The Cryosphere Discuss., 6, C2876–C2879, 2013
www.the-cryosphere-discuss.net/6/C2876/2013/

The Cryosphere Discussions

Interactive comment on "Modelling and mapping climate change impacts on permafrost at high spatial resolution for a region with complex terrain" *by* Y. Zhang et al.

Anonymous Referee #1

Received and published: 29 January 2013

General Comments

This manuscript presented a modeling study with very high resolution (10 m) to investigate the effects of climate change on permafrost of Ivvavik National Park of Canada. Large amount of work has been done to compile very high resolution input data, including both dynamic, e.g. climate driving, and static data, e.g. vegetation and drainage. The method used in this manuscript is efficient, and can be applied to other key permafrost regions. This work is within the scope of The Cryosphere; the results are well presented. I recommend publication of this manuscript in The Cryosphere, if the following several major comments are properly dealt with.

Major Comments

1) The authors stated that previous modeling work usually uses very coarse resolution, e.g. half degree, 2km, and 30m, which cannot accurately consider the effects of topography. In this manuscript, the authors only presented the work at 10 m scale. To convince the audiences, I suggest the authors to further simulate the changes of permafrost at the resolution of 30 m, 2 km, and half-degree, using averaged vegetation and drainage data at corresponding scale, and compare the outputs among different spatial resolutions to show the advantage of 10 m work.

2) The authors also stated that high resolution study can be used for land management. One of the hazards of permafrost degradation is ground subsidence on areas with thick ground ice. The manuscript has included the spatial heterogeneity of vegetation, topography and soil. I suggest the authors to consider the heterogeneity of ground ice if possible.

3) Emergency of shrub on the tundra is not unusually (Bonfils et al., 2012); and may be a very common phenomenon over the next 100 years. As indicated in the manuscript, ground conditions are related to vegetation types, which are the main controls of active layer depth. The NEST model is a land surface model, which cannot simulate the succession of vegetation. However, sensitivity tests can be performed to compare the differences of active layer depth between tundra and shrub over the next 100 years.

4) I would suggest the authors to split discussion and conclusions into two sections. Lots of work has been done to run simulation. However, too little effort has been spent on discussion.

Minor Comments

Abstract

P4600, L5, "are difficult to use for" -> "are difficult to be used for"

Methods and Data

P4604, L16-20. I cannot understand the logic. P4606, L17. The clustering method used to improve computing efficiency in this study has already been used in other studies. For example, Balshi et al. (2007) used similar method, called cohort, to study effects of wildfire. I suggest the authors to provide some text to introduce existing work.

Results

P4613, L8. Define talik

The differences of simulated active layer depths among different spruces, i.e. dry (type 7), mesic(type 8) and wet (type 9) slope, were very small (less than 30 cm). This is different from field experience. In some place of Alaska, the south slope (dry) has no permafrost; but north slope (wet) has permafrost. The small differences might be originated from the peat thicknesses used in Table 3, which were 0, 10, and 10 cm for dry, mesic and wet spruce, respectively. However, in Yi et al. (2009), the differences of organic soil thicknesses are large among black spruce with different drainages.

P4614 L4-7. The authors mentioned that "The correlation coefficient between the modeled and observed summer thaw depth is low, ..., mainly due to variation of ground conditions within an ecosystem type. The authors suggested that "more spatially detailed maps for ground conditions are need for high resolution mapping of permafrost". Whether these maps for ground conditions be possibly created at such a high spatial resolution?

Discussion and conclusions

P4616 L10-15. mention the drawbacks of modeling is good. I have two suggestions. One is to put this part in discussion part; the other is to add vegetation successions in the discussion part too.

References

P4621, L19. Missing period at the end.

Appendix

The effect of topography on the downward solar radiation is an important topic of this manuscript. It is good that the authors provided descriptions on how to calculate it. I would suggest the authors to provide source code as supplementary material. Those who are interested in studying the effect of topography may take advantage of the code.

Define TDD in table 1, INP in table 2

Table 3, what is the use of soil moisture classes in the NEST? Initialization?

Papers cited:

Balshi, M. S., A. D. McGuire, Q. Zhuang, J. Melillo, D. Kicklighter, E. Kasischke, C. Wirth, M. Flannigan, J. Harden, J. S. Clein, T. J. Burnside, J. McAllister, W. Kurz, M. Apps, and A. Shvidenko (2007), The role of historical fire disturbance in the carbon dynamics of the pan-boreal region: A process-based analysis, J. Geophys. Res., 112, G02029, doi:10.1029/2006JG000380.

Bonfils, C. J. W., T. J. Phillips, D. M. Lawrence, P. Cameron-Smith, W. J. Riley, and Z. M. Subin (2012), On the influence of shrub height and expansion on northern high latitude climate, Environ. Res. Lett., 7, 015503, doi:10.1088/1748-9326/7/1/015503.

Yi, S., K. L. Manies, J. Harden, and A. D. McGuire (2009), The characteristics of organic soil in black spruce forests: Implications for the application of land surface and ecosystem models in cold regions, Geophys. Res. Lett., 36, L05501, doi:10.1029/2008GL037014.

Interactive comment on The Cryosphere Discuss., 6, 4599, 2012.