The Cryosphere Discuss., 8, C1980–C1984, 2014
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The Cryosphere
Discussions
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Interactive comment on "The influence of surface characteristics, topography, and continentality on mountain permafrost in British Columbia" *by* A. Hasler et al.

Anonymous Referee #1

Received and published: 7 October 2014

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.

Major concerns:

-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.

-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.

-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? -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.

Minor points:

-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.

-P. 4782, l.7 “treat data”

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

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

-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).

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

-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.

-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.

-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.

-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.

-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?

-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.

Interactive comment on The Cryosphere Discuss., 8, 4779, 2014.