Detailed responses to the comments of the reviewers

(Reviewers' comments are in Times New Roman font, and the responses are in Arial font)

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.

Response: To re-run the model at resolutions of 30m, 2km and half-degree will take lots of time and efforts and the results do not fit very well within the current paper, which is already quite long. To better show the results at high spatial resolution, we revised a panel in Figure 4 by enlarging a small area to show the spatial details. The distribution of active-layer thickness in the 1970s was replaced since it is similar to that of the 2000s although shallower.

To show the effects of spatial resolution, we prepared the following figure of elevation at spatial resolutions of 30 m, 1 km, 2 km, 10 km and 20 km (The resolution of half degree in our study area (about 69 ^{o}N) is equivalent to a grid cell of about 20 km by 55 km). Slope and aspect calculated based these elevation maps will be very different at different resolution, so as the solar radiation calculated from them.

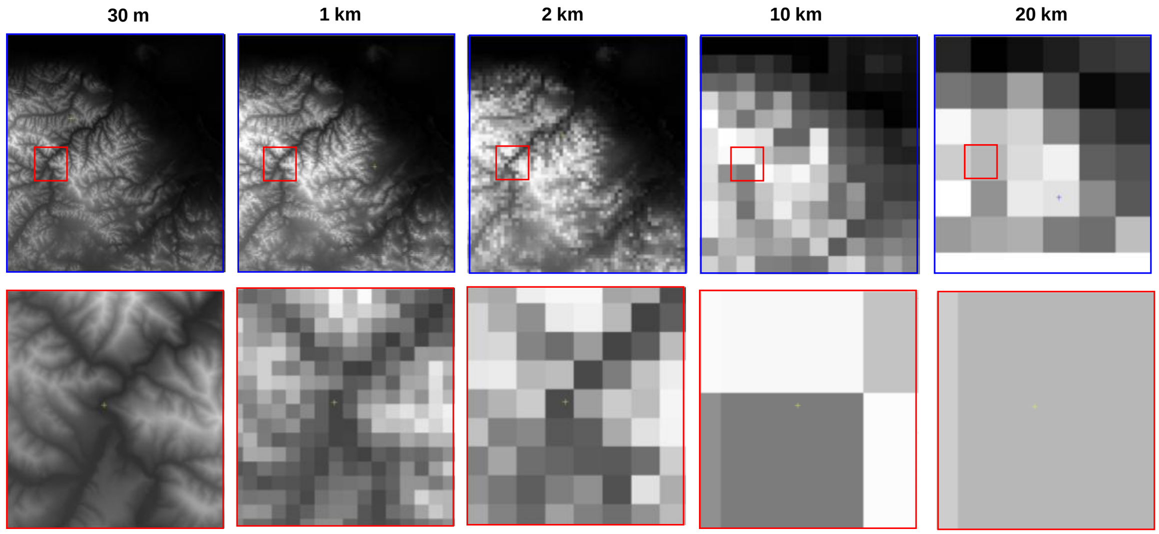

Figure 1. The effects of spatial resolution on elevation. The panels in the top cover the entire park, and the panels in the bottom are for the same red square area. Coarser resolutions are re-sampled from the 30 m resolution map using bi-linear interpolation method.

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.
Response: Agree with the point that ground ice is very important for the effects of permafrost thaw. However, there is no much information about the ground ice in this area, so we cannot include it in the modelling.

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.
Response: We did some sensitivity tests by changing leaf area index (LAI) and the snow drifting factor. Increasing LAI without changing the snow drifting factor will decrease active-layer thickness due to shading effects. Reducing snow drifting or even capturing some snow from the surroundings will cause faster thawing from the bottom of permafrost. Its effects on active-layer thickness depend on the stages of permafrost degradation, as defined by Zhang (Journal of Geophysical Research (Earth Surface), accepted on Feb. 11, 2013). When permafrost is in gradual-thawing stage, changes in snow drifting condition have little effects on active-layer thickness. When permafrost is in increased-thawing stage or taliks developed, reducing snow drifting (i.e.d, having more snow

accumulated) will increase summer thaw depth. Temporal changes in snow drifting is usually associated with changes in vegetation conditions. Therefore the real effects of vegetation change on permafrost depend on their combination effects and degradation stages of the permafrost. We added some sentences about these results in the revised manuscript.

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.
Response: Agree and revised accordingly.

Minor Comments
Abstract
P4600, L5, "are difficult to use for" -> "are difficult to be used for"
Response: Revised as "the results are not suitable for land-use planning and ecological assessment."

Methods and Data
P4604, L16-20. I cannot understand the logic.
Response: Agree and we deleted the latter part of the sentence.

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.
Response: The method used by Balshi et al. (2007) is somewhat similar but not as explicit as we used in this study. They used a spatial resolution of 0.5 degree for fire disturbance simulation. Since there are different fire events throughout the time within a 0.5 degree grid cell, they simulated each unique fire history within a grid cell. Their purpose is to consider the unique features of the fire history, but our purpose is for computation efficiency.

Results
P4613, L8. Define talik
Response: Added a sentence to explain "talik" in the first paragraph in section 3.2.

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.

Response: Agree that organic layer thickness has significant effects on active-layer thickness and permafrost. Similar to the suggestions by Yi et al. (2009), we considered three spruce ecosystem types, which include information about their different drainage conditions, as shown in Table 3. We used the same peat thickness for spruce-birch mesic slope (Type 8) and spruce-horsetail wet slope (type 9) based on field observations (Table 3). As the reviewer indicated, this treatment probably is the major reason that the simulated active-layer thickness is similar between these two types. Active-layer thickness in dry slope (type 7) is deep than in other spruce types but permafrost still persists, because it is colder than you mentioned Alaska sites.

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?
Response: Mapping ground conditions at high spatial resolution is still a big challenge. One direction is to use satellite data of different sensors to map some selected area with the support of intensive field observations. Another approach is to develop new methods to consider the uncertainties.

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.
Response: Agree. We put this part into the discussion section (separated from the conclusion section) and included some sensitivity test results about the effects of vegetation change.

References
P4621, L19. Missing period at the end.
Response: Done.

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.
Response: Agree and we are willing to release the source code as supplementary material.

Define TDD in table 1, INP in table 2
Response: An explanation of TDD was added in Table 1, and a full name for INP was used in Table 2.

Table 3, what is the use of soil moisture classes in the NEST? Initialization?
Response: Soil moisture class was used to estimate the lowest water table depth for lateral outflow. This is explained in the second paragraph in section 2.3.4.

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.

Anonymous Referee #2

Received and published: 30 January 2013

General comments
The aforementioned manuscript by Zhang et al. demonstrates the use of numerical process based modelling to calculate and investigate two important permafrost variables including distribution and active layer thickness (ALT) in an Arctic area with complex topography. This paper is of particular interest to the community of The Cryosphere because it investigates permafrost conditions spatially with the effects of transient climate change whereas many only look at equilibrium conditions. There are several novel aspects of this study and I believe the authors have done an excellent job acquiring good data to use from many sources. I believe this study fits both the scope of the journal and illustrates a technique, which is transferable to other areas meriting publication upon the following issues being addressed or cleared up.

I am concerned about the resolution of this study (10 x 10 m), I understand that the remote sensed ecosystem classification is at this resolution however, the DEM is not. The DEM is taken from Canadian National Topographic system maps, which only have a resolution of 30 x 30 m. I do not see how the authors were able to resample this to a finer resolution. Please provide a rational as to how this resample was done, without addition elevation data or justification the study would have to be completed at 30 x 30 resolution which is sill very fine for this area.

Response: We re-sampled the 30m resolution DEM to 10m so that we can take advantage of the 10 m resolution ecosystem map developed from SPOT imagery. At 10 m resolution, the model results are easier to match the sites of the field observations, since ecosystems vary in a short distance in this region of complex terrain. We added a sentence in the second paragraph in section 2.3.2 to indicate the purpose of the re-sampling.

I feel the title of this paper would benefit from the addition of "Arctic" this is because of the different type of permafrost research which is done in mountainous areas (e.g. Apline Europe) apposed to that which is done in High Arctic areas (e.g. Canadian High Arctic Islands). This study area is unique because it encapsulates some aspects of both systems and thus should be mentioned in the title. In addition I feel that not enough is done to speak about this uniqueness in the study area section, there is no mention of elevation ranges as an example.

Response: Agree. We added "Arctic" in the title and some related parts in the manuscript. We also added several sentences in the first paragraph of the section 2.1 about the unique land and ecological features of this area.

Another aspect of the methods that needs to be addressed is the interpolation of Mean Annual Air Temperatures. Almost all Environment Canada stations are located in valley bottoms or areas of low elevation and as a result it is difficult to predict temperature change with elevation. Many models use a standard Surface Lapse Rate (SLR) of -6.5^{o}C/km which on a yearly scale has been shown to be inaccurate (See Lewkowicz and Bonnaventure, 2011). What SLR was used to make this model? The justification for this should be included in the discussion section of this paper.

Response: Climate data were derived directly using the method and software from Wang et al. (Journal of Climatology, 26, 383-397, 2006). Based on this method, monthly climate was interpolated using station data and elevation. Temperature lapse rates are different for different months and different locations. The following figure is some results I checked for an area in different months. Temperature decreases with elevation in summer months but shows inversion in winter months. This pattern is in agreement with observations qualitatively (Canadian Parks Service, Northern Yukon National Park resource description and analysis, 1993). However, the sparse and relatively short climate records in Arctic regions are a source of uncertainty. We added a sentence in the discussion section about this data gap.

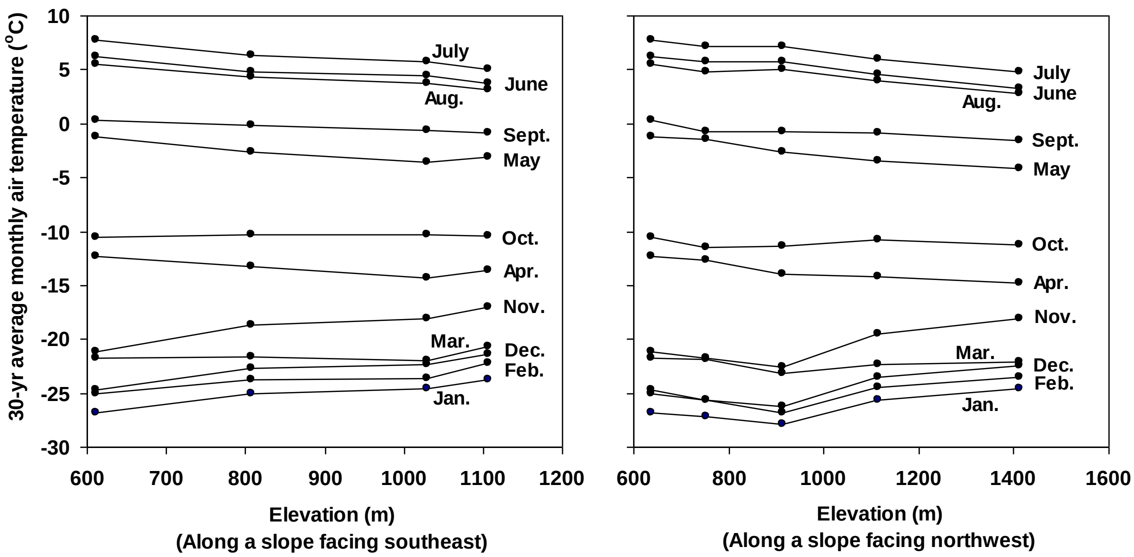

Figure 2. Changes in 30-year average monthly air temperature with elevation from a valley site (69.27 ^{o}N, 140.87 ^{o}W) to southeast and northwest slopes.

I feel that the Discussion and conclusions section should be split into two sections. I feel as if more should be spoken about in the discussion, which is currently a bit short. One aspect that should be discussed in more details is the errors an uncertainty associated with this modelling as well as the application to different areas in the future. I have to say however, that the use of the Appendix for equations is appropriate.
Response: Agree and we provided discussion and conclusion sections separately. More discussion about uncertainties was added, such as the effects of the year used to initialize the model, climate data, and possible changes in vegetation conditions.

Minor comments
In the introduction Page 4601, line 29, avoid using the word "we".
Response: Corrected.

In the methods section page 4602, line 23, I believe a reference is needed.
Response: The reference Canadian Parks Service (1993) was added.

In figure 1a, consider labeling Herschel Island, as this is an international journal and much permafrost research is conducted there so it provides a good reference.
Response: Added. And we also added two paragraphs to validate the model using the observations from Herschel Island and Richards Island (explained below). Their locations were indicated in Figure 1.

In figure 1b, there is a line extending off the coast into the Beaufort Sea, what is this? For figures 2 and 5 consider using colour categories rather than a ramp as this will make changes easier to see especially in 5c.

Response: The off coast curve in Figure 1b was erased. The maps are displayed in more than 100 colour categories to show the detailed differences. I used ramps to show the legend for simplicity. As for Figure 5c, I revised the colour scale to better show the spatial differences.

References

Lewkowicz A.G. and Bonnaventure P.P. 2011. Equivalent Elevation: a New Method to Incorporate Variable Lapse Rates Into Mountain Permafrost Modelling. Permafrost and Periglacial Processes. 22: 153-162, DOI: 10.1002/ppp.720.

Other revisions

1) Validation

Dr. Christopher Burn kindly suggested to test the results, especially the modeled temporal changes, with his observations conducted in the nearby Herschel Island, and at Illisarvik in Richards Island (69.48 ^{o}N, 134.59 ^{o}W). The related studies have been published in Journal of Geophysical Research (Burn and Zhang, 2009, doi: 10.1029/2008JF001087) and in Permafrost and Periglacial Processes (Burn and Kokelj, 2009. doi: 10.1002/ppp.655), respectively. He also kindly provided their original observations, including the unpublished data from 2009-2012. The observed summer thaw depths are from 1983 to 2012, probably the longest continuous field observations available. Based on these precious data, we added two paragraphs in section 3.4 to compare our model results with his observations.

The first paragraph is to compare the modelled changes in ground temperature with his estimates for the period since the beginning of the 20th century (1899-1905) in Herschel Island. Our modelled change is larger than his estimates (3.4 ^{o}C vs 2.6 ^{o}C at 1m depth, and 2.5 ^{o}C vs 1.9 ^{o}C at 20m depth), probably because the year we selected to initialize the model (1967) has a lower precipitation, therefore shallower snow depth. The observed summer-thaw depth from 2003-2007 compared well with the model results, except in year 2007, in which the observation is deeper.

The second paragraph is to compare the modelled active-layer thickness with the observed summer-thaw depth from 1983 to 2012 measured at Illisarvi in Richards Island. The correlation is significant for this 30-year comparison (R = 0.71), and the deepening rates are similar (deepening 2.3 cm and 2.9 cm per decade for the observations and the model results from 1983 to 2012, respectively). A scatter graph was added for this comparison, including the 4-year observations at Herschel Island.

2) Degradation stages

Zhang (Spatio-temporal features of permafrost thaw projected from long-term high resolution modeling for a region in the Hudson Bay Lowlands in Canada.

Journal of Geophysical Research (Earth Surface), accepted on Feb. 11, 2013) divided permafrost degradation process induced by climate warming into five stages. Using this new measure, our modelling results show progressive degradation stages in Ivvavik National Park although permafrost persisted in most of the park in the 21^{st} century. We added a paragraph (section 3.3) to describe the modelled degradations stages in the park, with two new figures to show the temporal changes and the spatial distributions under the two climate scenarios.