

Interactive comment on “Wind driven snow conditions control the occurrence of contemporary marginal mountain permafrost in the Chic-Chocs Mountains, south-eastern Canada – a case study from Mont Jacques-Cartier” by Gautier Davesne et al.

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We would like to thank Anonymous Referee #2 for the constructive review which permitted to improve the manuscript. His/her careful reading of the manuscript and his/her good knowledge of the subject-matter allowed to provide relevant suggestions and additions to the manuscript. We treat each point raised in detail and with great interest. Note that the line numbers given in this response refer to the revised version of the manuscript in track changes mode.

General comments: Comment 1: Referee #2: Is the MS innovative enough for the journal?

Authors: We are aware that the impact of the snow on permafrost thermal regime and distribution has already been studied in several sites around the planet, but not in eastern North America. At a regional scale, this study is therefore of great interest by providing a quantitative and qualitative understanding of the snow cover properties and effects on the ground surface thermal regime and mountain permafrost distribution in the Chic-Chocs Mountains and, most widely, in the Appalachian Range.

Comment 2:

Referee #2: Major comment: My major concerns are related to the results section 4.3 and discussion section 5.2 and partly 5.3. The first paragraph of the section 4.3 is a mixture results and discussion. Thus, it is somehow difficult to be sure which results are from this study and which are derived from the literature. On the contrary, the sections 5.2 and 5.3 (lines 385-390) included completely new results.

Authors: We agree with this comment. The section 4.3 Snow physical and thermal properties was a mixture of results and interpretation, while the section 5.2 Metamorphism and physical properties of the snowpack incorporated new results. As suggested by the referee 2, we reworked deeply both sections. In the section 4.3. (Results), the first paragraph was moved to the section 5.2. (Discussion) lines 380 to 393. In the section 5.2 (Discussion), we moved the paragraph which explains how we calculated the thermal gradient through the snow pack – to the methodology (lines 170 to 183). We also moved the results of the thermal gradient calculation to the section 4.3. (Results) (lines 281 to 285 and lines 293 to 295). Figure 10 became figure 6.

Specific comments:

Comment 3: Referee #2: Title: Why is there a full stop in the end?

Authors: We removed the full stop.

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Comment 4: Referee #2: Abstract: The abstract is partly incomplete. It presents the aims and results but lack conclusions.

Authors: Agreed, we added a sentence that highlights the conclusion of the study (line 27 to 30).

Comment 5: Referee #2: Line 13: It would be nice to see the absolute elevation of the studied mountain (in the brackets after the name).

Authors: Agreed, we added the elevation (line 15).

Comment 6: Referee #2: Line 20 and 23: Please be consistent in the use of space between numbers and °C. Moreover, use minus sign instead of soft hyphen (-) in relevant places throughout the MS.

Authors: Agreed, modifications made to be consistent in the MS.

Comment 7: Referee #2: Line 31: To my opinion, the Table 1 is not needed and could be deleted because there already are many tables and figures in the MS (and Table 1 is the first to remove).

Authors: We consider this table to be useful for readers who are not familiar with the abbreviation regarding thermal terms.

Comment 8: Referee #2: Lines 37-38 (Howe, 1971): Can the presence of permafrost be based on an over 40 year old reference in this marginal permafrost environment (especially considering what is presented in lines 96-99)?

Authors: We cited the paper of Walegur and Nelson (2003). This reference, more recent than Howe (1971), confirms the present-day occurrence of permafrost in Mount Washington (line 55).

Comment 9: Referee #2: The section 2: Relative elevations could be presented somewhere (relevant when considering temperature inversions).

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Authors: Agreed, we added the elevation for Cap-Chat and Cap-Madeleine weather stations (line 95).

Comment 10: Lines 108-109: How typical were the meteorological conditions of the studied years compared to the long-term climate conditions (based on data from the nearest met station)?

Authors: Unfortunately, the measurement of snow falls at the stations of Cap-Madeleine and Cap-Chat are discontinuous, consequently, we cannot calculate the annual total snow accumulations.

Comment 11: Referee #2: Line 116: Why didn't you use freely available Landsat scenes of the study years to explore the general patterns of snow ablation and accumulation?

Authors: A student in our lab made the study of the onset and melt dates of the seasonal snowpack over Mont Jacques-Cartier by analysing Landsat 5 and 7 images from 1990 to present. This study shows interesting results but the error was high due to the poor resolution of images, the frequent clouds cover which reduce the visibility of the target and the long lapse of time between 2 successive images. For this reason, we only deduced the timing and duration of the snowpack based on the daily GST recorded from 2008 to present at the borehole of Mont Jacques-Cartier.

Comment 12: Referee #2: Line 144: Reference to a wrong table? Also line 153.

Authors: Yes, corrections made.

Comment 13: Referee #2: Lines 193, 196 and 199: I think "Fig. 3, Photo 1" could be "Fig. 3A" etc.?

Authors: We agree, the figure 3, caption and citation in the MS have been modified as suggested.

Comment 14: Referee #2: Line 198: Gelifluction? Or rather solifluction (gelifluction +

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frost creep) in this environment?

Authors: Yes, we agree that gelifluction is not the unique process which leads to the development of the solifluction lobe on the SE slope of Mont JC. The melt water derived from the long-lasting snowbank is likely the most important factor. We thus replaced gelifluction by solifluction in the MS (line 251).

Comment 15: Referee #2: Line 261: Rather alpine than tundra (please check and be consistent throughout the MS).

Authors: We agreed. We replace “tundra zone” by “alpine tundra zone” in the MS.

Comment 16: Referee #2: Line 274: Amazingly low minimum temperature considering the measurement site (summit and ground surface)?

Authors: The air temperature can drop below $-35\text{ }^{\circ}\text{C}$ in winter at this elevation. A value of $-30\text{ }^{\circ}\text{C}$ at the ground surface is thus not surprising in areas where the buffer effect played by the snowpack is very weak.

Comment 17: Referee #2: Lines 419-422: It would be nice to see a bit more discussion on this topic (the results of this study—sensitivity of marginal permafrost—climate change indicator).

Authors: We agree with this comment. We added the lines 484 to 489 to mention the high sensitivity of this kind of permafrost to the climate changes due to the quasi direct connexion between the air temperature and the internal ground temperature (no buffer effect played by snow, high thermal conductivity and low ice content of the bedrock). More information concerning the permafrost evolution and sensitivity in the recent context of climate change is available in the following paper we recently published, and to which we refer the reader

Gray, J.T., Davesne, G., Godin, E. and Fortier, D.: The Thermal Regime of Mountain Permafrost at the Summit of Mont Jacques-Cartier in the Gaspé Peninsula, Québec, Canada: A 37 Year Record of Fluctuations showing an Overall Warming

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Trend, Permafrost and Periglacial Processes. doi: 10.1002/ppp.1903, 2016.

Comment 18: Referee #2: Conclusions: In the end of this section, there could be a more general conclusion(s) of the study results–permafrost sensitivity–climate change interface.

Authors: We added the lines 409 to 515 to bring more general conclusions.

Comment 19: Referee #2: Table 4: Please spell out/explain abbreviations and symbols. Tables and figures should stand alone.

Authors: Modifications made.

Comment 20: Referee #2: Figure 5: Please spell out/explain abbreviations and symbols (ps, lambda and R). Authors: Modifications made.

Comment 21: Referee #2: Figure 6: Please spell out MAGST and ACR and explain the meaning of numbered locations (also Fig. 8).

Authors: Ok, sensors are represented as points and the number are their labels (ID).

Comment 22: Referee #2: Figure 7: Please spell out MAGST. How was the PISR variable computed (not mentioned in the methods section)?

Authors: We agree. We added a description in the methods section to explain how the PISR values were obtained (lines 207-209)

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/tc-2016-211/tc-2016-211-AC2-supplement.zip>

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-211, 2016.

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