Authors reply on referee comments on “The influence of surface characteristics, topography, and continentality on mountain permafrost in British Columbia” by A. Hasler et al.

Dear Editor, dear Referees,

Thank you for your effort with reviewing and commenting on the submitted manuscript! All of you consistently criticize the longwinded character of the paper, the too diverse methods used (two different models additional to the empirical parts) and the overly detailed description of data processing. In response to your suggestions, we made significant changes as summarized below. The most important modifications include:

A) The 3D-modeling has been removed (section 3.3, respective paragraph [P.4797, L.20ff] of section 4.3, Fig. 9, respective conclusion).

B) The thermal diode model has been removed (section 3.4, respective paragraph [P.4800, L14ff] of section 4.4, Fig. 6, Fig. 11). The part of the discussion that followed this paragraph was adapted and rephrased. One new figure equivalent to Fig. 12 was added as a second illustrative example of the main hypothesis of section 4.4.

C) We have shortened the description of the data processing and shifted respective content to the supplementary material to guarantee the traceability of the processing (section 3.1 and 3.2 have been shortened; Figs. 2, 3, and 5a have been shifted to the supplementary material; Figs. 4 and 5b were combined in one figure).

D) We have simplified the manuscript regarding by reducing the amount of abbreviations used in the text.

Author comment (AC) to Ref. #1

RC#1: In the presented manuscript, Hasler et al. attempt an analysis of in-situ records of air, ground surface and ground temperatures in British Columbia, with a particular focus on mountain permafrost. While there are publishable aspects in the study, the reader is lost in a maze of details, techniques, models and interpretations which do not seem to connect to a conclusive picture at all. One gets the impression that the authors attempt to cram in all material they have, instead of designing a manuscript around a carefully chosen working hypothesis. A possible working hypothesis is stated in the abstract: “These findings suggest, that empirical permafrost models based on topo-climatic variables may be applicable across regions with significant macro-climatic differences.” In this case, at least the 3D modeling and the diode model could be left out, but the model approach (for instance a TTOP model with n-factors for snow) and the spatial and temporal scales of application need to be defined. There are other aspects, e.g. permafrost in vertical rock walls, which could be put in the center or the manuscript, but the authors need to reconsider this in the light of the available data.

AC: To make a more concise paper we reduced the content of the manuscript to the empirical parts of the study. All modeling aspects are left aside. Correspondingly the paper is now a pure description and interpretation of the observed temperature time series, mean annual temperatures, and respective offsets. Regarding the methodology it is a hypothesis generation, hence the cited “hypothesis” is a result of the present study. Accordingly, the application of the hypothesis with a simple permafrost distribution model may be subject to a later study.

Major concerns:

RC#1: -3D-modeling: From the images, Mt. Gunnel seems to represent a very particular setting with little to no representativeness. I can not see how more general conclusions can be drawn from the modeling exercise, other than “3D-heat conduction is important at sites with pronounced topography”. Maybe the simulation is of local interest, e.g. for rock wall stability, but such a study is too narrow for the audience of The Cryosphere. I recommend removing this part.

AC: We follow this recommendation. The respective parts have been removed from the manuscript as described above.

RC#1: -The diode model: Firstly, the term “diode model” is misleading in my opinion. A diode completely blocks current in one direction, while snow only limits heat flux. In the circuit scheme, the resistor Rs drawn parallel to the diode is at least as important to make the scheme work (the diode could e.g. be replaced by a switch which is closed in summer). More importantly, I do not see what can be learned from this new model that could not be learned from standard Fourier’s Law of heat conduction. In my opinion, the scheme is fully equivalent to a heat-conduction-scheme with three grid cells of temperatures Tair, Tsurf and Tground (thermal inertia defined by the absolute heat capacity of each grid cell), heat fluxes between the cells defined by thermal conductivities, and a forward Euler time integration scheme. The “resistances” would then be related to the inverse of the thermal diffusivities, multiplied/divided by the time step and the vertical spacing between the grid cells. To assess the transient response of the ground, any transient heat flow model would be at least as suitable as the new diode model. Although being a nice visualization, the diode model is superfluous in the context of this study. It is introduced, but not extensively employed to come to the conclusions of the study. I recommend removing the diode model. If the authors choose not to do so, an extensive comparison to existing transient schemes is required, plus a validation/comparison with in-situ measurements. It is also necessary to derive the values for Rs from the measurements, which are used later.

AC: We agree that the model adds nothing new compared to a very simple 1D heat conduction model considering snow insulation. However, the focus is not on the heat conduction in the soil but on the energy exchange with the atmosphere. The parameters of the diode model have even less physical meaning than stated above. Its only attempt was to illustrate the effect of the annual air temperature amplitude that is negatively correlated with snow cover thickness on the large scale. This effect could be shown with any model that accounts for the sensible heat exchange between the air and the ground surface and that considers the insulating effect of the snow cover (e.g. Goodrich 1982). We left this model out as suggested.

RC#1: -The final sentence of the abstract: I have considerable doubt that this quite far- reaching conclusion can be drawn from the presented data material. In the context of macro-climatic differences, only two regions are compared: HUD near Terrace and NE BC with all the other field sites. The findings do not contradict this conclusion, but they do not fully support it either. On p. 4793, the authors state “However the variation in the parameters of interest (surface type, snow accumulation, slope, aspect, elevation, macro-climate etc.) is not systematic enough and the sample is too small (cf. 5 Sect. 2.2) to quantify the offsets along all potential gradients.” If that’s not possible, how can the authors distinguish from macro-climatic gradients?

AC: With the sentence above we meant that a statistical quantification of the difference of the offsets by classes (e.g. by surface type or site) or by a continuous explanatory variable (e.g. PISR) is not possible. However an exemplary comparison of particular locations with similar conditions can still be done but has less “proving power”. We clarified: “The variation in the parameters of interest (surface type, snow accumulation, slope, aspect, elevation, macro-climate etc.) is not sufficiently systematic and the sample is too small to quantify the difference in the offsets along all potential gradients with statistical methods. Accordingly, the approach we use is an exemplary comparison of the offsets at locations that differ mainly in the parameter of interest but are as similar as possible in the other parameters.”

We rephrased the final sentence of the abstract: “These findings suggest, that topo-climatic factors strongly influence the mountain permafrost distribution in British Columbia”.

We rephrased the conclusions to emphasise that these findings are based on a relatively small amount of observations (locations).

RC#1: -The manuscript contains many cryptic abbreviations, such as “PIN_wx_Tair”, “W_Trock”, “ls_Tsurf”, which seem to be taken directly from the field book, but at least in some passages make reading a painful task. With a bit of detective work and Tables 1 and 2, everything can be found, but few readers will actually do this. The authors should consider reducing the abbreviations to a more manageable level.

AC: We reduced the abbreviations in the text. MAGST, MAAT, MAGT, TTOP, SO and TO are still used and are well established abbreviation in the permafrost community. Further we still use MAT for “mean annual temperature” in general. All other abbreviations such as ATA, RMAT, all biogeoclimatic zonation abbreviations are avoided. The short names for sites and locations are still used in the figures for conciseness (legends would be too large with full names) but in the text we used the full name with the short names in brackets where ever necessary. E.g. if we would use “Pink Mountain – weather station – air temperature” instead of “PIN_wx_Tair” in the figures there would not be much space left for the plot.

Minor points:

RC#1: -Abstract: The first third of the abstract is a general description of the wider field. This could be part of an Introduction, but is too extensive for the Abstract. Concentrate on the findings of the study.

AC: We have shortened this section but kept it. It is necessary to introduce the problem addressed in this contribution.

RC#1: -P. 4782, l.7 “treat data”

AC: corrected

RC#1: -P.4783, l.1: gradient of what?

AC: clarified: “precipitation gradient”

RC#1: -P.4783, l.13: Has this indeed be found or is it just plausible, since it is the case in other areas?

AC: Yes both have been found. We rephrased the sentence to emphasize the general character of the statement: “(…) all sites lie close to the climatic boundary for permafrost to exist.”

RC#1: -P.4784, l.9: I guess “significant” is meant in a statistical sense? It would be better to be explicit and use “statistically significant” (here and in many other cases).

AC: Here it is the result of a statistical significance test (t-test; Egginton, 2005). Below the term is used if the mean offset is larger than the spreads (min – max) of the offsets (see definition at P.4789, L.12). A statistical test (e.g. t-test) of the location parameter does not make sense for the MATs because they are not normally distributed. However, the results of our evaluation of significance are more conservative than the ones of a testing algorithm assuming normality because we use min and max values.

RC#1: -P. 4784, l.20: I agree that these parameters are hard to obtain, but not that they are meaningless.

AC: Rephrased to “of limited use due to extreme small-scale variability”

RC#1: -P.4789, l.23. Noetzli (2008) is not a peer-reviewed paper, so it is a rather weak reference to prove that the modeling procedure is sound. I do not question the soundness of the modeling, but it should be backed up with some further references. See also Major comment.

AC: obsolete - section removed from revised manuscript.

RC#1: -P.4790, l.8: Since it is permafrost-free conditions below the block, the heat flow from below could in principle be significant. At least a sensitivity analysis with “reasonable” values oriented at possible temperature gradients should be done.

AC: obsolete – section removed from revised manuscript (however, the heat fluxes related to lateral gradients in such small and extreme topographic features are several orders of magnitude larger than the geothermal heat flux).

RC#1: -P. 4794, l.13: 30-50cm seems rather little for a block field to generate a thermal offset. If indeed ventilation occurs over a larger volume, e.g. an active layer of 2-3m, the thermal offset to the top of permafrost may be pronounced, but hardly measurable at 30-50 cm depth.

AC: (Line 17ff?) We think it is remarkable that no offset is observed at all in this surface layer. We mentioned the shallow position of the sensors to clarify that a part of the thermal offset might be missed. From our understanding even a thin blocky layer would cause a thermal offset in radiation exposed slopes (difference between net radiation influence at the surface and predominantly sensible heat exchange below). We added to the sentence “(…), however, a part of the TO may be missed due to the shallow measurement depth.” We rephrased the respective conclusion to “The observable surface offsets on coarse blocky material are similar to the ones in fine-grained material.” and left away the second part on the TO.

RC#1: -P. 4796, l. 11: I disagree with the second part of the statement, the part concerning downscaling. If radiation measurements are available in the wall, this may be true, but it is not entirely straight-forward to model rock-wall temperatures in vertical walls using an energy balance scheme, especially the computation of sensible heat fluxes, for which the local air temperature and wind field around the wall must be known.

AC: Yes it requires assumptions about the sensible heat transfer as would be the case in any other situation. Further, the short-wave terrain reflection and long-wave terrain emission are of particular importance in these situations. However, vertical bedrock is still the “simplest” situation regarding surface characteristics and snow cover. This statement is not a result of the present study but an introduction to the topic. We clarified: “(…) comparably good parameter (…)”. We erased “downscaling” and added a reference for the statement.

RC#1: -P. 4798, Sect. 4.4: This is a rather extended discussion of an indeed interesting effect, but the final conclusion seems to be that the in-situ data set is not sufficient/suitable to shed more light in the issue?

AC: We shortened the discussion of the modeling part. Further we clarified the statement about the suitability of the in-situ data (cf. reply to major concerns). The comparison of the average effect per site between continental and costal climate can not be done due to the non-systematic representation of different topographic situations and surface characteristics. However, an exemplary point by point comparison can be done if locations with similar conditions are selected. The hypothesis is hence not “tested” with statistical means but supported by few examples. We added a second example in replacement of the generalization performed by the “thermal diode model” in the initial version.

RC#1: -P. 4802, l. 19ff: this is a potentially interesting finding, but there many factors, such as snow melt rates related to latitude, which complicate the picture. It also depends on how thick the snow pack really is – if it is 1.5m vs. 3.0m, insulation is large in both cases. Also wind drift hugely complicates the picture, since snow depth is then no longer a function of precipitation (and thus continentality) alone.

AC: The main message is: If other factors are similar, the two correlated factors “annual precipitation sum” and “annual air temperature amplitude” tend to outweigh each other. This is true in the examples presented, hence for mid-latitude humid to rather dry climate and we expect it to be valid for this climatic range. We are aware of the complicating effect of local snow patterns. In particular we refer to this effect as follows: „(...) the macro-climatic effect on snow-cover induced SOs is much smaller than expected from local studies, where only the snow-cover thickness varies (...)“. Further, Figure 13 illustrated the different effect on the surface offset of micro-climatic and marco-climatic variation in snow cover thickness.

We rephrased the paragraph and clarified the difference between micro-scale and macro-scale variation in snow depth. Additionally, we emphasised the hypothetical character of this outcome.

Your comment raises several questions:

a) Is the postulated hypothesis plausible at any latitude?
b) Is there an lower/upper limit of snow thickness where the compensational effect of the air temperature amplitude becomes negligible?
c) Is the postulated hypothesis plausible for different topographic and micro-topographic settings (snow blow-ins, radiation exposure)?

These have been addressed as follows:

a) We clarified: “(…) such as the case in our study region (and other mid latitude mountain ranges).
b) Yes, there may be a lower limit of the compensational effect but there the SO becomes small anyway. We clarified: “For a climatic range where a persistent winter snow cover builds up (…)”. We introduced a second example with thin snow cover. There the compensational effect becomes smaller but the SOs are smaller as well.
c) For snow blow-ins or avalanche deposits the continentality effect may become dominant. In radiation exposed situations the snow persistence effect is expected to be more pronounced. However we don not have data from avalange deposits or strong blow-ins. To detail this may be subject of a separate modelling study because flied data from such situations is difficult to obtain.

Author comment (AC) to Ref. #2

RC#2: The study has a great potential to present relevant and valuable results related to how surface characteristics, topography and macro-climate parameters are controlling the mountain permafrost distribution in British Columbia. However, in its current state it is too long-winded and sometimes difficult to follow - mainly due to the fact that too many methods, results, abbreviations and interpretations are included in one single paper.

AC: We reduced the amount of text in the Methods section and abbreviations as introduced above.

RC#2: To be fully appreciated the paper needs to be made more concise and to be carefully structured and some parts related to e.g. the data processing, the thermal diode model and sections in 4.4. could be left out. It is a data-rich paper, and this shows in its length which is significantly longer than normal for TC.

AC: The paper is now substantially shorter in text and figures have been reduced to 8 from 13 in the original manuscript.

RC#2: Specific comments: Although being an interesting concept I do not see any big advantages to introduce the “thermal diode model”. It is only confusing for the readers and is not necessary to support the conclusions of the study. The model is fully equivalent to standard heat conduction theory and general standard terms used by the permafrost and soil science community for decades. Thus I suggest removing this part to get the paper more concise.

AC: done.

RC#2: The authors use a lot of space to present the pre-processing and analysis of the ground temperature data sets, and introduce also the outcome of that work as a part of the conclusions. To get the paper more concise and structured this section should be made much shorter and not be a focus of the study. I suggest that the authors could publish their methods related to calculation of mean annual temperatures, its uncertainty analysis and treatment of data gaps in an own short-communication paper etc.

AC: Although the general aspects of the pre-processing and analysis are common to any GST data, there are specific aspects in the presented processing, which would be difficult to present as stand-alone paper. We shifted many details and three figures of the methods section to the supplementary material and shortened the section 3.1 and 3.2 accordingly. We introduced some explanations on the N-factors that became more prominent with Figure 6 and 7.

RC#2: Some of the key-findings related to surface- and thermal offsets, influence of micro- topography and macro-climate influence are very interesting and are sometimes compared with studies from the European Alps. However, the results could also be discussed more carefully in light of previous results from areas having more similar climatic conditions, in e.g. parts of Scandinavia. Some related Scandinavian studies are:

Farbrot H, Hipp T, Etzelmüller B, Isaksen K, Ødegård RS, Schuler TV, Humlum O.

2011. Air and ground temperature variations observed along elevation and continentality gradients in Southern Norway. Permafrost and Periglacial Processes. DOI: 10.1002/ppp733

Gisnås K, Westermann S, Schuler TV, Litherland T, Isaksen K, Boike J, Etzelmüller B. 2014. A statistical approach to represent small-scale variability of permafrost temperatures due to snow cover. The Cryosphere, 8, 2063-2074

Juliussen H, Humlum O. 2007. Towards a TTOP ground temperature model for mountainous terrain in central-eastern Norway. Permafrost and Periglacial Processes 18: 161–184. DOI: 10.1002/ppp.586

Ødegård RS, Isaksen K, Eiken T, Sollid JL. 2008. MAGST in mountain permafrost, Dovrefjell, southern Norway, 2001–2006. In Proceedings of the Ninth International Conference on Permafrost, Fairbanks, Alaska, Vol. 2, Kane DL, Hinkel KM (eds). Institute of Northern Engineering, University of Alaska Fairbanks: Fairbanks; 1311–1315

AC: Thanks for these nice references! We inserted three of them and one other Scandinavian reference in the discussion section.

RC#2: Abstract: Please make the abstract brief, with aim, scope and findings of the paper clearly indicated. Some of the first text could be shortened.

AC: We made some changes to the abstract. We think it now fits the above structur.