

Interactive comment on “Permafrost degradation risk zone assessment using simulation models” by R. P. Daanen et al.

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In light of previous reviews and my own personal reading of the paper, I believe that the paper needs to be rewritten, but I strongly encourage the Authors to resubmit it. In particular I agree with the first reviewer who states: the description of the modelling chain used is insufficient. In fact I had to consult the original literature in order to get a clear and complete picture of what the Authors did. Dr. Michi Lenhning also made several critical points, which the Authors should answer with the greatest care. The overall impression is that the modeling was unnecessary to drawing the conclusions about the risk assessment. The value added to the research by the modeling needs to be more clearly emphasized. Finally, the fact that Figure 5 is not referenced in the text and the inclusion of a "foreign" paragraph at page 1037 communicate a certain sense

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of superficiality in the drafting of the paper which does not honour the scientific efforts behind it.

Some detailed comments:

1025, Line 15-20 - I am not sure that the kind of modeling presented is the best that the community can do at large scale. A little review in this sense could help.

1026 - Line 17- seperately -> separately

Table 1 - Not self-explanatory (One can deduce some significance from the Units, but it should be clearer)

Figure 2 - What are the units ?

1030 - Line 5 - a comma is missing before "then frozen soil,"

1030 Line 10 - Risk evaluation according to flow chart. <- (full stop, not comma)

1031 - Section 2.4 - Is the model able to reconstruct PT for the last years? This is a basic exercise that all climate projections do (simulation of a control period data). This is actually done (pg 1032) but it should be introduced before, and better explained.

1031 - Line 17 - We speculate ... I can accept the speculation but some literature or rationale in its support would be appreciated. On which basis is the speculation made ? Is it relevant for the overall goals of the paper?

1032-Line 18 - Why, then, were new simulations not performed with better drainage ? ... the sentence probably needs to be rephrased.

1033 - Line 25 - Does this mean that increased salinity increases thermal capacity ?

1034 - Line 4 - Why jump from projection to measures ? Would it not be better to organize the paper so that the assessment of the modeling chain results, confronted against the control period measures, is presented first, and only afterwards are the forecasting results presented?

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1034 - I suppose that Figure 9 reports MAGT. This Figure cannot stand alone; information is missing

1035 - Line 13 : But also an underestimation of thermal capacity, isn't it ?

1036 - "Permafrost temperatures in the northern portion of the country are strongly affected by warming winter temperatures whereas the temperatures in the south are buffered by melting ground ice and deepening of the active layer". The phrase is composed of two parts which seem unrelated to me, unless further specifications are given.

1037 -I assume that the following part is a refuse: "With the discovery of America (Columbus, 1492) a new continent was opened up. However its full exploitation by Europeans and their offspring was not fully complete until many centuries later, as reported by James et al. (1776). During this interval, known as the Winning of the West (Smith and Weston, 1954), a major role in the development of the continent was played by the lowly revolver (e.g. Green et al., 1900). Recently, Phillips (1999) suggested that the magnetosphere could have played an even more significant role. In order to pursue this conjecture, the authors of this work have carried out a historical survey and have found startlingly little evidence for such a claim."

1042 - The procedure that the table should explain is not clear. "Parameter in the sediment classification" would mean that there is a parametrized sediment classification. Instead the table contains the values of model parameters used during simulations. These parameters are not described in the text, and the header of the table is not self-explanatory. Therefore, I suggest that it should be redrawn completely and presented with all necessary information.

Fig 5 - The risk evaluation decision flow diagram is OK but it would require an in-depth comment in the text. Instead the Figure is not even referenced.

Fig 6 - Trivial map, isn't it?

Interactive comment on The Cryosphere Discuss., 5, 1021, 2011.

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