

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 #1 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 providing 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.

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General comments: Comment 1: Referee #1: Much work has been done with respect to permafrost mapping utilizing the BTS (basal temperature of snow) approach in the Swiss Alps and also more recently in northwestern Canada. It might be useful to compare the equilibrium winter GST obtained for Mont Jacques-Cartier to the range in BTS utilized to determine permafrost probability in these other studies.

Authors: We agree with the importance of mentioning studies undertaken in other sites (especially in the European Alps, the Canadian Rocky Mountains, and the Japanese Alps) regarding the BTS technique used to predict the presence and absence of permafrost. We therefore added complementary information in the introduction (lines 44 to 52) to briefly describe the BTS technique and its limitations for our study. Indeed, on the plateau of Mont Jacques-Cartier - where the snow thickness is very low - the BTS technique is not applicable because a snowpack of at least 80 cm is required to produce reliable BTS values to predict permafrost occurrence. We thus choose to install temperature data loggers to continuously measure the GST for 2 years which enabled determining the threshold of snow thickness above which the MAGST exceeds 0°C. As suggested by the Referee #1, we compare the threshold found for Mont Jacques-Cartier with the one found by Smith and Riseborough (2002) in the discussion part (lines 443 to 447).

Specific comments:

Comment 1: Referee #1: L13 – You could mention the type of data utilized in your analysis to demonstrate your hypothesis (rather than outlining objectives in the next sentence).

Authors: we agree and modified the beginning of the abstract (Lines 14-15).

Comment 2: Referee #1: L15 replace “was” with “were” at end of line

Authors: modification made

Comment 3: Referee #1: L25-29 – Additional papers that may be relevant here and

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elsewhere in the paper that have considered permafrost in mountains in western Canada: Lewkowicz et al. (2012); Bonnaventure et al. (2012).

Authors: We agree. We added Harris (1981) and Lewkowicz et al. (2012) and Bonnaventure et al. (2012) as suggested by the reviewer. We also added Magnin et al., 2016 as the most recent reference available for the Alps regarding the study of the snow control on permafrost (Lines 35-37).

Comment 4: Referee #1: L30-31 – It is not clear here what you mean by snow cover providing a cooling or warming effect. Do you mean if there is little snow, then greater heat loss occurs so surface temperatures will be lower. Also, are you referring to the “surface offset” – see Smith and Riseborough (2002).

Authors: Yes, the thermal effect brought by a snowpack depends on the balance between cooling and warming effects, whose magnitude depends in turn on the thickness, duration, timing and thermal and optical properties of the snow. The warming effect is mainly brought by the insulating capacity of the snow while the cooling effect is brought by the albedo of snow for short wave radiation and its melting which favours latent heat consumption and thus delayed ground surface warming. We agree to use the term “surface thermal offset” to be consistent with Smith and Riseborough (2002) to qualify the offset brought by the snowpack between air temperature and GST. Modifications have been made through the entire MS.

Comment 5: Referee #1: L41 – revision suggested “..and the spatial extent of :.” OR say “spatial distribution of permafrost at this site”.

Authors: We modified for “..and the spatial extent of”.

Comment 6: Referee #1: L48 – Do you mean surface offset? See Smith and Riseborough (2002)

Authors: Yes, modification made to be consistent with Smith and Riseborough (2002).

Comment 7: Referee #1: L71 – Should it be “surface environmental lapse rate”

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Authors: We agree. The term environmental lapse rate is indeed more adapted in this case.

Comment 8: Referee #1: L91 – Revision suggested “: : deep temperature cable that has been monitored continuously since 1977..”

Authors: revision accepted

Comment 9: Referee #1: L93 – You could say “Early measurements between 1977 and x, indicated : :.” (I assume since you give a range that these are measurements made over a few years)

Authors: No, we mentioned here the first measurements made in 1977 following the thermistor cable installation. We made slight changes to clarify this part (lines 119-120).

Comment 10: Referee #1: L94-99 – give temperature at ZAA at beginning of the monitoring period so comparison can be made with the 2013 value. Also why not just say that the temperature at ZAA had risen to -0.3_C by 2013 indicating warming and degradation of permafrost.

Authors: We agree, modification made line 123.

Comment 11: Referee #1: L101 – Isn't the impact of snow on GST fairly well known from other studies?

Authors: Yes, many studies already dealt with the impact of snow on the ground surface thermal regime. We changed the sentence to be more specific on the study case of Mont Jacques-Cartier.

Comment 12: Referee #1: L103 – “measured” might be better word than “monitored”

Authors: We agree

Comment 13: Referee #1: L110-111 – suggested revisions “: : probe 350 mc long.” “: : generally conducted in (late?) March or early April..”

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Authors: suggestion accepted Comment 14: Referee #1: L121 – refer to Fig. 1 for location of Petit Mont Saint-Anne

Authors: Fig. 1 added

Comment 15: Referee #1: L122 – “determine” might be better word than “measure” since some things are calculated from measured values.

Authors: suggestion accepted

Comment 16: Referee #1: L131 – Isn't Lunardini (1981) the original reference for this?
Authors: Yes, the original reference is indeed Lunardini (1981). Modification made.

Comment 17: Referee #1: L136 – Domine et al. (2011) is not in reference list – right year?

Authors: Reference added in the list

Comment 18: Referee #1: L142 – do you mean complete melt/disappearance of snow pack?

Authors: Yes, clarification made

Comment 19: Referee #1: L147 – Positive air or surface temperature?

Authors: Positive air temperature. Modification made.

Comment 20: Referee #1: L153 – Do you mean “beneath a deep snow-bank”?

Authors: Yes, modification made.

Comment 21: Referee #1: L162 – suggested revision “: : on the MAGST was assessed using : :”

Authors: we agree, suggestion accepted.

Comment 22: Referee #1: L170-175 – Did you define the freezing season using the GST and use the same period for summing the air and surface freezing degree days.

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This is what was done by Karunaratne and Burn (2003). Others (e.g. Lewkowicz et al. 2012) consider the air and surface freezing season separately.

Authors: Yes, we considered the air and surface temperature separately. We made slight changes in the manuscript to better distinguish the air and surface freezing DD.

Comment 23: Referee #1: L176-178 – Some of the literature related to BTS (Basal temperature of snow) might be relevant here.

Authors: We added Hoelzle et al (1992); Ishikawa, (2003) as reference related to the concept of WEqT and BTS (line 230).

Comment 24: Referee #1: L178-179 – Latent heat is also released as the active layer freezes in the fall and winter and this can maintain GST near 0_C – see for example Riseborough and Smith (1998).

Authors: Yes, that is true, but in Mont Jacques-Cartier the water content of the regolith layer which covered the summit is expected to be very low. The zero-curtain effect brought by the freezing of the active layer is thus very limited and is not detectable on the GST recorded over the plateau.

Comment 25: Referee #1: Results section – In some places results seem to be combined with background information and some interpretation that might be better in the discussion section.

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)

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(lines 281 to 285 and lines 293 to 295). Figure 10 became figure 6.

Comment 26: Referee #1: L200 – suggested revision “..depth was greater than: : :”

Authors: Suggestion accepted

Comment 27: Referee #1: L211 – suggested revision “: : :was similar to that observed: : :”

Authors: Suggestion accepted

Comment 28: Referee #1: L222 – Do you mean Fig 5b? Also, you need to label a,b,c on the figure.

Authors: Yes, modification made.

Comment 29: Referee #1: L251 – Elevation linked to air temperature, vegetation influence?

Authors: Unfortunately, we cannot statically study the influence of the vegetation on the GST because only 1 sensor is installed on the krummholz belt.

Comment 30: Referee #1: L255 – suggested revision “: : :was highly spatially variable: : :” Authors: Suggestion accepted

Comment 31: Referee #1: L256 – You could say there is a range in winter GST of 14_C. Also you should refer to “sites” rather than “sensors”.

Authors: Yes, modification made.

Comment 32: Referee #1: L263-264 – Heat is conducted but not temperature. Why don't you just say that there is limited insulation provided by the snow pack.

Authors: We agree. We removed the unclear sentences.

Comment 33: Referee #1: L269 – Beneath the snow bank

Authors: Suggestion accepted

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Comment 34: Referee #1: L289-293 – You probably don't need this.

Authors: We preferred to keep that sentence because it enables to clearly introduce the various parts of the discussion.

Comment 35: Referee #1: L328- Delete last part of sentence regarding giving names to figs.

Authors: We agree.

Comment 36: Referee #1: L330-355 – You could write in a more passive voice in this section.

Authors: We agree. This section has been reworked.

Comment 37: Referee #1: L341 – “were” is probably better than “are”; “in” is probably better than “on” L342 – “fluctuated” might be better word.

Authors: Yes, modification made.

Comment 38: Referee #1: L349 – revise “: : : high values up to 100”

Authors: modification made.

Comment 39: Referee #1: L377 – This short zero curtain might also be related to limited moisture content of the active layer (rapid freeze back and minimal latent heat effect) - see for eg. Riseborough 2001; Riseborough and Smith 1998.

Authors: Yes, indeed, please see response to comment 23.

Additional changes:

The title 3.1. have been changed to Interannual snow thickness The title 3.3. have been changed to Snowpack onset and melt analysis The title 4.2. have been changed to Snowpack onset and melt

Please also note the supplement to this comment:

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<http://www.the-cryosphere-discuss.net/tc-2016-211/tc-2016-211-AC1-supplement.zip>

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