The Cryosphere Discuss.,
doi:10.5194/tc-2016-210-RC3, 2016

The Cryosphere
Discussions
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Interactive comment on "Effects of variability of meteorological measures on soil temperature in permafrost regions" *by* Christian Beer et al.

Anonymous Referee #3

Received and published: 21 October 2016

Reviewed paper concerns important topic of models sensitivity to input parameters. I agree with referee #1, that the paper may be more suitable for Geoscientific Model Development journal. In this research authors investigate how results of climatic models applied to ground temperature dynamics depend on forcing variability. Based on model experiments authors notice increasing of warming effect of snow and surface vegetation on ground temperature due to reduction of climate forcing variability. But if for snow cover mechanisms causing such changes were explained for vegetation cover these changes were just stated with very basic notice of water/ice content affecting moss/lichen thermal diffusivity. Investigating vegetation cover authors limit themselves with mosses and lichens as a type of vegetation has strongest influence on ground surface energy balance. But, despite of the fact that this type of surface vegetation spreads wide neglecting of other ecotypes such as grass- and shrublands, deciduous forest etc

does not allow to apply results of this study for entire northern hemisphere. Combination of lichens and bryophytes in one group was done, as far as I can understand based on similarity of its thermal properties, which is also needs to be better justified. But this approach is appropriate only for models cover short period of time. For long-term estimations differences in physiology of these plants types cannot be ignored. So due to higher productivity mosses form thick soil organic layer composed with fibrous or peat, while lichens usually associated with bare soil or very thin organic layer. Results of modelling for site located at 70N and 100E are absolutely unreal! In according to Permafrost map of Russia (http://nsidc.org/data/docs/fgdc/ggd600_russia_pf_maps/) compiled based on data collected during 1960-1980 permafrost temperature at this point was -7 to -8C and could not get colder since that time, so temperature at the depth of 38 m in 2020 cannot be so low (-13.8) as it is shown on the Figure 8d. Such a weird results make all other model outputs very doubtful. It is also not clear why authors choose this location as a reference point. Few more minor comments: Lines 200-203. It might be better to state pattern of decreasing of differences in standard deviation of variability by latitudinal zonality of diurnal temperature oscillations. Daily temperature variability in high latitudes is lower because of daylight time is extremely short during winter and extremely long during summer. Lines 288-289. I would recommend to authors be more specific and give some values of lichens/mosses thermal diffusivity obtained from their model. Lines 321-322. So low values of soil temperature increase are typical for sites where permafrost temperature now is close to the freezing point. In according to data of long-term measurements (Romanovsky, V. E., Smith, S. L., & Christiansen, H. H. (2010). Permafrost thermal state in the polar Northern Hemisphere during the international polar year 2007–2009: a synthesis. Permafrost and Periglacial Processes, 21(2), 106-116.) real permafrost temperature trends are in a range 0.05 to 0.1 K per year. Based on all abovementioned comments I cannot recommend this paper for publication as it is now. My suggestions for authors are: 1. Give better and more comprehensive description of model instead of reference to previous publication. Clarify what is a role of vegetation layer: is it a part of computation domain or boundary condition. 2. For long-term computations differences in soil organic layer formation under mosses and lichens must be taken in consideration. 3. Site-specific approach looks more appropriate for such kind of research than regional. I would recommend use database of Global Terrestrial Network – Permafrost program (http://gtnpdatabase.org/boreholes) to select sites for model validation.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-210, 2016.