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

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GENERAL COMMENTS

This paper presents the spatially distributed application of the physical based model Alpine3D and its snow module SNOWPACK to the small-scale simulation of snow cover patterns and rock surface temperatures in two rugged, steep rock walls on the Gemsstock ridge in the central Swiss Alps. The topic is of high-interest for the scientific community studying mountain permafrost. In fact the distribution, persistence and consequently the thermal effect of snow cover in steep rock walls is poor known and its modeling is challenging due to the scarcity of field observations and the incapacity of the existing models to reproduce wind and gravitational transport of snow in steep topography. The field dataset used in this study has, in my opinion a very high potential. It consists of 30 rock surface temperatures aligned over a ridge cross-section, a 0.2m DEM derived from terrestrial laser scanner (TLS), 4 snow-depth maps derived from

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Discussion paper

TLS winter campaigns, meteo and snow-depth data from a near automatic weather station (AWS). The study spans two years with 2 complete winter seasons.

Despite these excellent premises, the objectives of the work are not well defined and it is difficult to understand what are the main results. The exposition is very fragmented, each system component (eg. Measured snow, modeled snow, measured temperature, modeled temperature,...) is treated separately and is very difficult to gain an overall view and draw more general findings and conclusions. In the discussion there is scarce attention in the references to plots and tables and some errors have been reported. Some speculative sentences have been reported in the conclusions. From the technical point of view I believe that, if one of the goal of this work is to reproduce carefully the thickness of snow on the rock wall (to assess the effect on surface temperatures), then the adopted precipitation scaling is not appropriate since it generates errors exceeding 0.5m which have huge effects on modeled temperatures. Finally, there are serious deficiencies in the use of references as well as in the choice of the statistics (R2 and MBE) used to evaluate the model performance. All these topics are further explained in the Specific Comments section below.

In conclusion I believe that this work has the potential for providing very important results for the scientific community but a big work of revision and reprocessing must be done before publication on TC. Major revisions.

SPECIFIC COMMENTS (MAJOR REVISIONS)

1. The approximate use of technical terms as well as of references (often totally wrong!) denotes the scarce attention paid by writing the introduction chapter. I suggest to the authors to deeply review this chapter by checking carefully the references (all along the paper!).

2. Sections 3.3.1 and 3.3.2 can be merged and shortened (mainly 3.3.1) by providing less detail about Alpine3D and SNOWPACK that are well known and documented models.

3. The precipitation scaling is a very promising idea but it does not seem to work very well as it is. It would be very interesting to understand why in 3 of the TLS campaign does not work providing quantitative analysis of these discrepancies (see technical comment). Moreover, looking at figure 2 is evident that it works quite well on the validation point N7 but is scarce at point S9. In my opinion a simple ratio between AWS snow-depth and TLS snow-depth is a too simplistic approach and represents the main limitation of the present study. I suggest the authors to put together all the TLS campaign data and AWS snow-depth data and try a more complex statistical approach which includes at least the topographical characteristics (ele, slp, asp) and doy (day of year) of the cells as scaling predictors. A first attempt could be to build a linear model with all the predictors, run a stepAIC on it for selecting the significant ones and use the resulting regressive model to scale the precipitation.

4. In my opinion the sections 3.3.4 and 4.4 are totally disconnected from other chapters, not in terms of concepts (energy balance is fundamental) but in terms of contents and argumentations. There are no links or references to what observed or discussed in the other sections, there is no think over possible source of modeling uncertainty, is just a chronicle on the course of each component along the seasons. I suggest the authors to remove these chapters, due to the already high number of data and elements to discuss. As it is, the energy balance discussion looks a digression that distracts the reader from the main subject of the paper. Alternatively the section 4.4 must be deeply reworked in order to provide precise evidence of what is discussed in the section 5.1 (Lines 471-484).

5. Section 4.1.1. The description of the measured snow cover variability by TLS is interesting but useless for the purpose of the paper and has scarce relevance for the scientific community because is too detailed and site-specific. It lacks effort to outline most general patterns of snow accumulation in steep rock walls. It would be very interesting to explore if in your dataset exists a relationship between snow-depth-TLS and steepness of the grid-cell. This analysis might be, I guess for the first time, a real mea-

sure of snow-depth in steep rock walls and provide the community some indications on the snow-depth thresholds to use for modeling experiments in steep rock walls. At first, this analysis (i) could exclude the cells above ledges and (ii) could analyze NW and SE faces separately.

6. Section 4.1.2. The statistics provided (R2 and MBE) are not sufficient. R2 indicates the fraction of variability (variance) in the observation that is explained by the model. Used alone it says little about model performance in strict sense because e.g. in case of temperature you can have an R2=0.99 with 10°C of bias. The modeling efficiency (ME) must be used also. MBE describes the direction of the error bias. Its value is related to the magnitude of values under investigation. A negative MBE occurs when predictions are smaller in value than observations, positive MBE occurs when predictions are greater in value than observations. In case of snow-detph has no sense to provide a mean value of MBE (-0.002 m!!) over the entire model domain because over- and under- estimations vanish each other. Mean absolute error (MAE) or root mean square error (RMSE) must be used instead. Also error bars in Fig.2 look strange, see technical comments. I suggest this paper for further detail: Mayer, D., and D. Butler (1993), Statistical validation, Ecological modelling, 68(1-2), 21–32.

7. Section 4.1.3. If one of the objective of this paper is (accurately) simulate the influence of snow cover on NSRT in steep rock walls I guess that differences in the order of 0.5 – 1m between observed and modeled snow depth is too much for obvious reasons. To reduce this uncertainty, as said in specific comments n.3, the precipitation scaling must be totally revised.

8. Section 4.2.2. This section would be a validation of NSRT but is very poor under this point of view. The absence of statistical metrics to evaluate model performance is evident here (see general comments). The description of discrepancies between obs. and mod. is only qualitative, comments are limited to temperature without any reference to the modeled snow which is the main constraining factor. In particular, observing together Fig.2a and Fig.3 results that temperature modeling has better perfor-

mance where snow modeling has worst performance (point S9). Nothing is said about that. This section, that potentially could be the core of the paper, must be strongly improved.

9. Section 4.2.3. The idea of a run with forced snow-free condition is good but results are not exploited at all. This run could be used as reference to quantify the potential thermal effect of snow cover at different slope and aspect (see Pogliotti, 2011). This is a way to generalize the results and valorize the dry run. Of course, the precipitation scaling must be improved before (specific comments 2).

TECHNICAL CORRECTIONS

- Line 29: the term “rock avalanche” refers to big falls of earth material (of up to millions of metric tons) able to reach velocities of more than 50 meters per second and leave a long trail of destruction. In the Alps such phenomena are not “numerous” (e.g. Val Pola 1987, Tschierva 1988, Brenva 1997, Thurwieser 2005) and even less those where permafrost can be directly listed among the trigger factors. The right term is “rock falls”.

- Line 30: strange references, Gruber & Haeberli 2007 is better and more comprehensive than Gruber 2004b, e.g. Fisher 2012 (Nat. Hazards Earth Syst. Sci) is missing.

- Line 31: Davies et. al 2001 is wrong! Gruber et. Al 2004a is wrong! Fisher 2012 (Nat. Hazards Earth Syst. Sci) is more appropriate than Fisher 2006, Gruber & Haeberli 2007 is missing, Allen & Huggel 2013 (Glob. and Planetary Change) is missing, Saas 2012 (Nat. Hazards Earth Syst. Sci) is missing, Deline et al. 2015 (Snow and Ice-Related Hazards, Risks, and Disasters, chapter 15) is missing. . . and many more.

- Line 35: Gruber 2012 is wrong! e.g. Guglielmin 2003 (Geomorphology) is missing

- Line 36: if you cite only Fiddes et al. 2015 add “e.g.” because exist more

- Line 37: kilometers

- Line 41: transient changes. . . Harris et al. 2009 alone has no sense because is a big

state-of-the-art of mostly all fields of research around mountain permafrost... Noetzli & Gruber 2009??

- Line 46-49: ...cannot capture. . . the ground thermal regime. I'm not sure of that. The Fiddes 2015 approach has not been yet validated against field measures.

- Line 56: remove "However"

- Line 56-58: this statement is too strong and do not consider that the temperature of a point in depth integrates the contribution of a certain area at surface. This area is wider as deeper is the point so the effect you are talking about is probably limited to few meters. Thus, in my opinion, to investigate the 3D subsurface heat flow is not necessary to reproduce surface temperatures with so-high spatial resolution. Please, reformulate this sentence considering also these aspect.

- Line 59-60: Gruber 2004 is wrong!, Gruber & Haberli 2007 is a kind of review and snow control only is mentioned, remove it. Pogliotti 2011 is probably the first work that systematically investigate the thermal effect of snow cover (moreover with high affinity with the present work) even in steep rock walls and is missing. Magnin 2015, Haberkorn 2015a & 2015b are missing too!

- Line 63: Pogliotti 2011 is wrong!

- Line 65: Gruber et al. 2004A is wrong!

- Lines 82-85: this sentence is not clear, explain better.

- Line 106: elevation range must be explicit in the site description.

- Line 127: Remove However. In this study, only data from. . .

- Lines 130-136: what you describe here is not evident neither from figure 1 nor from table 1 but just in figure 3. If you don't show a plot you have to describe better the differences you observe in the temperature fluctuations in order to justify your choices.

- Lines 191-194: the initialization is important. Provide here, synthetically, more details about initialization without reference to another paper. Is not clear as it is.

- Line 205: remove high resolution

- Line 211: Uncertainties in modeling...

- Line 213: R2 is the coefficient of determination! MBE is not the right statistic in this case, look at specific comments.

- Lines 209-213: move this paragraph as preamble of chapter 4.

- Lines 216-218: remove.

- Lines 222-224: what is the “snow depth driving mode” of snowpack? Something that convert snowfall in liquid precipitation? By which snow density value? This is a key step of your precipitation scaling, please explicit all the detail, synthetically, without references to other papers.

- Lines 225: “integrated” seems a mathematical term, please use a synonym.

- Line 228: replace “onto the DEM” with “in each grid cell”.

- Lines 228-232: replace this sentence with “cells where TLS data were non available have been excluded from the analysis”.

- Line 233: TLS campaign.

- Lines 233-241: explain better why you choose only the TLS of December 2013 for driving the precipitation scaling and provide quantitative proofs for this choice (model performance on modeled vs. observed NSRT). Look also specific comments.

- Line 247: see specific comments 4.

- Line 262: see specific comments 5.

- Line 277: see specific comments 6.

- Line 279: MBE = -0.002 m has no sense. MBE is the wrong statistic in this case (see specific comments).

- Lines 282-283: explain the method used for calculating the error bars and exactly what they represent. Is not clear. How can I have an error bar of $\pm$0.3m and a difference obs./mod. (red dot, red line) of about 1 m?

- Lines 300-301: explain/explore better the reasons of such a huge difference in S9.

- Line 287: see specific comments 7.

- Line 334: what does it means “auspicious accordance”? please try to be more adjective

- Line 335: MBE is the wrong statistic in this case (see specific comments).

- Line 330: see specific comments 8.

- Line 346: see specific comments 9.

- Lines 363-364: this sentence is ambiguous, what does it means “not pronounced as expected”? Expected for N/S differences (?) this is not the real case. Expected for snow-free, steep, conditions(?) this is not the real case. If you average all the measures of a mountain side like the yours, the value you got is exactly what I expected.

- Line 366: remove “compensating”

- Line 367: remove “In 2013-2014”

- Lines 367-370: respect the colon, merge these two sentences in one

- Lines 374-376: the higher SD of modeled temperatures derives essentially by the scarce ability in reproduce real (in terms of thickness) snow cover conditions on both sides.

- Line 378: how can you say that underestimation is mainly in summer? (fig. 3?). Explicit.

- Lines 379-380: remove “therefore”, this sentence is not a direct consequence of what you said before, or only partially. This is a comparison with the 3.6°C stated at line 363. Contextualize better this sentence.

- Line 384: compared to what? Modeled or real snow covered conditions? It is very difficult to follow your reasoning looking at Table 3 because the number in the text are often means of values in different columns of the table and moreover rounded! If you need these numbers add columns in the table!

- Lines 383-390: rework this section in accordance with the previous comment. Consider also the specific comments n.9

- Lines 392-399: very poor description. Provide more details or remove this section, figure 6 and the “grid” lines in table 3.

- Line 401: see specific comments 4.

- Line 447: modeling of water flow within fractures is not relevant for reproducing surface rock temperatures. Also the influence of surface water flow is negligible in comparison to a correct simulation of snow cover thickness.

- Line 451: check the references (see specific comments 1)

- Lines 452: please explicit the value of snow density used (see also technical comment Lines 222-224)

- Line 453: remove “However”

- Lines 454-455: the first half of the sentence (from However to AWS) is obvious thus can be removed, the second half is not clear, explain better this concept of non-linear settling. Include also the sentence after.

- Lines 457-458: this is not evident from your data. Look the table attached (Fig.1) and justify your sentence.

- Line 459-461: is not evident to me. Check the references (see specific comments 1)

- Line 462: what is the “apparent insulation”?

- Lines 465-466: heat flux at the bottom (20m below) cannot be seen in surface in so short simulations!

- Lines 468: remove “While”

- Lines 471-484: this is interesting but is very difficult to see the evidence of what you are saying in the plot 7 as well as find references in the text of section 4.4. See specific comment 4.

- Lines 485-486: move this in the results providing evidence of the source data. Keep in mind specific comments about the use of MBE.

- Lines 489-499: in my opinion this belong to section 5.1. Check the references (see specific comments 1) all along this paragraph.

- Line 500: replace “possibly made” with “introduced”

- Lines 504-505: looking at table 3 the warming effect on MANSRT is up to 3.7°C at N7 (2012-2013) and up to 1.5°C at S9 (2012-2013). Please keep attention and precision in reference to plot and table contents!

- Lines 508-511: this obviously depends on the amount of snow. A persistent thin snow cover has always cooling effect both at N and S faces, while a thick snow cover has warming effect. Thus the reason you observe on average a warming effect of snow cover is because you allow the accumulation of thick snow. If you have a look a other cells with thin snow I’m sure you can observe cooling effect between dry and snow simulation. So change this sentence keeping in mind also these aspect.

- Lines 515-520: this sentence is very interesting but not well introduced nor supported by findings of this paper. Provide more detail, evidence and argumentations in order to support this suggestion.

- Line 524: this section is very interesting and useful for the modeling community, but is poor of numerical evidences. Please, provide a synthetic table (or plot) where the influence of grid resolution on the model performance becomes evident (see also specific comments for assessing model performance in the correct way).

- Lines 551-553: I would say, “the results of the present work help to quantify the potential error...”

- Line 554-556: “Alpine3D simulates near-surface rock temperatures and snow depth in the heterogeneous terrain accurately.” in general this is true but is not the case of this work. The reason is that the precipitation scaling procedure is weak and provide unreliable precipitation input to the model. In my opinion this conclusion does not reflect the real result of this work.

- Line 556-558: lateral heat-flux is negligible in comparison to the effect of a bad precipitation input.

- Line 559-561: this is true, the potential of the dataset is very high but the choice of exploring just 2 cells on the N face and 2 cells in the S face strongly constrain this potential. See also general comments.

- Line 562: this sentence on the lateral heat flux is speculative. Nothing in the results provides the basis to verify this statement.

- Lines 569-571: also in that case no numerical evidence about model performance are provided in the results hence this sentence is speculative too.

FIGURES AND CAPTIONS

Table 3.

- Caption (Line 812), replace “data” with “cells”. How do you identified snow-free cells?

Figure 1.

- The boreholes are not considered at all in this work then I suggest to remove it from the figure and caption to avoid confusion.

- I suggest to replace the three colorful elevation plot by a “classic” but more readable cross-section along the logger line which easily can gives the information about elevation and steepness at one-shot. Figure 2.

- Just figures a) and f) are relevant for the interpretation and discussion of the precipitation scaling. Remove figures b) c) d) e) that are not relevant and enlarge figure a).

- The range in figure f) has been constrained at ± 0.5m for graphical reasons, but a frequency distribution plot (barplot) of differences on the model domain should be inserted as compendium to provide a comprehensive overview of modeled snow depth uncertainties.

Figure 3.

- Caption: dT are present also in the plots d) and e) not only in b) and c) as stated.

Figure 5.

- The boxplot shows the meadian but in the text and table 3 the references are always to the mean. Please modify the boxplot in accordance with the text.

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sensor	2012-2013		2013-2014	
	Δdays	ΔMANSRT	Δdays	ΔMANSRT
N7	-15	+1.1	6	-0.1
S9	-1	+1.2	-15	-0.2

Fig. 1.

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Discussion paper