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Interactive comment on "A statistical approach to represent small-scale variability of permafrost temperatures due to snow cover" *by* K. Gisnås et al.

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This paper addresses the important topic of the variability of subgrid ground temperatures, especially in heterogeneous environments. It is commonly acknowledged that high resolution representation of the surface is required in heterogeneous environments in order to capture the variability of processes such as ground temperature, which would not be resolved at coarser resolutions. However, this is often computationally very costly.

In order to address this problem the authors provide a proof-of-concept for a statistical representation of subgrid ground temperatures based upon the assumption that the

distribution of snow heights significantly controls ground temperature variability. The approach is developed and tested at three field sites spanning a climatic gradient of 61-79 deg N in Norway. The authors compute transfer functions between measured snow heights and measured mean annual ground surface temperature (MAGST) captured by a network of temperature loggers. These transfer functions are then used to compute modelled MAGST as driven by measured snow heights (GPR). The authors conclude that if the distribution of snow heights is known then a statistical representation of ground surface temperatures is possible.

I believe this to be an important topic and that statistical approaches to capture subgrid distributions of physical processes can be an important approach to allow scalability of models. I think the approach and direction taken by the Authors is interesting in its attempt to reduce the complexity of the subgrid problem and in developing a novel technique for MAGST quantification in Northern regions. However, I think the aims and conclusions of this work need to be refocused before publication, both in introduction and discussion to really define what the contribution of this work is, as well as interpretation of the validity of results in terms of how widely applicable they are, both temporally (past/future) and spatially (beyond the field sites used in this work). Additionally, I feel as a proof-of-concept it misses some details on how it upscales to regional applications – as I understand it is intended to.

As discussed below I feel that there are some shortcomings in the experimental design/ methods and resulting conclusions which need to be addressed more thoroughly in a revised version of the paper, together with an enhanced discussion on limitations of the approach.

A. GENERAL COMMENTS:

1. In northern climates, such as the focus of this study, solar radiation plays a much reduced effect compared to more southerly (and steeper) terrain (although it is difficult to generalise as a large range of latitudes 61-79 deg N are covered). However, I am

not convinced that snow height can be stated so unequivocally as the single most important variable governing ground temperatures. Aspect and ground type can be significant even at very fine scales (e.g. Gubler et al. 2011). For example, a snow pack of insulating depth may fail to de-couple the ground from the atmosphere when the surface is composed of large-block material. Additionally, choice of sampling site has a large influence on this studies results and interpretation of significant processes. For example, if the Ny Alesund field site was located some 2 km south-east on the steep N facing slopes - other processes related to aspect or slope (e.g. avalanche deposition requiring a different modelling approach to that of wind-blown snow) may become significant. The sites in general appear to be reasonably homogeneous so it is difficult to assess the importance of other variables on MAGST variability in these climatic zones. Along the same lines, a thorough description of variability of other variables (topography, surface, subsurface) that influence MAGST and how these were sampled would be useful to interpret the results (e.g. Fig. 3).

2. Wind-blown snow modules (such as Alpine3D) which could be used to compute a snow height distribution can be expensive to run on a fine subgrid, particularly over large areas as they require a fully distributed simulation. It would be good to provide some more details on the costs of running such modules and impact on efficiency of the proposed subgrid scheme (how many years would you run etc.). In addition a quick comment on the ability of these schemes to re-create wind-drift patterns at various spatial-scales (assuming we are interested in the fine scale at which topographically modified wind patterns can be very different from the larger scale forcing) and uncertainties related to processes such as sublimation, would be useful. Especially considering such a method is fundamental to the implementation of the proposed scheme over larger areas.

3. Do you think the snowmobile surveys would have a sampling bias (aside from snow/no snow that is mentioned) due to the terrain it is possible to cross (because of difficult terrain, steep slopes) and therefore choice of field site? How does this influ-

ence the whole experimental design and wider application of results?

4. Study sites do not represent really complex topography, especially in Svalbard - how transferable is this method to more heterogeneous environments? For example, how would the approach be expected to perform if the study footprint (or coarse grid) lay across a North/South mountain ridge where other processes could be important in driving MAGST? This point relates to how this scheme upscales as a regional modelling approach as the footprint of a coarse model grid unit (e.g. 1 km x 1 km CryoGRID), cannot be assumed to be as homogeneous as the field sites appear to be.

5. How transferable are the calibrated N-factors to other years? It would be good to include some comments on how this would upscale temporally/ spatially. Would the model be recalibrated every year? What is the spatial resolution over which a given calibration would be considered valid?

6. In general, the assertion that wind-blown snow is the most dominant process at subgrid scales (while possibly true at the field sites in this study) is not proven by the data as other variables which govern MAGST are not tested. In addition, the conclusion that GST variability is small during summer and early winter (p524 l.6) really depends on the heterogeneity that exists within the footprint tested.

7. The authors state that this approach enables a simple equilibrium permafrost model to reproduce observed ground temperature distributions. I think this statement needs to be backed up a little more strongly than simply eye-balling Fig. 2+5.

8. It seems that some of the same datasets/ sites were used to calibrate as well as evaluate the model which calls into question independence of results.

B. SPECIFIC COMMENTS:

1. p.511 l.23: Perhaps change “implemented” to “established”.

2. Sect. 3: How was the random distribution of loggers achieved?

3. Sect. 3: What was the basis of each field-site footprint selection?

4. p.515 l.19: I think this sentence needs to be re-phrased.

5. A definition of “snow maximum” would be helpful, e.g. p.515 l.19. Acknowledgement of uncertainty with the selection of this date would be good.

6. p.515 l.21: “lacks 13 days to an entire year” - do you really mean a range here (13 days–1year)?

7. p.515 l.21: Is surface temperature assumed to equal air temperature in gap filling?

8. p.515 l.25-26: Re-phrase to make what you mean clearer.

9. p.517 l.1: What are the likely implications of under-sampling shallow snowpacks?

10. p.517 l.18: How are the degree days extrapolated to the field-sites? How would this be done without measurement stations near by i.e. away from established experimental sites?

11. p.517 l.22: "surface vegetation type" - did you also consider non-vegetated surfaces?

12. p.518 l.15: How exactly is the snow cover duration calculated? Do you only consider a thermally-insulating snowpack (decoupled surface and atmosphere)? Schmid et al. (2012) discuss this topic and how a melt-date can be robustly estimated from ground temperature measurements.

13. p.518 l.20: Perhaps describe this phenomenon as the zero-curtain which is related to thermal inertia due to phase change (Outcalt et al. 1990).

14. p. 521 l.10: Exactly 1 m? always? Perhaps modify to "approximately 1 m".

15. p.521 l.13: ”is” > “are”

16. p.521 l.20-23: Make sure this isn’t misunderstood as a generally applicable conclusion.

17. p.522 l.20: Agreed, but as stated above a wind-model still needs to be run on the fine-grid with suitable wind field and there are significant uncertainties with processes such as sublimation. These associated costs and uncertainties should be mentioned.

18. p522 l.27-28: I don’t think you can confidently say this as the statistical technique applied here may not be valid under future conditions.

19. p523 l.16: I would suggest this is a strong statement considering that aspect and slope, ground etc. were not sampled. How representative is the study footprint from the broader study area?

20. Qualifying statement about solar radiation you have on p.523 l.18 should already be made much earlier to allow the reader to know you are not generalising your results to regions where other variables are certainly significant.

21. Fig. 1 needs to be larger.

REFERENCES:

Gubler, S., Fiddes, J., Keller, M., and Gruber, S. (2011): Scale-dependent measurement and analysisof ground surface temperature variability in alpine terrain, The Cryosphere, 5, 431–443.

Outcalt, S. I., F. E. Nelson, and K. M. Hinkel (1990), The zero-curtain effect: Heat and mass transfer across an isothermal region in freezing soil, Water Resour. Res., 26(7), 1509–1516, doi:10.1029/WR026i007p01509.

Schmid, M.-O., Gubler, S., Fiddes, J., and Gruber, S.: Inferring snowpack ripening and melt-out from distributed measurements of near-surface ground temperatures, The Cryosphere, 6, 1127-1139, doi:10.5194/tc-6-1127-2012, 2012.

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