each grid cell from 1982 to 2010. The resulting correlation coefficients were then averaged for each climate zone classified using the annual mean air temperature (1982-2010) and binned into 2.5 °C intervals. The climate variables used in the correlation analysis included air temperature, snow water equivalent (SWE) and snow cover extent (SCE)..."

2) 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.

Response:

We agree with the reviewer. The main focus of this paper is to evaluate the role of snow cover on soil thermal and carbon dynamics. Therefore, we changed the title to "The role of snow cover affecting boreal-arctic soil freeze/thaw and carbon dynamics".

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We also made associated changes to the abstract. Please refer to the manuscript for details.

3) 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 he only satellite data driving the model are the GIMMS3g NDVI data. Please clarify this point or correct me if needed.

Response:

The reviewer is correct that the GIMMS3g NDVI data are the only satellite data driving the model. Other satellite observational datasets, including snow cover extent (SCE) were used for model validation, but are independent of model drivers. We agree with the reviewer that there may be misunderstanding of the satellite datasets used in the model simulations. We have clarified this in the manuscript, including the following sections:

Page 3, Paragraph 3: "The objective of this study is to assess how northern soil thermal and carbon dynamics respond to surface warming and changes in snow cover conditions during the satellite era (since 1979)."

Page 7, Paragraph 2: "Primary model drivers include daily surface meteorology and satellite-based normalized difference vegetation index (NDVI) records."... "The third-generation Global Inventory Monitoring and Modelling Studies (GIMMS3g) NDVI dataset (Xu et al., 2013) was used to estimate litterfall seasonality and FPAR, as critical inputs to the TCF model (Yi et al., 2013)."

Page 8, Paragraph 3: "A dynamic litterfall allocation scheme based on satellite NDVI data (Appendix A2) was used to prescribe the model simulated daily litterfall fraction

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through each annual cycle...".

Page 13, Paragraph 2 and Figure 3: "The model simulated SCE was generally consistent with satellite observation based global climate data records documenting weekly SCE changes..."

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

Response:

We compared the model simulated snow cover extent (SCE) with satellite-based snow cover records (Brown and Robinson 2011), and the results are now presented in Section 3.1 (Page 13) and Figure 3 of the revised manuscript:

"The model simulated SCE was generally consistent with satellite observation based global climate data records documenting weekly SCE changes (Brown and Robinson, 2011; Fig. 3). The model simulations show a similar mean seasonal cycle as the satellite observations, with spring snow melt mostly occurring from April to May, and fall onset of seasonal snow cover occurring in October over the 1982 to 2010 record (Fig. 3a). The model simulated SCE shows consistent changes with the satellite observations in spring, indicating realistic simulation of the snow melting process. However, the model generally underestimates SCE in the fall and winter. The model did not directly simulate SCE, which was calculated from simulated snow depth using an empirical equation (Eq. 2). Based on Eq.2, the modelled SCE will never approach 100 percent, while the satellite data indicates nearly complete winter snow cover over the study domain. Larger model SCE differences from the satellite observations are ex-

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pected when the snow cover is relatively shallow and patchy owing to the relatively coarse spatial resolution of both model simulations and satellite observations. Moreover, the satellite SCE dataset is presented as a binary classification at a weekly time step, which may not adequately depict transient SCE fluctuations under active surface melting and freezing processes in the fall (Kim et al., 2015)." We also compared the model simulated non-frozen period with a global freeze/thaw (F/T) dataset (Kim et al., 2012) based on satellite passive microwave remote sensing including Scanning Multichannel Microwave Radiometer (SSMR) and Special Sensor Microwave Imager (SSMI). The results were presented in below Fig. R1. The surface F/T status of the satellite dataset was classified using a temporal change classification of 37 GHz daily brightness temperature measurements. The 37GHz frequency has a relatively shallow penetration depth and is more closely related to air temperature rather than soil temperature (Kim et al., 2012). Our results also show that the satellite-based non-frozen period is very similar to the non-frozen period calculated from the model inputs of air temperature from the WFDEI dataset, and generally longer than the non-frozen period calculated from model simulated soil temperature at 1cm depth (Fig. R1). Therefore, we chose not to include this additional analysis in the model validation.

5) 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.

Response:

Thanks for the references. They are now cited:

Page 18, Paragraph 2: “Strong snow cover insulation on soil temperature sustains soil respiration even under very cold winter air temperatures and the resulting winter soil respiration can be a large component of the annual NEE budget (Sullivan et al., 2010).”

Page 19, Paragraph 1: “Surface warming during the summer has a dominant control on upper soil layer temperatures (<0.5 m; Fig. 5a), while a deeper winter snowpack has a persistent warming effect on deeper soil temperatures in colder climate areas (Fig. 5b; Gouttevin et al., 2012).”

6) 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..

Response:

We acknowledge that it could be true; however, the influence of this potential inconsistency on our analysis is very likely small. The major objective of the sensitivity analysis is to separate the effects of warming temperature and changing precipitation on simulated soil thermal and carbon dynamics. The reason that we chose slightly different rescaling methodology for T and P is due to the difficulty in characterizing the daily frequency of precipitation events and thus creating a daily P climatology. Simply averaging daily precipitation time series will create a number of small precipitation events, and is inconsistent with the precipitation probability distribution. Thus we created a daily T climatology (this also means a monthly T climatology) for the simulations of the “P-varying” sensitivity scenario, but used a monthly P climatology for the simulations of the “T-varying” scenario. Besides, the model simulations were aggregated to monthly

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time step before conducting the analysis.

7) 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.

Response:

We agree with the reviewer and moved this sentence to the methods section (section 2.2.1; Page 6, Paragraph 2).

8) 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.

Response:

We revised the caption of Table 2, and indicated that the R and RMSE uncertainty was calculated as standard deviation of R and RMSE values. We also added the daily mean GPP value for each plant functional type. However, this was not done for the NEE fluxes since most sites are carbon neutral or a small carbon sink and the daily mean NEE values are generally close to 0 or small negative values. âĂČ

Review 2:

1) However, SOC decomposition is not only a function of temperature and moisture as assumed in this and many other modeling studies. There are many other important environmental controls on SOC decomposition which largely remains not included in any modeling studies such as organomineral controls, microbial controls, soil aggregation, and other pedogenic controls. Relative sensitivity of these environmental controls on total magnitude of soil respiration remains unexplored, and I will be pleased if authors include these missing controls in the limitationssection. Further, please include discussion about winter warming impacts on soil respiration.

Response:

We added an additional paragraph in Section 4.3 discussing the effects of other environmental controls on SOC decomposition as recommended by the reviewer (Page 19, Paragraph 2):

“Although soil temperature and moisture are the two major environmental controls on soil carbon decomposition, other factors may also influence soil decomposition rates and permafrost carbon feedback potential, but not represented by our modeling study (Hobbie et al., 2000). A number of chemical and biological factors can affect the temperature sensitivity of soil carbon decomposition in northern soils, including enzyme abundance, microbial population size and oxygen availability (Waldrop et al., 2010). Previous studies also show that soil carbon decomposition rates may be depth-dependent. Accounting for vertical changes in soil biogeochemical properties and processes, including the size and substrate quality of the soil active layer and permafrost carbon pool, and the degree of N mineralization with decomposing permafrost carbon, may have significant impacts on the sign and magnitude of the projected high-latitude carbon response to future warming (Koven et al., 2011; Koven et al., 2015). Finally, changing wintertime soil microclimate will alter the amount and timing of plant-available nutrients (N) in tundra ecosystems, and may drive a positive feedback between snow, soil temperature, microbial activity, and plant community composition (Schimel et al., 2004; Sturm et al., 2005).”

We also addressed the winter warming impacts on soil respiration in Section 4.2 (Page 18, Paragraph 2):

“On the other hand, winter warming may change the depth and structure of insulating snow cover, affecting underlying soil temperatures, which could alter soil N mineralization rates and soil microbial activities that influence ecological processes during the growing season (Schimel et al., 2004; Sturm et al., 2005; Monson et al., 2006).”

2) Another important area I like to point is authors mentioned at multiple places in the manuscript about large spatial heterogeneity in the study area. They used a variety

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of data sources with diverse spatial resolutions in their modeling efforts. It will be interesting to know about the contribution of this spatial mismatch of driving datasets in determining the total uncertainty in the modeled outputs. In my knowledge, no analysis was conducted to determine the spatial distribution of uncertainty across the study area. If so, please include this in your limitation section.

Response:

We agree with the reviewer that spatial heterogeneity is one of the major uncertainty sources for the model simulations in the pan-Arctic domain (and for other large-scale model simulations). We regridded all of the input datasets into same 25-km EASE-GRID projection prior to the model runs. Therefore, the contribution of the scale mismatch of the input datasets is likely small. However, the subgrid spatial heterogeneity may contribute more to the uncertainties in the model simulations. For example, we used the dominant vegetation type for each 25-km grid for the model simulations even though more detailed information on the vegetation cover is available. We now address this in Section 4.3 (Page 20, Paragraph 3): "Current large-scale model simulations, including this study, generally operate at the order of tens of kilometers or even larger, and may not adequately represent the effects of surface heterogeneity on simulated permafrost hydrologic processes and soil carbon decomposition processes (Koven et al., 2011; Rawlins et al., 2013; Schuur et al., 2015). For example, most models prescribe a dominant vegetation type or a single value for the organic layer thickness commensurate with the model spatial resolution, which likely introduces large uncertainties to the model simulated moisture and heat fluxes and thus the permafrost properties."

3) P5L15-16 Surface organic layer is an extremely important factor in regulating active layer dynamics and extremely heterogeneous soil property, and not/or improperly represented in current model structures. I am not clear how surface organic layer was represented in this study? Please make sure that surface organic layer, soil organic layer and SOC layers mean different things, and use these terms appropriately.

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Response:

We agree that both surface and soil organic layer are very important in regulating soil active layer properties in permafrost areas. In this study, we did not represent the surface organic layer (i.e. nearly pure organic matter of the surface soil layer) separately from the soil organic layer. Instead, a simplified approach (Lawrence & Slater, 2008; Rawlins et al., 2013) was used to distribute the observed soil organic carbon from the Circumpolar Soil organic Carbon Database into the upper 1.4 m soil layers. This approach was consistent with previous regional modeling studies and a necessary simplification for a large-scale modeling study, given the general lack of more detailed spatial information on organic layer properties over the pan-Arctic domain.

4) P6L20, may be you mean surface organic layer by "SOC layer"?

Response:

Yes, we corrected this as "surface organic layer" in the revised manuscript.

5) P6L6-8 please explain more how was this effect was represented in this study?

Response:

We represented this effect by introducing the soil organic carbon fraction into the equations calculating the soil thermal and hydraulic properties. This has been explained in Appendix A1 of the revised manuscript. We also modified this sentence for better clarity as: "Other model improvements include accounting for the impact of soil organic carbon content on soil thermal and hydraulic properties (Appendix A1, Eq. A3)..."

6) P6L28-P7L3 I don't understand the meaning of this citation here, Hugelius et al. (2014) did not partition SOC into fast and slow pools, and neither the depth distribution adopted in this study is consistent with Hugelius et al. (2014).

Response:

We removed this reference, and also revised the sentence as:

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“The three litterfall pools were distributed within the top 20 cm of the soil layers; the three fast SOC pools were distributed within the top 50 cm of the soil layers, and the deep SOC pool extended from 50 cm to 3 m below the surface”.

Most models (including our study) generally use crude assumptions for the SOC distribution along the soil profile due to lack of detailed measurements, and this may cause additional uncertainty in the model simulations. We addressed this uncertainty in the discussion (Section 4.3, Page 19, Paragraph 2):

“Previous studies also show that soil carbon decomposition rates may be depth-dependent. Accounting for vertical changes in soil biogeochemical properties and processes, including the size and substrate quality of the soil active layer and permafrost carbon pool, and the degree of N mineralization with decomposing permafrost carbon, may have significant impacts on the sign and magnitude of the projected high-latitude carbon response to future warming (Koven et al., 2011; Koven et al., 2015).”

7) P7L25-P8L2. No consistency between this statement and table, please correct this. Table shows only one tundra site and one boreal forest site.

Response:

There are three tundra towers representing three tundra types at the tundra site. We clarified this in the text (Page 6, Paragraph 2):

“Therefore, we selected one boreal forest and one tundra site with detailed in situ measurements (including carbon fluxes, soil temperature and soil moisture) for additional model evaluation (Table 1). The boreal forest site represents a single tower, whereas the tundra site includes three towers representing three different tundra community types.”

8) P8L12-14 Do you think measurement at 5 cm depth is representative of entire soil profile? Your earlier description assumes 0-20 cm litter pools (P6L28-29), so I think this measurement might qualify litter pool temperature not soil temperature.

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Response:

We agree that the soil measurements at 5 cm depth are not able to represent the entire soil profile. But there are very few reliable measurements in the Arctic tundra area, and this site (at Imnavait Creek, AK) is one of the very few sites with good-quality measurements of carbon fluxes, soil temperature and moisture available at the same time. We tried to clarify this in the methods (Page 6, Paragraph 2):

"Therefore, we selected one boreal forest and one tundra site with detailed in situ measurements (including carbon fluxes, soil temperature and soil moisture) for additional model evaluation (Table 1). The boreal forest site represents a single tower, whereas the tundra site includes three towers representing three different tundra community types." In Section 2.1, we indicated that the three litterfall SOC pools were distributed within the top 20 cm of the soil layers (Section 2.1). However, we did not assume that the top 20cm are purely litter layers.

9) P15L12 Mishra and Riley (2014) reported observation based high resolution ALD estimates for entire Alaska. This data might be useful to compare with your model results.

Mishra U., and W.J. Riley. 2014. Active-layer thickness across Alaska: comparing observation-based estimates with CMIP5 earth system model predictions. Soil Science Society of America Journal, 78:894-902.

Response:

Thanks for this reference. This dataset provides a good spatial map of ALT and we cited this reference. Like all other coarse-resolution models, our model is not able to adequately represent discontinuous and sporadic permafrost areas mainly due to spatial heterogeneity; therefore, our modeling results indicate maximum thawing depths greater than 3m for most of southern Alaska, and non-existence of surface permafrost. This is also shown in the model comparisons in Mishra and Riley (2014). Therefore, we

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did not compare our results with the ALT map available from this reference; however, we are using these data in a follow-on model sensitivity study to evaluate impacts of sub-grid spatial heterogeneity in active layer properties, and the impact of coarser scale model simplifications of these properties on estimated carbon fluxes. These results are being used to improve parameterization and representation of these processes in global carbon models.

10) P18L23-28. Can you support your statement with appropriate citation? This statement is saying snow depth causes more soil respiration than surface air temperature?

Response:

Our results indicate both deep snow pack and warming air temperature generally increase soil respiration (Fig 6), but they have different effects on the ratio of soil respiration from the surface and deeper soil layers. Deep snow pack has a stronger warming effect on the deeper soil layers and therefore increases the soil respiration more from the deeper soil layer than from the surface layer. This was partially supported by a snow addition experiment in Nowinski et al. (2010), which indicated that deep snow treatment increases deep and old soil carbon decomposition in permafrost soils at an Arctic tundra site. This has been addressed in the discussion section of the revised manuscript (Section 4.2, Page 19, Paragraph 1): "This is also supported by a snow addition experiment in Alaska tundra areas (Nowinski et al., 2010), which showed that a deeper snow treatment resulted in a larger contribution of deep and old soil carbon decomposition to total soil heterotrophic respiration. "

11) Table 2 and Figure 4,5,6. I prefer validation results presented as R2 values, as its functionally related. Is it possible to convert R to R2 values?

Response:

We changed the R value to R2 values for Table 2. However, for Figures 4-6, we chose to keep using the R value due to the necessity of differing negative and positive

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correlations between climate variables and soil temperature or carbon fluxes in the analysis.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C5641/2015/bgd-12-C5641-2015-supplement.pdf

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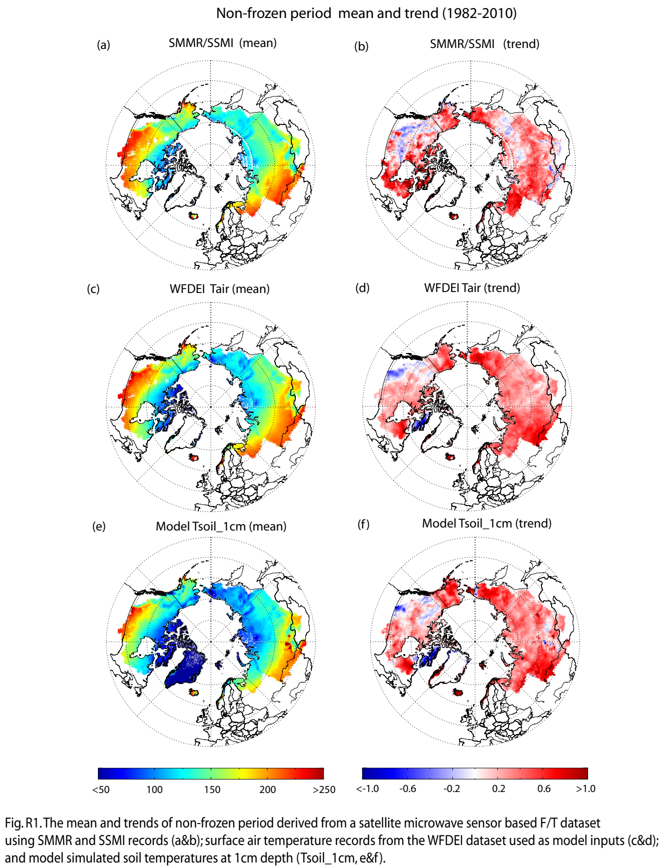

Fig. 1. Fig. R1. The mean and trends of non-frozen period derived from a satellite microwave sensor based F/T record and model simulations