Reviewer comments

Author response, ***Changes made in manuscript***

Interactive comment on “Variability of the Surface Energy Balance in Permafrost Underlain Boreal Forest” by Simone Maria Stuenzi et al.

Anonymous Referee #1

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Stuenzi et al. use measurements and modelling of surface energy balance processes for a boreal forest and grassland site in Siberia to explore the impact of vegetation characteristics on ground thermal dynamics and snow cover. They couple a 1-D land surface model to a multilayer canopy model to account for radiative transfer through the canopy and for soil thaw dynamics. They find that the forest canopy efficiently reduces solar radiation at the forest floor causing slower soil warming and thaw and delayed snow melt.

The manuscript aims to better describe how vegetation interacts with soil thermal regimes in the continuous permafrost zone of Siberia. The authors provide an important and detailed description of the most relevant vegetation-ground interactions, which can help to better understand permafrost responses in a warming climate. One concern is that no radiation, snow, or Bowen ratio measurements were available at the forest site. I know of the difficulties of setting up long-term observations in such environments, but the performance of the model simulations for the forest site - which is the major focus of this paper – cannot be properly assessed without these observations.

The authors discuss this shortcoming in the discussion, but – in my opinion – they need to justify better then why the modelling results should be trusted. The modelled forest GST seem to reasonably fit the measured GST, but modelled soil thaw is delayed compared to observations. This raises the question if the modelling results regarding snow phenology are meaningful.

The manuscript provides a good overview and description of the relevant surface energy balance processes, but the authors could highlight how their study advances our understanding of surface energy balance processes in the forested permafrost zone. Bonan and Shugart (1989) outlined many of the relevant interactions and, for example, Chasmer et al. (2011, DOI: 10.1002/ppp.724) present results on forest canopy effects on radiative processes in a permafrost environment.

We thank the reviewer for this positive overall evaluation of our study and for taking the time to review our manuscript. We have worked through all of the posed questions and suggestions made by the reviewer, which has improved our manuscript. Please note that any changes and additions to the text that we propose for the revised manuscript are highlighted in ***bold****.*

With this manuscript we, indeed, aim at improving our understanding of how the vegetation interacts with the solid thermal regime in the *continuous permafrost zone of eastern Siberia. We would like to thank the reviewer for acknowledging the importance of this detailed description of some of the most relevant vegetation-ground interactions in understanding permafrost responses to a warming climate.*

Indeed, setting up such long-term measurements in these environments is highly challenging, but we agree that the model performance cannot be assessed completely without further integration of measurements. The model does successfully reproduce the measured ground surface temperatures (GST) of the monitored year (August 2018 - August 2019). Nevertheless, GST does not provide a full picture of the surface energy balance. Therefore, we suggest to further validate the model performance with additional measurements. Acquisition of sub-canopy radiation data is, as recognized by the reviewer, highly challenging and was unavailable within the scope of the fieldwork underlying the presented study. Since not many monitoring sites exist around Siberia, we suggest using existing and available data from the rather near, well-documented and well-studied research site Spasskaya-Pad at 62°14’N, 129°37’E. Through the Arctic Data Archive system (ADS) we have been provided meteorological and radiation data from beneath and above the larch-dominated forest canopy for 2018. This data can be used for additional model validation. We suggest adding this additional model validation to the appendix of our manuscript since it is a rather technical aspect, which is not directly related to our major study site.

Preliminary paragraph added to the Appendix: ***“For further validation of the model performance we use existing and available data from the rather near, well-documented and well-studied research site Spasskaya-Pad at 62°14’N, 129°37’E. Through the Arctic Data Archive system (ADS) we have been provided meteorological and radiation data from beneath and above the larch-dominated forest canopy for 2018. Therefore, we have set-up and ran a 5-year simulation for this study site, using ERA-interim forcing data for the coordinate above and a summer LAI of 3.66 m^2m^{-2} (following the measurement-based LAI in Ohta et al. 2001) and a tree height of 18 m. Since the study site is larch-dominated we have now implemented a simple leaf-off parameterisation which is used here. Winter LAI is set to 1.66 m^2m^{-2} (again based on Ohta et al. 2001) for the leaf-off period from 10. October - 10. April.”***

A detailed analysis of this validation run will be presented in the following revised manuscript. However, preliminary analyses of simulation results show a good fit with the modeled surface energy balance and justify the use of the model in the current version.

We further thank the reviewer for the suggested literature such as Bonan and Shugart (1989) who outlined relevant interactions and Chasmer et al. (2011) that present results on forest canopy effects on radiative processes in a permafrost environment. Bonan et al. (1989) is one of our main sources providing the overall framework of this study and is discussed on p.2, l.14 and on p.18, l.7 and l.10.

We have now carefully studied the article by Chasmer et al. (2011) which does present very interesting results for interaction *processes in a discontinuous permafrost zone in Canada. From this study we have learned that vegetation on the edges of permafrost plateaus tends towards reduced fractional canopy cover (by up to 50%) and reduced canopy heights (by 16–30%). The reduced biomass can cause a positive feedback because of lower canopy shading (up to 1h per day less), which leads to an increase in incident radiation at the ground (+16% at open sites) and higher longwave radiation losses (+74% at open plateau sites). We will incorporate this important reference in the following sentence in the introduction (p.2, l.15):* ***"Changing climatic conditions can promote an increasing active layer depth or trigger the partial disappearance of the near surface permafrost. Further, extensive ecosystem shifts such as a change in composition, density or the distribution of vegetation (Holtmeier and Broll, 2005; Pearson et al., 2013; Gauthier et al., 2015; Kruse et al., 2016; Ju and Masek, 2016) and resulting changes to the below- and within-canopy radiation fluxes (Chasmer et al., 2011) have already been reported."***

Other comments

Page 2, Line 19: In some cases, permafrost thaw and forest loss can also lead to increased

CO2 uptake as shown for thawing ice-rich permafrost in northwestern Canada and Alaska.

We agree with the importance of this finding and suggest rearranging the sentence to the following (p. 2, l. 19): "Changes to the vegetation - permafrost dynamics can have a potentially high impact on the numerous feedback mechanisms between the two ecosystem components. ***Increased soil carbon release from thawing permafrost through the delivery of soil organic matter to the active carbon cycle (Schneider Von Deimling et al., 2012) is modified by vegetation changes, which can compensate for carbon losses due to an increased CO_2 uptake (as observed at ice-rich permafrost sites in northwestern Canada and Alaska, Estop-Aragonés et al., 2018) or even further accelerate total carbon loss (Romanovsky et al., 2017)"***

Page 3, Line 25: By how much has the summer precipitation decreased?

We agree that this is an important question and we propose to add the following information (p.3, l.25): ***"Annual precipitation showed an increasing trend from 1900 until 1990, mainly due to an increase in wintertime precipitation. Between 1995 and 2002, summertime precipitation has decreased by -16.9 mm in August and -4.2 mm in July (see table 1 in Hayasaka (2011) for further details)."***

Page 5, Line 20: How was tree height estimated? Are there any ground-based LAI measurements in similar forest types?

We thank the reviewer for this question and have added the following information to the manuscript (p.5, l.20): ***"In a vegetation survey along a 150 m transect from the grassland into the forest, the tree height of every tree within a 2 m distance was estimated. Trees <2 m were measured with a measuring tape, trees >2 m were measured with a clinometer or visually estimated after repeated comparisons with clinometer measurements."***

Further, we recognize that the information given on LAI estimation on p.10, l.26 is insufficient, therefore we have modified the paragraph to the following, more detailed description: ***"LAI can be estimated from satellite data, calculated from below-canopy light measurements or by harvesting leaves and relating their mass to the the canopy diameter. Ohta et al. (2001) have described the monitored deciduous-needleleaf forest site at Spasskaya Pad research station, which has comparable climate conditions but is larch-dominated. The value of the tree plant area index (PAI), obtained from fish-eye imagery and confirmed by litter fall observations, varied between 3.71 m^2m^{-2} in the foliated season and 1.71 m^2m^{-2} in the leafless season. This value does not include the ground vegetation cover. Further, Chen et al. (2005) compared ground-based LAI measurements to MODIS values at an evergreen-dominated study area (57.3° N, 91.6° E) south-west of the region discussed here, around the city of Krasnoyarsk. The mixed forest consists of spruce, fir, pine and some occasional hardwood species (birch and aspen). They find LAI values between 2 m^2m^{-2} and 7 m^2m^{-2}. To assess the LAI we use data from literature*** **and the experience from the repeated field work at the described site*****. Following Kobayashi et al. (2010) who conducted an extensive study using satellite data, the average LAI for our forest type is set to 4 m^2m^{-2} and stem area index (SAI) is set to 0.05 m^2m^{-2}, resulting in a plant area index (PAI)of 4.05 m^2m^{-2} and 9 vegetation layers for model simulations."***

Table 1: Perhaps, the equations in Table 1 could be shown in the table itself or at least qualitatively described. As it is now, the content of the table is not easy to grasp.

We agree with the reviewer that table 1 has little additional value. ***We have added Eq. 2 and Eq. 4 to the table directly.***

Page 6, Line 4: Why did the authors choose the PFT "deciduous needleleaf forest"? They mention that the site is dominated by Picea obovata (92%). Wouldn't an evergreen needleleaf parameterisation be more adequate? Also, if using the PFT "deciduous needleleaf forest", wouldn't it be necessary to include a phenology module? This is partly discussed later in the manuscript, but should be already mentioned here.

We thank the reviewer for this remark. The PFT evergreen needleleaf (NET) is used for the simulations. ***This has been corrected in table A5 and on page 6.*** *As discussed later (p.21, l.14), the development and implementation of a phenology module was out of scope for this study, mainly because of the little presence of deciduous taxa at the chosen study site.* ***For a more detailed study of larch-dominated forest ecosystems, a simple phenology module has been implemented for the additional validation site at Spasskaya Pad where a much higher amount of deciduous taxa is present. This consists of a simple winter leaf-off parameterisation and results in a lower winter LAI (based on measurements from Ohta et al. 2001) and a leaf-off period from 10. October - 10. April (see the preliminary paragraph above describing the Spasskaya Pad validation site).***

Page 8, line 8: How was soil thermal conductivity parameterised?

We agree that this information should be added to our manuscript and have done so on p.8, l.10: ***"Soil thermal conductivity is parameterised following Westermann et al. (2013 and 2016) and is based on the parameterization in Consenza et al. (2003). The thermal***

conductivity of the soil is calculated as weighted power mean from the conductivities and volumetric fractions of the soil constituents water, ice, air, mineral and organic.”

Following Westermann et al. (2013, 2016) the according equation describes the soil thermal conductivity (k)

$$k = \left(\sum_{\alpha} \Theta_{\alpha} \sqrt{k_{\alpha}} \right)^2$$

with the volumetric fractions (θα) of water, ice, air, mineral and organic. This parameterization has been used as standard in CryoGrid for a number of publications (i.e. Nitzbon et al. 2019, 2020). The temperature-dependence of the thermal conductivity, which gives rise to the thermal offset between ground surface and permafrost temperatures (Osterkamp and Romanovsky, 1999), is contained in the temperature-dependent water and ice contents as detailed in Sect. 2.2 in Westermann et al. (2013). This parameterization above is chosen for simplicity (e.g., de Vries, 1952; Farouki, 1981 describe other parameterizations), as reliable recommendations for a particular conductivity model are lacking for permafrost areas and as our chosen parameterization allows us to successfully reproduce observed annual freeze and thaw cycles at permafrost sites under differing environmental conditions (Westermann et al., 2013).

Figure 3: What are the error bars showing? What is the input to calculate error bars (hourly, daily, weekly data)? It seems as if the variability (i.e. error bars) for the observed grassland turbulent energy fluxes is much larger than the modelled variability.

*We agree with the reviewer that this has not been made clear. Whiskers in modeled data show the standard deviation based on daily averaged data. Further, the whiskers in the measured data at the grassland site were based on half-hourly data. We thank the reviewer for pointing this out and **have adapted this for the measured data at the grassland site to be based on daily values as well**. This provides more comparable variability values. We modify the figure caption accordingly to explain what the whiskers show: **“Surface energy balance for snow-covered (28.10.2018-27.04.2019) and snow-free (10.10.2019-27.10.2019 and 28.04.2019- 10.10.2019) periods at the ground surface of grassland and forest and at the top of the canopy of forest (Forest TOC). Shown are the net radiation (Qnet), sensible (Qh), latent (Qe) and storage heat flux (Qs) for the model runs of the forest and grassland site as well as the measured values at the grassland site. The bars indicate mean values while the whiskers show the corresponding standard deviations.”***

Table 2: Is this the same data as shown in Figure 5? If so, the authors could think about showing only one of them (figure or table).

*We agree with the reviewer that this is indeed a repetition; therefore, **the values shown in Table 2 are added to Figure 5 directly. Table 2 has been removed.***

Figure 6: Most of the model-observation comparison is of qualitative nature. The authors could add some performance metrics (e.g., RMSE, R2, bias: : :)

We thank the reviewer for this suggestion and agree that performance metrics would be helpful to evaluate the model performance. ***We will add performance metrics such as R^2 values to describe the significance of the differences between measurements and model outcomes as well as between the forest and grassland sites to figure 4, 5 and 6.***

Page 18, line 12: The authors report a bias in modelled GST during the winter. Since GST measurements are available, the forest-grassland comparison could be more meaningful if it were based on observational data.

These numbers are provided in Figure 5 (and formerly Table 2). Based on this suggestion the measured differences will now also be provided in the text (p. 18, l. 10): ***"Our results are in agreement with these observations, but further demonstrate that the impact of mixed boreal forest on the GST is strongest during the snow period and the summer peak with the warmest months. Our model reveals an average of 6.5°C higher GST during the snow-covered period and 1.5°C lower GST during the snow-free period. Measurements reveal an average of 2°C higher GST during the snow-covered period and 2.3°C lower GST during snow-free periods."***

Page 22, line 17: Could lateral flow of water contribute to differences in ground water content?

We would like to thank the reviewer for this important question. Indeed lateral water flow could contribute to the differences in actual measured ground water content. This process is neglected in this baseline, one-dimensional model set-up where we try to investigate the influence of forest on the surface energy balance of the ground. Recently, Nitzbon et al. (2019) have integrated lateral fluxes of heat, water and snow in the CryoGrid scheme. Higher forested ground water content was measured in point measurements taken in 2018, but no year-long ground water content measurements are available for our forest site, therefore this is not discussed in detail within this study. We add this information to p.10, l.12: ***"Lateral water fluxes are neglected in this baseline, one-dimensional model set-up."*** *We further add the following statement to the discussion (p. 22, l.7):* ***"Further, one aspect not represented in the model is the moisture transport and migration in frozen ground or the forming of ice lenses. Lateral water flow and snow redistribution may be important processes to be investigated in the future since they can strongly modify the thermal regime."***

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