

## Author final responses to Reviewers (Ref. No.: tc-2016-73)

The authors thank referee #2 for the useful remarks and suggestions. All referee' comments (left) and our responses to them (right) are listed below.

### Referee 2:

Referee comment	Author answer
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!)	The introduction will be reworked. Please see also answer 4 to referee 1.
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.	We will shorten section 3.3.1 and 3.3.2 in order to avoid repetitions. In addition we will merge sections 3.3.1 and 3.3.4. The surface energy balance is a core element of Alpine3D and belongs in the description of the energy balance model. Apart from the changes mentioned, the model description is already concise. Subsections 3.3.1 and 3.3.2 will still be treated separately for a better overview.
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	We agree with the referee that the comparison of the modelled to the measured snow depth data clearly showed discrepancies in modelling absolute snow depths. However, snow depth distribution and especially snow cover duration are reproduced nicely by the model. Well reproduced snow cover duration was found to be most important for modelling the ground thermal regime (e.g. Fiddes et al. 2015; Marmy et al. 2013), which becomes obvious in Fig. 3b, c. Please see also answer 34 to referee 1. Although a quantitative analysis of the precipitation scaling approach is currently being evaluated (Voegeli et al., submitted) and beyond the scope of this contribution, we performed some additional analysis. This has been done since both referees

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.

expressed concerns about the discrepancies resulting from precipitation scaling. An additional figure (histogram) will be provided in the results section in order to justify the choice of one TLS used for precipitation scaling. Please see this figure attached to this response letter (Fig. 1 for revision). Here solid lines illustrate the distribution of the ratio modelled/measured snow depth for the 4 TLS available. The TLS of 11 December 2013 (20131211, pink line) is centred by 1 (since this TLS was used for precipitation scaling). Snow depth is underestimated for the other 3 TLS campaigns, while using the TLS of 11 December 2013 for precipitation scaling. Based on the solid lines in the figure attached we think it might be better to use snow depths derived from the TLS 19 December 2012 for precipitation scaling. Dashed lines in the figure attached show an intercomparison between each TLS. First each pixel is corrected with the mean value of the TLS. Thus the relative snow depth per scan is calculated. Then the ratios of the relative snow depths of each TLS are compared to the other scans. For each pixel a ratio of 1 would imply that the ratio with the mean value is constant between TLS campaigns. Hence one can consider this to be the best possible result while building a statistical model. While comparing the envelope of the dashed and solid lines it becomes obvious that the scatter of the dashed lines is similar or larger than the precipitation scaling approach, especially for high-winter TLS. The scatter of the envelope is too wide to build a representative statistical model. We therefore come to the conclusion that the precipitation scaling is currently the best possible method to introduce varying snow depths into the rock

	<p>walls. It is also clear that the method is not perfect, but we consider this future research to improve.</p> <p>In addition it has been shown repeatedly (e.g. Lehning et al., 2011) that small-scale statistical modelling of snow depth based on terrain parameters does not work very well. This is why we decided to use the scaling approach based on the measured snow distribution. We will provide the figure attached (Fig. 1 for revision) and additional discussion regarding this point in the revised manuscript.</p>
<p>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).</p>	<p>Section 3.3.4 will be merged to section 3.3.1. Section 4.4 will be reworked. Please see also answer 2.</p>
<p>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 measure of snow-depth in steep rock walls and provide the community some</p>	<p>Section 4.1.1 will be reworked.</p> <p>The relationship between measured snow depth and slope angle will not be provided, since already enough methods and results are presented. Further it is not within the scope of this study and such an analysis will be presented elsewhere.</p>

<p>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.</p>	
<p>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.</p>	<p>The subsections 4.1.1 and 4.1.2 will be combined and lines 278-280 will be deleted. Please see also answer 29 to referee 1.</p> <p>Regarding the statistics used in this manuscript: first, MBE is important in case of snow since the bias over a whole area has huge implications. Second, the modelling efficiency is approximated by the <math>r^2</math>, even if root mean squared error or MAE are more common in some communities. In general, there is no single error analysis that says it all and every one is a little different. The choice of the authors to use <math>r^2</math> and MBE is not a bad one. However, as requested the MAE will additionally be provided.</p>
<p>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.</p>	<p>Please see answer 3, as well as answer 34 to referee 1.</p>
<p>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</p>	<p>Section 4.2.2 will be rewritten. From our point of view differences between modelled and measured data are quantitatively. Please see answer 6.</p> <p>A link to snow cover conditions in the rock walls has been done in lines 336-338. More details will be given here.</p>

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 performance 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.	It is correct that the ground thermal regime depends on snow conditions, but mainly on snow cover duration, not on absolute snow depths. Please see answer 34 to referee 1, as well as answer 3. Not only snow cover duration, but also ground conditions are important for near-surface rock temperature modelling. In the S facing slope NSRT can be simulated well since permafrost is absent in the S and most NSRT are around 0 °C below a thick snowpack. In addition the S rock surface is more homogenous (dip slope) compared to the N face (scarp slope). Thus the interaction between adjacent rock portions sticking out of the snow and rock portions covered by thick snow is reduced on the S face.
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).	A comparison between simulations of snow-covered and snow-free scenarios was done in order to quantify errors made while neglecting snow in steep rock wall thermal modelling. Please see answer 38 to referee 1, as well as lines 101-104, 242-245, section 4.2.3, 4.3.3, 4.3.4 with Fig.6 and parts of 4.4. The objective to run Alpine3D also with forced snow-free conditions might not have been clear. This will be clarified in the text.
10. 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, Tschierwa 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”.	Will be changed to ‘rock fall’.
11. 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.	Will be changed.
12. Line 31: Davies et. al 2001 is wrong!	Davies et al. (2001) and Gruber et al. (2004a)

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.	will be deleted. Other references will be provided, also with respect to your suggestions.
13. Line 35: Gruber 2012 is wrong! e.g. Guglielmin 2003 (Geomorphology) is missing	Gruber (2012) will be removed.
14. Line 36: if you cite only Fiddes et al. 2015 add “e.g.” because exist more	Will be changed.
15. - - - Line 37: kilometers	We will change ‘meters’ to ‘metres’, since British English is used throughout the manuscript.
16. 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??	Harris et al. (2009) will be removed. Noetzli et al. (2007) and Noetzli and Gruber (2009) will be moved at the end of the sentence.
17. 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.	Please see answer 13 to referee 1. The model results of Fiddes et al. (2015) were validated in the same publication against a network of air temperature, ground surface temperature and snow depth measurements, as well as data loggers (PERMOS) to evaluate ground surface temperature in coarse debris and bedrock.
18. Line 56: remove “However”	Will be removed.
19. 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.	The sentence will be reworked.
20. 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	Please see answer 14 to referee 1.

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!	
21. Line 63: Pogliotti 2011 is wrong!	Pogliotti (2011) will be removed.
22. Line 65: Gruber et al. 2004A is wrong!	Please see answer 16 to referee 1.
23. Lines 82-85: this sentence is not clear, explain better.	The whole sentence will be deleted.
24. Line 106: elevation range must be explicit in the site description.	Will be given.
25. Line 127: Remove However. In this study, only data from...	‘However’ will be deleted and sentence rewritten for better understanding.
26. 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.	Please see answer 23 to referee 1.
27. 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.	The sentences will be rewritten. However, all information regarding the initialization is given.
28. Line 205: remove high resolution	Will be removed.
29. Line 211: Uncertainties in modeling...	Will be changed.
30. Line 213: R2 is the coefficient of determination! MBE is not the right statistic in this case, look at specific comments.	Will be changed. Please see also answer 6.
31. Lines 209-213: move this paragraph as preamble of chapter 4.	The methods and results section will be reworked. This paragraph will possibly remain in the methods section.
32. - Lines 216-218: remove.	Will be removed.
33. 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.	The ‘snow depth driving mode’ means that SNOWPACK was driven with measured snow depth as model input (not liquid precipitation). SNOWPACK converts fresh snow falls in precipitation under consideration of snow settlement, as well as fresh snow density which are both calculated based on a statistical model. Although this is not a key step in our precipitation scaling, but rather a common approach to calculate liquid precipitation if only snow depth is available, we will provide additional explanation on this topic. Detailed

	information, however, is given in Lehning et al. (1999) and Wever et al. (2015). We think providing these references in the manuscript is sufficient. As you mentioned, SNOWPACK and Alpine3D are well known and documented models.
34. lines 225: “integrated” seems a mathematical term, please use a synonym.	Will be changed.
35. Line 228: replace “onto the DEM” with “in each grid cell”.	Will be changed.
36. Lines 228-232: replace this sentence with “cells where TLS data were non available have been excluded from the analysis”.	Will be changed.
37. Line 233: TLS campaign.	Not changed.
38. 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.	Please see answer 3.
39. Line 247: see specific comments 4.	Please see answer 4.
40. Line 262: see specific comments 5.	Please see answer 5.
41. Line 277: see specific comments 6.	Please see answer 6.
42. Line 279: MBE = -0.002 m has no sense. MBE is the wrong statistic in this case (see specific comments).	Please see answer 6.
43. 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 $\pm 0.3$ m and a difference obs./mod. (red dot, red line) of about 1 m?	<p>The error bars in Fig. 2a represent the errors only of the validation data itself. An error bar of <math>\pm 0.3</math> m is composed of both an error of <math>\pm 0.08</math> m due to errors of the TLS method itself and an error of <math>\pm 0.22</math> m inherited in the precipitation input data due to precipitation scaling.</p> <p>The highest inaccuracies of validation data occurred in areas with a strongly heterogeneous surface (N face).</p> <p>The error bars do not indicate differences between measured and modelled snow depth. The error bars in Fig. 2a might be omitted.</p>
44. Lines 300-301: explain/explore better the reasons of such a huge difference in S9.	Differences up to 1 m between measured and modelled snow depths in the S facing slope are mainly due to inadequate

	<p>description of snow settlement. This is explained in the discussion section 5.1 (lines 451-456).</p> <p>Lines 299-301 will be removed, since the results will be presented without any assessment or interpretation of the data. Possible explanations for model uncertainties are presented in the discussion.</p>
45. Line 287: see specific comments 7.	Please see answer 3, as well as answer 34 to referee 1.
46. Line 334: what does it means “auspicious accordance”? please try to be more adjective	Will be changed.
47. Line 335: MBE is the wrong statistic in this case (see specific comments).	Please see answer 6.
48. Line 330: see specific comments 8.	Please see answer 8.
49. Line 346: see specific comments 9.	Please see answer 9.
50. 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.	MANSRT differences between the NW and SE faces are smoothed due to thick snow. MANSRT differences between both faces would have been bigger if the slopes would have been snow-free, as it is often assumed in literature for steep rock faces. The text will be clarified.
51. Line 366: remove “compensating”	Will be removed.
52. Line 367: remove “In 2013-2014”	Will be removed.
53. Lines 367-370: respect the colon, merge these two sentences in one	Will be changed.
54. 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.	Please see answer 3, as well as answer 34 to referee 1.
55. Line 378: how can you say that underestimation is mainly in summer? (fig. 3?). Explicit.	The sentence will be deleted.
56. 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.	The sentence will be reworked.
57. Line 384: compared to what? Modeled or real snow covered conditions? It is very difficult to follow your reasoning looking at	Modelled MANSRT of snow-free simulations were around 2 ° C colder to both measured

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!	MANSRT and modelled MANSRT assuming snow-covered conditions. This will be stated in the text. In this section only the 2 °C value (line 384) was rounded. This will be clarified in the text. Other values can be calculated from Table 3.
58. Lines 383-390: rework this section in accordance with the previous comment. Consider also the specific comments n.9	Please see answer 57 above. The difference in line 88 is calculated for modelled snow-free conditions between the N and the S facing slopes. Please see answer 9.
59. Lines 392-399: very poor description. Provide more details or remove this section, figure 6 and the “grid” lines in table 3.	Please see answer 38 to referee 1.
60. Line 401: see specific comments 4.	Please see answer 4.
61. 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.	‘Water flow in fractures’ will be removed.
62. Line 451: check the references (see specific comments 1)	The references will be checked.
63. Lines 452: please explicit the value of snow density used (see also technical comment Lines 222-224)	SNOPWACK calculates fresh snow density for each time step by a statistical model. Please see answer 33. Lines 451-455 will be reworked for a better understanding.
64. Line 453: remove “However”	Will be removed.
65. 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.	Please see answer 63.
66. Lines 457-458: this is not evident from your data. Look the table attached (Fig.1) and justify your sentence.	Fig. 3b, c will be cited. In Fig. 3b, c it is shown that modelled and measured NSRT are in good agreement, although absolute snow depths vary by around 0.5 m. Please see also answer 34 to referee 1. In addition, snow cover duration for the loggers shown in Fig. 3b, c is given in Table 2, which will be also referred to.
67. Line 459-461: is not evident to me. Check the references (see specific comments 1)	Please see answer 44 to referee 1.

68. Line 462: what is the “apparent insulation”?	‘Apparent’ will be removed.
69. Lines 465-466: heat flux at the bottom (20m below) cannot be seen in surface in so short simulations!	Will be removed.
70. Lines 468: remove “While”	Will be removed.
71. 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.	References to Fig. 7 only belong to lines 477-480. Please see also answer 4.
72. Lines 485-486: move this in the results providing evidence of the source data. Keep in mind specific comments about the use of MBE.	Please see answer 45 to referee 1.
73. Lines 489-499: in my opinion this belong to section 5.1. Check the references (see specific comments 1) all along this paragraph.	This paragraph will not be moved to section 5.1, since model uncertainties are not discussed in this paragraph.
74. Line 500: replace “possibly made” with “introduced”	Will be changed.
75. 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 504-505 refer to the entire rock wall (Table 3) not to single locations (Table 2). We will cite Table 3 and clarify the text.
76. 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.	The influence of snow on mean annual rock temperatures close to the surface of course depends on snow depth and especially on snow cover duration. In this study snow accumulates for around 9 months a year and has a warming effect on bot NW and SE faces. The effect of thin snow on rock surface temperatures, especially on mean annual temperatures is still poorly studied. Whether thin snow has a cooling or warming effect on mean annual rock temperatures on both N and S faces strongly depends on snow cover duration. Thin snow < 0.2 m will not persist on S faces for several months, especially not during the months with most intense radiation and its effect on mean annual rock temperatures is still not clear and should be better investigated in future.

	The contradiction of the presented results to previous studies (e.g. Hasler et al. 2011; Magnin et al. 2015) will be discussed more differentiated and the sentence will be reworked.
77. 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.	Please see answer 47 to referee 1.
78. 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).	Please see answer 51 to referee 1.
79. Lines 551-553: I would say, “the results of the present work help to quantify the potential error...”	Sentence will be reworked.
80. 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.	Sentence will be reworked.
81. Line 556-558: lateral heat-flux is negligible in comparison to the effect of a bad precipitation input.	Please see answer 3, as well as answer 34 to referee 1. Paragraph will be reworked slightly (lines 554-558).
82. 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.	Please see answer 30 to referee 1.
83. Line 562: this sentence on the lateral heat flux is speculative. Nothing in the results provides the basis to verify this statement.	Sentence will be removed.
84. Lines 569-571: also in that case no	Please see answer 51 to referee 1.

numerical evidence about model performance are provided in the results hence this sentence is speculative too.	
85. Table 3: Caption (Line 812), replace “data” with “cells”. How do you identified snow-free cells?	<p>The sentence in lines 811-812 refers to the model run considering snow (in Table 3: modelled N grid snow &amp; modelled S grid snow) and to the model run lacking snow (in Table 3: modelled N grid snow-free &amp; modelled S grid snow-free), in the latter the precipitation input was forced to be zero. Modelled results given in the respective lines of Table 3 were averaged over the entire N and S facing model domain. Thus a comparison between the run considering snow and the run without snow has been done.</p> <p>We might replace ‘data’ with ‘runs’. In this case ‘cells’ are wrong. We will rework the table for better understanding.</p>
86. 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.	<p>The 30 shallow NSRT logger locations were used to validate model results. Please see answer 30 to referee 1. The horizontal borehole (points BH N and BH S in Fig 1a, e, f), which was drilled through the whole ridge, provided rock temperature data in various depths, which were used to initialize our model (Section 3.3.2). We will therefore not remove the boreholes.</p>
87. 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.	Will be changed. Please see also answer 52 to referee 1.
88. 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).	We will revise Fig. 2. Please see answer 29 to referee 1.
89. 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	We will either provide a scatter or a bar plot to show differences between measured and modelled snow depth. Please see also answer 29 to referee 1.

inserted as compendium to provide a comprehensive overview of modeled snow depth uncertainties.	
90. Figure 3: Caption: dT are present also in the plots d) and e) not only in b) and c) as stated.	The caption was ambiguous. We meant that dT was calculated in Fig. 3b, c between measured and modelled snow-covered conditions, although snow-free conditions were also shown. In Fig. 3d, f dT is calculated between measured and modelled snow-free conditions. Will be reworked.
91. 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.	In the boxplots the mean will also be provided.
91. 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.	In the boxplots the mean will also be provided.

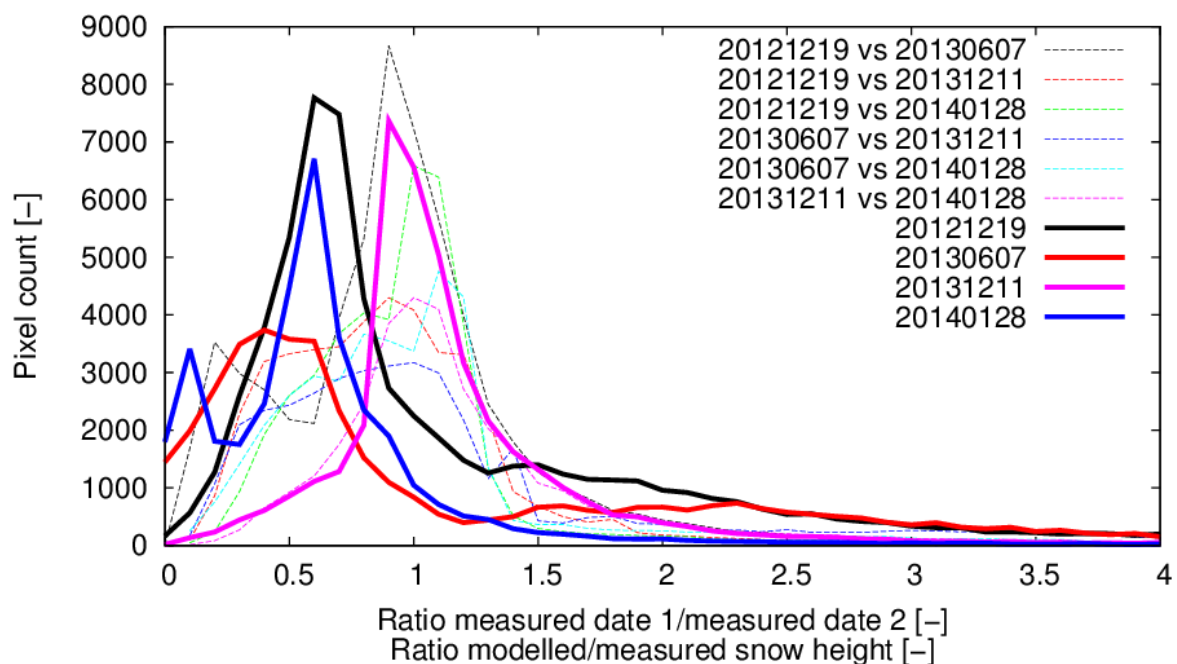


Fig. 1 for revision: Histogram for TLS data: solid lines illustrate the distribution of the ratio modelled/measured snow depth for the 4 TLS available. The TLS of 11 December 2013 (20131211, pink line) is centred by 1 (since this TLS was used for precipitation scaling). Dashed lines show a comparison between each TLS. First each pixel is corrected with the mean value of the TLS. Thus the relative snow depth per scan is provided. Then the ratios of the relative snow depths of each TLS are compared to each other.

## Abbreviations

AWS: automatic weather station

DEM: digital elevation model

ILWR: incoming longwave radiation

IMIS: Intercantonal Measurement and Information System

ISWR: incoming shortwave radiation

MAE: mean absolute error

MANSRT: mean-annual near-surface rock temperature

MBE: mean bias error

NSRT: near-surface rock temperature

NW: north-west

$r^2$ : coefficient of determination

SE: south-east

TLS: terrestrial laser scanning

## References used in the response to referees

- Davies, M.C.R., Hamza, O., and Harris, C.: The Effect of Rise in Mean Annual Temperature on the Stability of Rock Slopes Containing Ice-Filled Discontinuities, *Permafr. Periglac. Process.*, 12, 137-144, doi:10.1002/ppp378, 2001.
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