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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.

Anonymous Referee #2

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This paper treats an important topic by assessing sub-grid variability of ground temperatures. Today, a well-known problem in mountain areas is the high resolution required by model grids to capture the existing large variability caused by different factors such as the highly variable snow height or the different surface and sub-surface characteristics. Therefore, approaches are needed to overcome this problem and to bridge the scale gap between for example gridded climate data with a resolution of 1 km2 and the complex mountain topography requiring a much higher spatial resolution. The approach of the authors is based on the idea that one of the most variable and influencing factors determining ground temperature is the snow cover, which is in their case simply replaced by the easy measureable snow height. The authors introduce then statistical transfer functions between measured snow heights and mean annual ground temperatures measured with many small temperature loggers. Then they compare modelled temperatures driven by GPR measured snow heights with measured temperatures of the loggers at three different investigation sites, Ny Ålesund, Svalbard, Juvasshøe, Jotunheimen and Finse, Hardangervidda. They conclude that the agreement between modelled and measured temperatures is good and therefore the transfer functions can be used very well.

General Comments: In general, I see well what the authors intended and I would say that their approach is somehow reasonable to reach their target for the investigation area within Scandinavia. However, I see some fundamental questions, which arise with their approach and I would like to suggest that the authors addresses these topics in a revised version of the paper in their discussion chapter in a more detailed manner.

1. What do we learn from scaling factors like nF and nT? These scaling factors do not enhance our process understanding and are in general questionable in this context. The scaling factors were mainly introduced by Lunardini (1978) with a main focus on engineering applications. Therefore, I would expect for a paper like it is presented here, a much more process-based approach, which would fit more into a geoscientific journal like 'The Cryosphere', aiming at a better process understanding. However, I completely agree that these scaling factors are sometimes useful for the application of calibrated permafrost models simulating the current or maybe also the past permafrost distribution in areas like the Arctic and Scandinavia but in alpine to high-alpine environments, they are not applicable, because for example the freezing n-factor, nF, has a very large seasonal variability, which cannot be captured with annually calibrated factors.

2. In addition, if n-factors are used to perform future scenario modelling based on a TTOP approach several problems will arise. Temporal extrapolations with statistical derived n-factor models are not straightforward, because these models do not contain any non-linear feedback mechanisms such as the influence of non-conductive processes such as water transport in the ground or air within coarse grained ground and

neglecting any transient changes of the n-factors itself.

3. The model validation with an independent dataset is in my opinion very important. I believe that the used dataset is not completely independent although the authors used different methods of snow height measurements like manual snow probing and GPR measurements. I would suggest that the model should have been applied at an independent place where the measured data was not used for the establishment of the transfer functions.

4. The authors calibrate their model with only one factor (snow height) knowing that although they get statistically seen very good results, it does by no mean reflect the variability of the 'real' nature, where snow height is an important factor among many others like aspect, slope, ground characteristics (vegetation, bedrock, fine or coarse material). As the authors correctly state at the end of their discussion on page 523, this would require a more sophisticated statistical approach. Therefore immediately the question arises 'why you did not include other parameters like the ones mentioned before?'

5. Fiddes and Gruber (2012, 2013) and Gubler et al. (2011, 2013) for example show approaches, being more process-based but with the same objective like this paper to overcome the problem of sub-grid variability. The question arises here 'why not using a more sophisticated approach to overcome the sub-grid variability?'

6. The authors talk in their paper always from ground surface temperature. However, the sensors are NOT at the surface itself, they are some cm in the ground, therefore they should better use the expression ground temperatures. If a surface temperature is measured you will of course receive completely different results because of radiation effects.

Specific Comments:

1. Page 512, line 15: please use always the same units in meter instead of centimeters.

2. Page 513, line 1: suggestion: better use maritime instead of marine

3. Page 513, line 12: maybe better to talk about 'active permafrost' in the sense that active layer refreezes during winter. At your site probably relict permafrost will still exist in larger depth for hundreds of even thousands of years based on the fact that the heat flow is strongly reduced and low gradients are observed as mentioned on line 8 above.

4. Page 515, line 5: missing an before accuracy

5. Page 515, line 19: missing a before probe

6. Page 517, line 15: missing the before number

7. Page 517, line 25 and 26: I do not believe that the estimate of this temperatures can easily be transferred to the ground temperature without taking into account the highly non-linear processes within the ground itself. I would probably agree at a site like Juvasshøe, where you have a quite direct relation between the atmosphere and the ground (mainly bedrock). However, at sites with complex ground characteristics it is by far not so straight forward.

8. Page 518, line 18: The authors write: 'The median at Finse is close to 0°C during the entire winter, showing that GST is decoupled from the air temperature in large parts of the field area.' I interpret this sentence probably different as the authors do, therefore this sentence needs clarification. I would interpret the fact that the temperatures at Finse are close to 0°C not only as a 'decoupling' effect but much more an effect that there is mainly no permafrost at the investigation site and therefore the ground temperatures stay at zero degree during the whole winter time. You have a decoupling also at the permafrost sites, but then the permafrost influences your measured ground temperature strongly

9. Page 520, line 9-11: This is quite an 'academic' exercise as in reality as already mentioned above other factors like ground characteristics have sometimes even a larger influence on the ground temperatures than the snow cover.

10. Page 521 & 522, line26 and 1-3: I fully agree with this sentence. However, it also shows how dangerous such statistical models can be. If you calibrate your snow distribution from the past with a statistical approach like you have it chosen here, this approach could fail for future conditions when other effects would change your snow redistribution for example by different future synoptic conditions.

11. Page 522, line 15: I agree that for Scandinavian conditions, the chosen approach is feasible but for other mountain areas it is not easily transferable.

12. Page 522, line 20: It is feasible but you loose process understanding and it is likely not applicable for predictions!

13. Page 522 and 523, line 27,28 and 1-4: I do not agree with this statement because of the already mentioned reasons in my general comments. Statistical relations are not useful for simulations of the future behavior of permafrost because of many non-linear effects.

Literature:

Fiddes, J. and Gruber, S. (2012): TopoSUB: a tool for effi cient large area numerical modelling in complex topography at sub-grid scales, Geosci. Model Dev., 5, 1245–1257, doi:10.5194/gmd-5-1245-2012.

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Gubler, S., S. Endrizzi, S. Gruber and R. S. Purves (2013). "Sensitivities and uncertainties of modeled ground temperatures in mountain environments." Geoscientific Model Development 6: 1319-1336.

Lunardini, V. J. (1978). Theory of n-factors and correlation of data. 3rd International Conference on Permafrost, Edmonton, Alberta, Canada, National Research Council, Canada.

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