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Discussions

Interactive comment on "Future permafrost conditions along environmental gradients in Zackenberg, Greenland" *by* S. Westermann et al.

Anonymous Referee #1

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This article is clearly presented and well written. The modelling technique appears to be effective at capturing small-scale variability in ground temperatures. I would recommend it for publication in this journal, however, I would suggest that significant improvements should be made beforehand.

For the introduction and methods, I have only minor comments:

Line 20-21 on page 3909, "which is of similar order of magnitude as the" should be "which is of a similar order of magnitude to the".

Line 8 on page 3910 "improved predictions on" -> "improved predictions of"

In Section 3.1 (first part) you should mention that only vertical heat fluxes are simulated (if this is indeed the case?). It would also be useful to know the temporal resolution of

driving data that CryoGrid2 requires.

Later in Section 3.1 (under "ground properties"), page 3918 line 12 says saturated conditions are assumed "except for fell", but in Table 1 it seems saturated conditions are assumed for all including fell?

In Section 3.3, the final paragraph discusses how the snow modelling was validated. However there are no numbers or plots. I would strongly recommend giving more quantitative information here, ideally a plot. (For example, Figure 2 supports the n-factor scheme well.)

In Section 3.4, most of the steps seem to refer only to air temperature? Is the same procedure used for snow depth? Please clarify this. It would also be helpful to clarify the random sampling procedure. It is not clear whether the offsets are taken from a whole year (from 2003-2010) for each past/future year, or the year from which offsets are taken changes every month. It also also not clear whether offsets are used only from one time in 2003-2010 for each correction factor, or whether a mean offset for several (randomly selected) years is calculated?

In Section 3.5, page 3923 line 12, "date" -> "data". Also line 21, remove "be".

For the remaining Sections (results, discussion, conclusions) more significant improvements are recommended.

For Figure 5, it would be useful to show the range of values for each vegetation class, similar to Figures 3/4/6. In fact you could even combine Figures 3 and 5, showing the observations on only the early part of the graph, and with a shaded band showing the range of simulated values. Combining the graphs may not work, since they are discussed in different places in the text. Please consider this but use your own judgement.

In Section 4.1 (under Active layer thickness) you state that "CryoGrid2 can capture the significant differences between the three sediment classes Dryas, Cassiope and

wetland caused by the different soil moisture contents". Is there no other difference between these classes in the model? Such as the NDVI factors, mineral soil composition? The statement that it is due only to the moisture contents does not appear to be fully justified.

Page 3925, "the biological activity ... are" -> "the biological activity ... is"

The list of uncertainties (Section 5.2) is a nice, concise list. However, it would be helpful to make it clearer which is likely to be the greatest uncertainty. Perhaps this could be achieved by considering order of magnitude estimates from the previous studies that you cite.

In the discussion and conclusions some claims are not fully supported by the work. Namely the final paragraph where you claim that GCM's "are not capable of correctly predicting the onset of permafrost thaw". Your study has shown that your technique can predict the onset of permafrost thaw, but it has not shown anything about any other model. It may be that large-scale models can predict it via some parametrization or grid-box mean - at least you have not made any justification that they cannot. In 5.3 you mention "a simple increase of the spatial resolution seems a prerequisite to resolve such shortcomings", but there is no discussion of how this could be achieved on a large scale. I suggest that you include more discussion of the possible applications of your techniques and emphasise how they can be useful for large-scale modelling, if you wish to address shortcomings of large-scale models. It may be that your high-resolution work could inform parameterizations of sub-grid-scale processes in GCM's?

Finally a major improvement that I suggest for this work is some more analysis of the results. You have included spatial variability of several factors (listed nicely in the beginning of Section 3), but there is no analysis of which of these factors is the most important to take into account - to which are the soil temperatures most sensitive? You claim briefly in Section 5.3 that the spatial variability of ground temperatures is caused mainly by the snow depth, but there is no evidence of this presented in the paper.

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It would be very interesting if there were some plots actually along the transect. If, for example, you plotted mean snow depths and soil temperatures along the transect (and other variables too for comparison), it may be clear that the snow has the biggest influence. Or you could compute the strength of correlation with soil temperature and snow depth. I would leave it up to you how you show the influence of each factor, but I must stress that it would add a lot of value to the paper from the point of view of a modeller, who may wish to know what the key aspects are that they should first consider.

I hope these comments have been useful, please let me know if it doesn't make sense or if you disagree. I enjoyed reading your paper and look forward to reading an improved version.

Best wishes.