

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

Anonymous Referee #2

Received and published: 19 August 2020

Comments on “Variability of the surface energy balance in permafrost underlain boreal forest” submitted by Simone Maria Stuenzi et al. to Biogeosciences

In this manuscript, the authors developed a biophysical atmosphere-land interaction model (CryoGrid) and examined its performance by comparing with observations at a mixed forest in East Siberia. By coupling with a multi-layer canopy scheme (CLM-ml v0), they accounted for the effects of vegetation canopy on surface processes. They conducted a series of simulations with different land covers (forest and grassland) and different leaf area index or canopy density. They found that coverage with high leaf area index affects surface energy budget, such as solar radiation transfer and insulation, and that it could play important roles in snow and permafrost dynamics such as active layer thickness.

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I agree that terrestrial ecosystems in northern high latitude is remarkable in terms of climate change, especially permafrost thawing that is thought as one of the tipping elements of the Earth system. Then, developing advanced models simulating boreal ecosystem processes is highly important. Nevertheless, I could not find out what is the original contribution of this study. For example, the model developed in this study seems similar to previous models such as CLM, ORHIDEE, LPJ etc. These previous models have already implemented leaf phenology and dynamic vegetation; at least some versions include permafrost dynamics.

Another concern on this study is that the authors used a very limited amount of observations. Especially, they validated the model performance to capture temporal variability only for surface temperature. Because many flux measurement sites are operating and providing a variety of observational data, I highly recommend validating the model by using a larger number of biophysical variables including energy and water fluxes. Also, I have a concern about the overly simplified simulation conditions, such as the lack of seasonal change in leaf area index in forest. Though in situ data were not available, at present, we can obtain time-series of leaf area index from satellite remote sensing as a good proxy.

Based on the inadequacy of scientific insights and model simulations, I cannot recommend this manuscript to accept for publication in the present form. The manuscript is too descriptive and needs more model validation with observations. Also, the authors need to devise model simulations to make insightful discussions.

Minor points

Page 3 Section 2.1: Can you give information on vegetation conditions, such as leaf area index and tree density?

Page 7 Line 3: I am not sure how within-canopy wind profile was parameterized and simulated by the multi-layer canopy model. Can you explain briefly, because this is an important feature of multi-layer canopy models?

Page 7 Line 7: Does the snow module include water loss by sublimation in winter? Under frigid environment like east Siberia, sublimation may not be negligible for snow water budget. Page 13 Figure 3: Can you give units to y-axis? Maybe, in $W m^{-2}$.

Page 15 Table 2: Can you give other statistic metrics such as root-mean square error between measured and modeled values?

Page 17 Figure 7: Even in forest site, it is difficult for me to imagine 1m snow depth in eastern Siberia, where typical annual precipitation is 200–300 mm. Can you give observed annual precipitation at the study site?

Page 20 Section 4.3: This section looks to be a summary of Bonan and Shugart (1989). What is the key message of the section derived from the present study?

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-201>, 2020.

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