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Interactive comment on "Distributed snow and rock temperature modelling in steep rock walls using Alpine3D" *by* A. Haberkorn et al.

Anonymous Referee #1

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The paper Distributed snow and rock temperature modelling in steep rock walls using Alpine3D from A. Haberkorn and her team presents a 3D modelling method to describe the small-scale patterns of snow accumulation over a steep rock ridge in the Swiss Alps and its effect on the rock wall surface temperature. The study focuses at the measurement point scale and the ridge scale, using a high resolution data set and model domain. The topic is of high interest for the scientific community investigating the distribution and changes of steep slope permafrost in alpine environment. The investigation of the snow control on steep slope permafrost, especially in 3D, is one of the current research challenges. This study therefore tackles a very challenging topic, which is well aligned with the current research directions and previous work from the team.

The methodological approach is sound and achieved. Analysis of the small-scale snow accumulation patterns and surface temperature variability in steep slope requires high spatial resolution. The presented study uses a high spatial resolution dataset with a 0.2 m DEM built upon TLS data, snow height data based on TLS surveys and rock wall temperature measurements at the surface. It proposes a scaling method to approximate snow accumulation from snow height records at a close weather station, and a 3D energy balance model. The method combination is relevant and field data are substantial, but after several readings, it is still difficult for the reader to have a clear overview. Some suggestions are given to improve the clarity of the method imbrication.

The paper is structured into seven parts further divided into subchapters. The structure is clear and appropriate to present the study. The overall paper is well written but some sections remain hard to follow because many details are given, and the way that the results are partitioned sometimes lead to confusions. As a result, it is very difficult to draw the outlines and retrace the main results at the end. The results are of high interest for the research field, but it is difficult for the reader to point out the most important findings and the main outreaches of the paper. The abstract is relatively poor given the high interest of the results; the main outcomes are not highlighted as much as they would deserve. Some major findings could be better emphasized in the result sections, but also in the abstract and conclusion. The conclusion does not seem to report the main outlines of the paper. In the same way, it is difficult to understand the key-objectives of the study: the validation of Alpine3D? The characterisation of the snow accumulation patterns over steep rock ridges? The characterisation of the small-scale snow control? Possibly this paper tackles all of these objectives, but they should be better outlined and better addressed. Detailing the specific research questions addressed in the paper would possibly help. The broad scientific context is not clear either: is this paper having outreaches in the climate change researches? In the cryosphere distribution investigations? For geomorphological studies? Being clearer with the global goals and results of the paper will ensure a greater impact of the results and will give the paper more visibility. Some suggestions are also given to help in improving the clarity of the paper. Due to the wealth of data and methods, also due to the possible outreaches and significance of the results for the scientific community, this paper definitely deserves publication in TC. However, it is important that the reader gain an overview of the main findings both for the global scientific context and for the research questions specifically addressed in the study, as well as a clear outline of the methods, related results and limits. It thus appear necessary to improve the text, at least in a first step since it is difficult to retrace the main interests and limits of the paper at the current stage. I recommend this paper for publication in TC after major revisions.

MAJOR REVISIONS

1. I suggest writing the abstract again: first, state what is the global scientific context and specific objectives of the paper. Improve the highlights of your main findings in the results paragraph. Try to be more quantitative if possible The results provided at lines 20-21 seem to contradict previous statements from former studies (e.g. Hasler et al. 2011 found a cooling effect of snow in the sun-exposed rock walls such as mentioned line 81), which is of high interested for the scientific community. This contradiction with current theory might deserve more developments, at least a few words in the abstract and some more in the results/discussion/conclusion to explain why the here presented findings differ from the previous ones (matter of snow height? Snow timing?).

2. Improve the presentation of the methods with (1) a specific figure and (2) an introductory section to explain in a very short paragraph how the methodological approaches, the various spatial resolutions from the different approaches, the characteristics of the input and output data are imbricated. The methodological approach is very complete and involves many steps with various sources of data and computing steps at various time and space scale. After several readings, it is still difficult to gain an overview of the imbrication of data and processing. To improve the visibility and understanding of the method outlines, I suggest preparing a specific figure to sum up the imbrication of the input/output data and processing (e.g. similar and maybe slightly more detailed than the Figures 2 from Noetzli et al. 2007; Figures 1 from Noetzli and Gruber 2009 or from

Fiddes et al., 2015). Also, it would be very helpful to have one or a few introductory sentences for chapter 3 to sum up how the methods and data are imbricated.

3. Improve the presentation of the results and their discussion. In a similar way than the methods, an introductory paragraph to sum up your approach and clearly explain the outlines of the result chapter would be really helpful given the high number of steps in the result presentation. Some general suggestions and comments are given here below, but more specific comments are given in the appropriate section. When simulating at high spatial resolution, the sources of uncertainty are many, and this result in sometimes important bias. Those sources of bias are not always well discussed (e.g. why the model is more performant on the N than on the S face?), whereas some discussion points seem disconnected from the study (why to mention the effects of water percolation along fractures? Where is the link with your study?). Also, to help in understanding the sources of errors, it seems important to compare topographical characteristics of your measurement points used for model evaluation in the "real-world" and those in the "numerical environment" (DEM). Do the sensors have same aspect in both situations? Same slope angle? This information could be added in Table 1 for instance. In case of substantial discrepancies between both environments, this could explain a part of the bias. Results are sometimes hard to follow due to the numerous back-and-forth between figures and the text. You sometimes refer to several figures for a same thing, and not all references are relevant (e.g line 336: you refer to Fig. 3b to show the difference between measured and modelled MANRST, whereas Figure 3 shows daily variation). Figure 3a is not referred in the text. The data on which the MBE and R^2 are calculated not always clearly indicated. Many confusions are arising and being more precise would help the reader to go straight to the point. Section 4.1.3. must be written more clearly, at that stage, it is hard to follow. It contains lots of essential information but some details are missing to well understand how the model evaluation is performed, how the misfit between measured and modelled value are taken into account to go further in the study (see detailed comments). Finally, the study seems to contradict previous findings. So far, it was suspected that snow on South faces cools

the surface temperature (e.g. Hasler et al., 2011). In this study, the opposite is stated, and the contradiction is not well discussed, nor well emphasized. What would be the possible factors/processes explaining that your findings are in contradiction with previous findings? Also, Figure 6 which shows an important part of the results is poorly discussed. By looking at this figure it clearly appear that vertical faces without snow induce colder conditions than snow covered slopes. This is well aligned with recent findings in Norway (Myhra et al., 2015) and should be better emphasized and discuss.

GENERAL COMMENTS

1. The references to the existing literature are not always consistent with the text. Some examples of inconsistencies between the text and the references are given in the specific comments, but not all of them. Please, consider this comment and verify your references all along the text.

2. Introduction: 1st paragraph is poorly written. First two sentences focus on rock wall permafrost (with a strange way to use references) whereas the two other sentences, apparently aligned with the first two sentence mention the need to model permafrost with example from very different alpine permafrost terrains, that are not relevant to address the questions related to “rock wall permafrost”. This must be improved to be more consistent and to better settle your study in its global research field.

3. The study site is made of a NW and SSE faces (according to Table 1) named N and S face. Whilst naming N and S face is not a problem, it seems that these slopes are considered as real N and S facing slopes in the study (e.g. the apparently unexpected low difference in surface temperature, which is maybe not as low as suggested given the real aspect of the slopes). During revision, this should be taken into consideration to avoid scientific imprecision and straightforward conclusions. âĂČ SPECIFIC AND TECHNICAL COMMENTS:

- Lines 20-21: is this sentence written in proper English? It seems confusing.

- Line 29: what does “large” mean? Some rock falls affected “narrow” rock faces, pinnacles, ridges... Is this word really appropriate?

- Line 31: Davies et al. 2001 didn’t not investigate the stability of permafrost in high Alpine regions but proposed a laboratory study under very specific conditions. Gruber et al. 2004a didn’t not investigate rock wall stability, but only permafrost distribution. Are these references really appropriate? - Line 35: the reference to Gruber, 2012 doesn’t seem appropriate since the sentence focus on permafrost modelling in the European Alps and Gruber’s work focused on global models.

- Line 36: there are better examples than Fiddes et al. 2015 as numerical modelling of mountain permafrost (especially in rock walls).

- Line 41: Harris et al. 2009 paper does not focus on modelling transient changes in rock wall permafrost. Here again, better examples could be provided (e.g. only keep Noetzli et al. 2007 and move it at the end of the sentence, other examples could be added: e.g. Noetzli and Gruber 2009).

- Line 46: “However this approach cannot capture...” Is it really because of the modelling approach that the small scale variability cannot be captured or because of the spatial resolution?

- Line 59: Gruber et al., 2004b and Gruber and Haeberli 2007 didn’t really study the snow control. The last reference, proposed some theories and hypotheses about the snow control, but not a study dedicated to its effect.

- Line 63: Pogliotti, 2011 focused on the snow control in steep rock faces similarly to the here presented study, but in 1D. He only proposed a review of the existing literature stating ablation processes in steep alpine rock faces, but did not study the gravitational processes directly such as suggested by this reference.

- Line 65: Gruber et al. 2004a study considered ideal rock walls, not the kind of “natural” rock walls described in the text before the reference.

- Line 82: is “However,” really the right term? It connects the starting sentence with the former sentence in the sense of “Nevertheless”, and opposes the new sentence to previous statement. But the smoothed temperature difference between N and S face results of the warming/cooling effect of snow, it is a consequence. Could you consider this and revise your sentence accordingly to avoid confusion? Maybe there is an opposition between two sentences but it is not clear when reading.

- Line 82-84: References are not consistent: do you mean that thick snow smoothes the variability of MAGST compared to snow free bedrock (Gruber et al., 2004b; Noetzli et al., 2007) or compared to bedrock with thin and intermittent snow (Hasler et al., 2011)? The sentence has to be more precise and the references better used.

- Line 89: What is the difference between NRST and the “rock thermal regime”? Do you mean the thermal regime at depth?

- Lines 118-119: could you explain why did you choose this reference period? Data availability?

- Lines 123-125 could you at least tell when the iButtons were installed in order that the reader doesn’t have to look for essential information into the referred paper.

- Line 127: here also I don’t understand the meaning of “However,”.

- Lines 131-132 and 135-136: could you be more precise with the features that you describe? What is the difference in temperature amplitude between N7 and N3? How do you see the snow influence on S9?

- Lines 183-184: it is difficult to understand the end of the sentence: “hence a constant upward ground heat flux is applied as the lower boundary condition”. Please, could you reformulate and be more precise?

- Lines 190: could you give an indication of the gap proportion in the meteorological data and of the bias induced by the gap filling procedure (even if information also exists in Haberkorn et al., 2015b)? Does the gap filling procedure induce a part of the bias in

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model results?

- Lines 200-201: the reason for which the thermal parameters, especially those at depth (such as 100% solid content which is unusual in modelling rock wall thermal regime) have been chosen is not clear. The utility of these parameters to simulate rock wall surface temperature is not clear either. Could you be more precise about this points?

- Line 233: Wouldn't be "one" instead of "an" in "an Alpine3D run"?

- Lines 234-235: could you provide a concise overview of the results for the three other TLS. What "coincided best with validation data" means quantitatively?

- Lines 278-280: it is not really easy to report the mentioned results to the figure. On which data are the R^2 and MBE calculated? You report to figure 2b and c to compare snow heights measured with TLS and modelled with Alpine3D, but those figures only show the measured snow depth. Also, a scatter plot would help the reader to better see the comparison between modelled and measured values.

- Line 284: "for each NRST logger": it is only 4 loggers, right? Why other NRST loggers were not used (except that those used are enough to represent snow cover variability according to lines 127-128)? One could easily think that using more loggers could provide more robustness to the MBE and R^2 analysis.

- Lines 288-292: this paragraph is not clear either. "four independent TLS", but one of them was used to scale the snow accumulation (11.12.2013), right? So, is it true to say "independent". "R^2=0.95": which data were used for this calculation: modelled versus measured snow height for each grid cells and for each TLS survey? Why to show results from 11.12.13 if those data are used for scaling (and are therefore not independent)? Could you show a scatter plot or at least better illustrate the model output by e.g. replacing one of the 3D or 2D view in Figure 2?

- Line 296: Is the term "validation" really appropriate?

- Lines 299-301: here you give a reason for misfit between modelled and measured values. The same explanation could be expected lines 290-292: where the under/over-estimations are coming from? Modelling of ablation? If it is given in the discussion, the same should be done for these lines 299-301. If you make the choice to directly discuss your results, an explanation could be expected lines 290-292.

- Lines 300-301: Here the modelled snow depth for the S measured point does not fit the measured values. A 1 m difference may have huge implications for the NRST simulations. How is that taken into account?

- Lines 342-344: why such a big bias (-2°C)? What is its implication in the overall results?

- Lines 362-364: the difference is calculated using the 30 NRST time series?

- Lines 363-364: not as high as expected for “real” North and South walls, which is not the case here, with rather NW and SE faces. This must be taken into consideration!

- Lines 392-399: figure 6 deserves much more description and precision. Could you be more precise in the text with the 1.9°C? For what? Entire model domain? North face.? South face? Measurement point controls?

- Lines 425ff: here the energy balance of snow free N7 is presented like absolutely different from snow covered N7 (“In contrast to”, ”differed strongly”) . However, when looking at Figure 7, the pattern of Qnet seems quite similar, only the magnitude differs, Qsensible differs in a certain degree. Are the terms really appropriate?

- Line 367: “effects: In” is either “effects. In” or “effects: in”

- Line 447: why not modelling heat transfers in fractures is a limit of your model? Are you also modelling the interior of the ridge? If not, please remove, there are already enough details to discuss. If yes, it should appear clearly all along the text that you do not only model surface temperature and substantial results on the model temperature at depth must be provided!

- Line 450: the consideration of snow cover at the ground surface is especially important to model small scale temperature variability. Some studies have shown that equilibrium temperature fields and long-term changes can mainly consider air temperature and solar radiation in steep slope. Please, rework the sentence accordingly.

- Lines 456-457: the statement is interesting (it appear more important to correctly model snow timing to better represent snow effect) but could you at least provide one example in order to help the reader to connect this discussion to the results?

- Line 461: again, the references are not adapted to the text: Gruber et al., 2004 and Noetzli et al., 2007 do not propose a “traditional snow modelling technique”.

- Lines 485-486: this belongs to the results, so move in another section and connect it with the presented results. On which source of data is this calculated? What is the difference with other presented MBE (e.g. MBE of -2°C line 342)?

- Lines 504-505: Isn’t the difference of snow free/snow covered faces between N/S aspects in the range of model uncertainty?

- Lines 515-520: these appear as important results that would confirm recent findings in Scandinavian rock walls (Myhra et al., 2015): rock walls favour the presence of permafrost (here in the Alps, that would be especially true for North slopes?). This must be better emphasized.

- Lines 541-542: reaching that stage of the paper, the use of 30 NRST logger is still not clear: where the validation is shown? In figures, only 4 loggers are used and discussed. Same question as previously: is “validation” really appropriate?

- Line 553: “50°”, how this threshold has been defined? It appears for the first time in the conclusion.

- Line 554: is “accurately” really appropriate when significant bias have been displayed?

- Lines 569-571: this is an interesting result but it has only be mentioned in the discussion. No quantitative information nor graphical results are provided for such statement. Either remove from the conclusion and remain as close as possible of your major findings, or develop the results related to grid-scale sensitivity analysis.

FIGURE AND TABLE

- Figure 1c: what are the peaks between 2930-2950 and 161750-161780 on the y and x axis respectively? They look like artefacts in the DEM. How did you clean up the points cloud before generating the DEM? Furthermore, I the figure could be improved by including a hillshade below the elevation colour scale to improve the visibility of micro-topography.

- Figure 2a: it is very difficult to read the legend, could you make it bigger?

- Figure 3: This figure must be improved. I propose the following modification for better clarity and readability. The legend: measured NRST and the measured-modelled NRST have the same line colours. Make different colour. Some lines are dashed or dotted but this does not appear in the legendOf course, the reader can then easily find out which line in the legend corresponds to which line in the graph, but it is confusing at first glance and does not support rapid overview of the Figure: make the legend consistent with the line style. The measured-modelled NRST is not shown at an appropriate scale. Why not displaying these differences in independent plots below the model output?

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