We appreciate the efforts of the two anonymous reviewers and their useful comments and suggestions for improving the manuscript. Their comments are addressed in the revised manuscript and explained in detail below in *italic*.

Reviewer # 1 general comments:

1. It would be clearer if the sub-sections in section 2 (data and methods) can be grouped into two sub-sections (2.1. model and 2.2 data). The model sub-section can include the GIPL model, snow dynamics, and soil thermal parameters, and the data sub-section can include descriptions of model domain and resolution, mineral soil conditions, peat soil conditions, climate data, and model setup, water treatment, etc.
   This is a good suggestion that would improve the structure of the manuscript. However, manuscript preparation guidelines allow only three levels of sections and inserting another section would increase the level to four so that we left the current structure.
2. The climate scenario generated by ECHAM 5 (A1B) is not a bad choice for this study but it would be convincing to provide some explanations. The assessment paper by Walsh et al. (2008) may provide a good support. The grids of ECHAM5 are coarser than half degree latitude/longitude, please explain why and how you down-scale the daily scenario data.
   Walsh, J.E., W.L. Chapman, V. Romanovsky, J.H. Christensen, and M. Stendel (2008)Global climate model performance over Alaska and Greenland. Journal of Climate, 21: 6156-6174
   We added: "Simulated 20th century (20C3M) ECHAM5 air temperatures for the northern regions (20-90 deg N) have been found to have the smallest bias out of all the models used in the IPCC AR4 assessment when compared to the 40 year ECMWF reanalysis (ERA40) data (Walsh et al., 2008) and was therefore chosen for the offline simulations of soil temperatures and permafrost. The original coarser resolution data was interpolated to 0.5 degree grid cells using the NCAR NCL software (NCAR 2011)".
3. I understand and experienced the difficulties to compare modeled grid results with site observations, but Fig. 3 is still not very informative. You can revise it or even delete it since a detailed description has been provided in the text.
   Figure 3 was taken out of the revised manuscript.
4. A table for the permafrost extent was presented (Table 4). I would like to see a similar table for active-layer thickness, which is more directly related to the major conclusion of the paper.
   This is a very good suggestion but we feel that it is difficult to report area averaged active layer depths for a transient simulation with warming climate. As the area classified as permafrost changes over time, averaging active layer depths in space would require setting a threshold depth for permafrost and year to year comparisons would relate to potentially different areas each year. For this reason we report thawed permafrost volumes rather than average active layer depth.
5. Page 182, Lines 14-17: Your model estimated permafrost extents for peatland varies from 34-43% to 72% of the total area of peatland depending on the depth of the ground temperature used for defining permafrost (Table 4). The former is similar to the results from map-overlaying and Tarnocai (2006), but the latter is larger. It would be clearer to indicate

this difference since the depths used to define permafrost have significant impacts as you show in Table 4 and the discussions in the following paragraph (the last paragraph in Page 182).

These differences are addressed in the paragraph starting at line 18.

6. For the discussion of the modeled snow conditions and impacts on permafrost (section 4.1.1), a spatial modeling work for Canada by Zhang et al. (2008) seems relevant. You may check to see the differences and similarities. Zhang, Y., W. Chen, and D.W. Riseborough (2008). Modeling long-term dynamics of snow and their Impacts on permafrost in Canada. Proceedings of the Ninth International Conference on Permafrost. P2055-2060.

   We have included the reference in the discussion of snow cover implications on soil temperatures in section 4.1.1.

Reviewer #1: Some minor comments/suggestions

1. Page 162, Line 20: "of the atmosphere" seems should be "below the land surface"

   Corrected.
2. Page 163, Line 23: add "from Russian permafrost region" at the end of the sentence.

   Corrected.
3. Page 166, Line 7: "1159 million km2 " this number did not equal the number given in Table 4 (1.41+11 = 12.41 million km2). In Table 4, [km3] should be [km2

   Corrected. The total model area domain is 12.41 Million km2.
4. Page 168, Line 6: Change "[Wm—2 K —1] (set to 20.14)" to "(set to 20.14 Wm—2 K —1])", and add the unit m2 K W—1at the end of "snow thermal resistivity". Add the units for Ds and ρ s

   Changed. Unites for Ds and ρ s are given above the equation.
5. Page 176, Line 5: "in the next century" should be "by the end of the century".

   Corrected.
6. Page 177, Line 19: Add "consecutive" between before 24 and months.

   Corrected.
7. Page 178, Line 18: "almost complete decline" should be "almost completely disappeared"

   Corrected.
8. Page 178, Lines 23-26: Add "from 43%" between "decline" and "to". Deleting "next". The sentence "By the end of the century, the peatland area underlain by permafrost at 2m depth will have decreased by one third and only ~200 000 km2 of the total peatland area of 1.4 million km2 will have permafrost at a depth of 2 meters": The numbers "~200000km2, and one third" are based on the 0.5 m (not 2m) depth ground temperature under dry conditions (bogs) according to the Table 4; You may just use relative change since you do not know the actual area of the dry peatlands (bogs)

   The sentence with the absolute numbers was taken out.
9. Page 179, Line 20: "important" means "significant"?

   Corrected.
10. Page 179, Line 24: "661 km3 " seems did not match the number in Table 5 (600 km3).

    This number refers to the increase in thawed volume and is the difference between the volumes in 2091/2100 and 2001/2010.
11. From line 23 in Page 183 to line 15 in Page 184. This part seems fit better in the next

section (section 4.2)
Section was merged into 4.2

12. Page 185, line 5. You may check/cite a study by Hossian et al. (2007) for SOC content in mineral soils in northern Canada. The content seems similar or slightly higher than that from Finland Hossain, M.F., Y. Zhang, W. Chen, J. Wang, and G. Pavlic (2007). Soil organic carbon content in northern Canada: A database of field measurements and its analysis, Canadian Journal of Soil Science, 87, 259-268
Reference was added.
13. Table 5. Since peatlands and uplands are separated land units, I feel Table 5 would be more intuitive by arranging the soil horizons based on uplands and peatlands. The term "mineral soil" some times means upland, and some time means mineral soil horizons. It would be clearer using different terms.
Table 5 was reorganized accordingly into a section for uplands (mineral soil and organic layer) and a section for peatlands (peat and mineral soil below peat).

Reply to reviewer # 2 comments

1. There is a comment on p. 167 that unfrozen soil water at temps below freezing can improve the transfer of heat in frozen soils. I think you mean that it can improve the simulation of heat transfer in frozen soils.
Unfrozen water increases the thermal conductivity of soil with decreasing temperatures and thereby can improve the transfer of heat (e.g. Faurik, 1981). For clarification, we added: "Taking into account frozen water in soil temperature models can therefore significantly improve the simulations".
2. Why did you use the ECHAM5 climate forcing rather than the forcing from another model? I am not criticizing the use of ECHAM5 data. But, if there was a reason for using it, it should be stated. If there wasn't a solid reason then you should simply note that the choice was arbitrary (and that most GCM projections are similar?).
See reply to reviewer #1 comment on the same issue.
3. Figure 3 is very difficult to read. The symbols are not clearly distinguishable and the
See reply to reviewer #1. Figure 3 was taken out of the manuscript.
4. In section 3.3, you note that the large increase in P is partly offset by ET increases. I don't think that the method for calculating ET is noted in the text.
The method was added to the description of the model (Sec 2.2.1): "Potential evapotranspiration in WBM_{plus} was calculated as a function of air temperature and day length using the Hamon relationship (Hamon, 1963)".
5. P. 180. I think that the Lawrence and Slater (2010) study showed that snow changes could explain 50-100% of the changes in soil temperature during the 20th century, not the 21st century.
Corrected.
6. P. 182. I think you mean Brown et al. 2000 for the permafrost classification.
Corrected.