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

The authors thank referee #1 for the useful remarks and suggestions. All the referee' comments (left) and our responses to them (right) are listed below.

General comments:

To evaluate the performance of the model applied, model results of snow depth and rock temperatures in 10 cm depth (NSRT) were compared to detailed measured validation data (snow depth of 3 independent TLS, 30 NSRT measurement locations). An error analysis of snow depth and NSRT (MBE, r^2) was performed at each of the 30 NSRT locations. While analysing the influence of snow on rock temperatures for the entire N and S facing rock walls, means of all NSRT loggers were calculated. The same is true for the error analysis.

Since it would be too much to show model results and their comparison to measured data, only 4 of the NSRT logger locations were chosen to show in detail. These 4 temperature loggers were chosen in order to represent typical NSRT evolutions depending on whether the location is snow-covered or not. For these 4 locations single error analyses are presented, but the error analysis for the 30 other NSRT loggers are also presented. According to both referees, that was not clear from the manuscript. We therefore will clearly state this.

Referee 1:

Referee comment	Author answer
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?).	We will rewrite the abstract after revising the whole manuscript. The research questions and objectives of this study will be clarified. In addition the main results will be clearly emphasized. The contradiction of the presented results to previous studies (e.g. Hasler et al. 2011; Magnin et al. 2015) will be discussed in more detail in the discussion section, but will not be provided in the abstract. There are still open research questions, which our study does not answer (e.g. thin snow > cooling yes or no).
2. Improve the presentation of the methods	An additional figure in form of a flow chart

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.	will be provided in order to present an overview of the methods/ data used for both driving and validating the model. We will adapt the chronology of the methods section based on this flow chart. In addition to the flow chart we will provide a short introduction in the methods section.
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	The structure of the paper will be revised. At the end of the introduction we will clearly state the aims and objectives of our study for a better understanding of the whole manuscript. According to this the methods and results section will be reorganised. Some introductory sentences will be provided in the beginning of both the methods and results sections to better lead the reader through the manuscript. It will be clearly distinguished between model results and validation data. Concerning the results section: first modelled and measured snow cover data will be presented since the snow strongly influences the thermal regime of the rock walls. The snow cover section (4.1) will be revised to clarify various confusing points. Fig. 2 will correspondingly be adapted (please see specific comments). In a next step the NSRT data will be discussed. The usage of NSRT data of the 30

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 R2 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	loggers for model validation will be better explained. In order to understand differences between modelled and measured NSRT data better, the topographic differences between the validation locations and their location in the model domain will be provided in an additional Table in the appendix. The references to figures in the results section will be shortened for better reading and only the most appropriate ones will be cited. In addition we will clearly distinguish between results and discussion. The calculation of the statistics between model and validation data will be addressed in the methods section for a better understanding. Only results of the statistics will then be given in the results section. The contradiction of results with previous studies (e.g. Hasler et al., 2011) will be discussed in more detail in the discussion section. We will provide an explanation of possible factors leading to this contradiction and will connect our findings to findings in literature of steep rock wall temperatures (e.g. Hasler et al., 2011; Magnin et al., 2015; Myhra et al., 2015). Associated to this we will also emphasize more Fig. 6, which is the core of our modelling study. To do so, we most likely merge section 4.3.4. with the other sections of 4.3.

snow covered slopes. This is well aligned with recent findings in Norway (Myhra et al., 2015) and should be better emphasized and discuss.	
4. 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.	The references to the literature are often too generalised. Therefore we will revise the references in the introduction and will check references throughout the manuscript.
5. 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.	The first paragraph will be rewritten in order to emphasize the need to study (measure and model) rock wall permafrost. The application of modelling mountain permafrost occurrence correctly over large areas, such as the Alps will be introduced elsewhere in the introduction.
6. 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.	An additional table giving aspect and slope measured in ‘reality’ and in the model domain (based on the DEM) for all 30 NSRT logger locations will be inserted, most likely in the appendix. In addition the interpretation of measured and modelled data will be improved with a special focus on the real aspect (NW/SSE) of the rock walls.
7. Lines 20-21: is this sentence written in proper English? It seems confusing.	The sentence will be rewritten.
8. Line 29: what does “large” mean? Some rock falls affected “narrow” rock faces, pinnacles, ridges... Is this word really appropriate?	‘Large’ will be deleted.
9. Line 31: Davies et al. 2001 didn’t not investigate the stability of permafrost in high Alpine regions but proposed a laboratory	Davies et al. (2001) and Gruber et al. (2004a) will be deleted.

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?	
10. 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.	Gruber (2012) will be deleted.
11. Line 36: there are better examples than Fiddes et al. 2015 as numerical modelling of mountain permafrost (especially in rock walls).	The reference Fiddes et al. (2015) refers to physics-based modelling of permafrost distribution in the European Alps. We think this reference is appropriate in this context, but of course just one of many examples. Since the first paragraph will be rewritten in order to point out the need to study rock wall permafrost, we will provide other references.
12. 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).	Harris et al. (2009) will be deleted. Noetzli et al. (2007) and Noetzli and Gruber (2009) will be moved at the end of the sentence.
13. 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?	Fiddes et al. (2015) cannot capture the small scale variability because of too coarse spatial resolution of the approach used. The sentence will be rewritten for a better understanding.
14. 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.	The three references will be replaced with more appropriate references on snow control in steep rock walls, such as Haberkorn et al. (2015a,b), Magnin et al. (2015), Mhyra et. al. (2015) and Pogliotti (2011).
15. 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	Pogliotti (2011) will be deleted.

processes directly such as suggested by this reference.	
16. 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.	Correct. Gruber et al. (2004a) will be replaced with references to studies dealing with snow in steep rock walls, such as Haberkorn et al. (2015a), Sommer et al. (2015) or Wirz et al. (2011).
17. 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.	The sentence will be deleted.
18. 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.	Please see answer 17.
19. Line 89: What is the difference between NRST and the “rock thermal regime”? Do you mean the thermal regime at depth?	The rock thermal regime close to the surface and at depth is meant. We will rewrite the sentence accordingly.
20. Lines 118-119: could you explain why did you choose this reference period? Data availability?	The study period from 1 September 2012 to 31 August 2014 was chosen in order to present 2 years of complete meteorological input, as well as validation data (TLS, NSRT).
21. 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.	The iButtons were installed on 9 July 2012. In this study we focus on the investigation period from 1 September 2012 to 31 August 2014, which is stated in section 2. The date of installation of NSRT loggers will not be provided in the text, since it is not relevant for the reader.
22. Line 127: here also I don’t understand the	‘However’ will be deleted and sentence

meaning of “However,”.	rewritten for better understanding.
23. 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 127-136 will be rewritten in order to describe better the snow/ no snow influence on NSRT. In addition it will be referred to Fig. 3 to better show the features described.
24. 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?	The whole section 3.3 will be shortened (please see also answer 2 to referee 2). Therefore this sentence will be rewritten and the depth, as well as the magnitude of the geothermal heat flux at the lower boundary will be provided.
25. 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 model results?	Data from the AWS Gütsch, maintained by MeteoSwiss, were used for gap filling for all parameters of the meteorological data series of Gemsstock from 22 March to 15 April 2013, as well as for correcting the erroneous ISWR measured at Gemsstock between 1 September 2012 and 15 April 2013. For the corrected ISWR in 2012 the mean absolute error was 14.4 W m^{-2}, the mean bias error was 8 W m^{-2} and the root mean squared error was 30.2 W m^{-2}. Calculated errors are reasonable, since the radiation sensor accuracy is ±20 W m^{-2}. In order to parameterize ILWR between 1 September 2012 and 15 April 2013, a combination of a clear-sky algorithm developed by Dilley and O’Brien (1998) and a cloud correction algorithm from Unsworth and Monteith (1975) is applied. For the all-sky ILWR the mean absolute error was 26.8 W m^{-2}, the mean bias error was –6.3 W m^{-2} and the root mean squared error was 31.9 W m^{-2}. Hence, parameterization errors are reasonable compared with the error range suggested e.g. by Flerchinger et al. (2009) for the combination of the Unsworth cloud correction and the Dilley clear-sky algorithm (root mean squared error of 27.1 W m^{-2}). Gaps in snow depth data were filled based

	on similarity with data from adjacent stations using geostatistical interpolation tools. Snow depth from 10 surrounding IMIS AWS within a distance of 20 km and from AWS Gütsch served as correction data for the snow depth of Gemsstock. Stations are located in flat terrain and cover all directions to consider different air flows at each station. Detrended weighting procedures were applied to account for elevation differences between Gemsstock and the neighbouring stations. The presented correction methods may are inappropriate to determine ISWR and ILWR exactly at one certain point in time, but are considered to be an acceptable solution for the input of an energy balance model running on a multi-annual timescale where the conservation of natural variability of the model input variables is much more important than the projection of single time steps. We think that gap filling only induces a minor part of the bias in model results and a meteorological error analysis is not within the scope of this study. All this information will therefore not be provided in the manuscript. The correction and error analysis are well documented in Haberkorn et al. (2015b).
26. 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?	Down to 0.5 m depth 99% solid and 1% pore space containing ice or water was assumed to account for near-surface fracture space. Between 0.5 and 20 m depth 100% un-fractured, solid rock was assumed. Further, it is not the scope of this study to model the influence of fractures on rock temperatures, which we addressed (but did not model) in Phillips et al. (2016). Although the geothermal heat flux is most likely negligible in the narrow and steep Gemsstock ridge, a geothermal heat flux (here: 0.001 W m^{-2}) had to be applied as

	lower boundary condition of the model. To ensure a marginal impact of this boundary condition on the analysed rock thermal regime close to the surface, it was important to model deep into the rock. We chose to model down to 20 m depth, since detailed rock temperatures for initializing the model were available from an on-site borehole. Model uncertainties resulting from the use of the geothermal heat flux as the lower boundary condition were evaluated in 1d SNOWPACK test simulations at the borehole location at Gemsstock. Here, modelled rock temperatures accord well with borehole rock temperatures measured at various depths down to 15 m for both NE- and SW-facing locations (r^2 = 0.6–0.88). Correlation decreases with increasing depth, since modelled temperatures are biased by the geothermal heat flux. Consequently, simulated rock temperatures could be considered to depths of approx. 10 m at Gemsstock. This data however, will be presented elsewhere. The physical properties of the granodiorite bedrock used for ground modelling are discussed in Haberkorn et al. (2015b). For consistency the same bedrock properties are applied.
27. Line 233: Wouldn't be "one" instead of "an" in "an Alpine3D run"?	Will be changed.
28. 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?	We will provide an additional figure (histogram) to justify the choice of one TLS as precipitation scaling input. Please see also answer 3 to referee 2.
29. Lines 278-280: it is not really easy to report the mentioned results to the figure. On which data are the R2 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	The results section 4.1 will be reorganized and rewritten, since it is confusing. Subsections 4.1.1 and 4.1.2 will be merged. Thus the sentences in line 278-280 will be deleted. In addition Fig. 2 will be reworked in order to provide more meaningful snow depth

would help the reader to better see the comparison between modelled and measured values.	information. Subfigures 2b, c, d, e will be replaced and instead three subfigures each will be shown on: independent TLS data, differences between the independent TLS data and model results at date of the TLS campaigns, as well as scatter plots of measured and modelled snow depth. For your clarification: the r^2 and the MBE were calculated between the measured snow depth (TLS) at 11 December 2013 and the scaled snow depth (precipitation scaling) at the same date.
30. 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 R2 analysis.	It is correct that the more validation data the better in order to provide robustness to the statistics applied (MBE, r^2). The MBE and r^2 error analysis between measured and modelled NSRT data was performed for each of the 30 NSRT loggers (section 4.3, Fig. 5, Table 3), but only NSRT and snow depth data of 4 loggers (representative for typical snow conditions in the rock walls) were presented in detail (section 4.2, Figs. 3, 4, Table 2), since providing data from all loggers would be too much. This will be clearly stated in the text and the appropriate sections (last section in introduction, methods, results) will be reworked accordingly, since it seems that the use of all 30 NSRT loggers was not clear for the reader. For instance in Table 3 data of all 30 NSRT loggers are used.
31. 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". "R2=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	Correct. The TLS of 11 December 2013 was used to scale the precipitation for model input. Only three independent TLS are therefore available for model validation. This will be changed in the text. The r^2 = 0.95 is a mean calculated between modelled and measured snow depth for each grid cell and each of the TLS. MBE, r^2 and MAE (please see answer 6 to referee 2) between the modelled and the independent measured snow depth data of

plot or at least better illustrate the model output by e.g. replacing one of the 3D or 2D view in Figure 2?	each of the three independent TLS campaigns will be provided in the revised manuscript. In addition Fig. 2 will be revised (please see answer 29).
32. Line 296: Is the term "validation" really appropriate?	‚Validation' is correct, but only for the three independent TLS campaigns. This will be rewritten in the text and figure caption.
33. 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/overestimations 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.	The misfit between modelled and measured snow depths (lines 299-301) is also valid for lines 290-292. Possible explanations for model uncertainties are again presented in the discussion section. Hence, the results will be presented without any assessment or interpretation of results. Lines 299-301 will therefore be deleted.
34. 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?	Although measured and modelled snow depth differences were > 0.5 m (especially on the S slope), these snow depth differences do not affect the rock thermal regime as long as snow depths are > 0.2 m. Steep, bare rock is decoupled from atmospheric influences for snow depths exceeding 0.2 m (Haberkorn et al. 2015a). Amongst others, Luetschg et al. (2008) and Zhang (2005) stated that the influence of snow depth variations on ground temperatures in the presence of a thick snow cover are small, whereas snow depth variations only have strong effects on the ground thermal regime for thin snow cover (in steep, bare rock we found the threshold to be 0.2 m). Of course such big snow depth differences might have an effect on the snow cover *duration*. However, both snow cover timing and duration were reproduced nicely by the model, which can be observed comparing measured and modelled NSRTs in Fig. 3b, c and Table 2 (snow cover duration). It has been shown repeatedly that realistically

	modelled snow cover duration over the winter is more important than accurately modelled snow depths at certain points in time (e.g. Fiddes et al. 2015; Marmy et al. 2013). While absolute snow depths were underestimated by Alpine3D, the well modelled snow cover duration implies an underestimation of snow melt in the model. This may be at least partly explained with the 1d snow module which does not account for 3d heat flow between adjacent snow-free and snow-covered rock portions, as well as micro-meteorological processes due to uneven heating during the ablation period which accelerate snow melt in reality. We will amend the manuscript regarding this issue.
35. Lines 342-344: why such a big bias (-2_C)? What is its implication in the overall results?	Likely explanations of model uncertainties are given in the discussion. Especially at locations lacking snow the underestimation of modelled NSRT may result from both air temperature and wind speed differences between rock walls and the flat field AWS. Air temperature and wind speed measured at the AWS may be a poor surrogate for the prevailing conditions in steep rock. Hence the turbulent flux simulations are biased (provided in discussion lines 480-483). Further, also differences in slope and aspect between the model domain and reality can be a possible error source (please see answer 6). The effects of too cold modelled NSRT at locations lacking snow are shown in Fig. 5. Boxplots representing model results show a bigger scatter.
36. Lines 362-364: the difference is calculated using the 30 NRST time series?	Correct. This is mentioned in lines 357-359. The text will be clarified, since it might not be clear.
37. 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!	We will clarify the text. Although NW and SE facing rock slopes are considered, the NSRT differences are still smoothed due to thick snow.

38. 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?	To do so we will rewrite Section 4.3.4, also for better understanding. The value of 1.9 °C is the difference of MANSRT averaged over the whole model domain (taking into account each pixel regardless of aspect) between the modelled snow-covered and the modelled snow-free scenario. For the snow-free scenario the precipitation input of the model was forced to be zero (explanation in lines 242-245).
39. 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?	Comparing the energy balance of both snow-covered and snow-free (see explanation above) scenarios at location N7 reveals important differences especially in May, June and July (ablation period): for snow-covered conditions (Fig. 7a) all available energy is used to melt the snow, indicated by the snow melt term Q_{melt}. In contrast during the same period all available energy is used to directly warm the rock assuming snow-free conditions (Fig. 7b), which is then compensated by the sensible heat flux. These differences in energy fluxes between snow-covered and snow-free scenarios result in totally different NSRT evolution. In addition different albedo effects arise between both scenarios (snow versus bare rock).
40. Line 367: "effects: In" is either "effects. In" or "effects: in"	Will be changed.
41. 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!	'Water flow along fractures' will be removed.
42. Line 450: the consideration of snow cover at the ground surface is especially important to model small scale temperature variability.	It is true, that in near-vertical, ideal, snow-free rock faces air temperature and solar radiation might be sufficient to model

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.	ground surface temperatures. However, in fractured, structured and variably inclined rock faces this is not the case and the snow has to be taken into account, as already stated e.g. in Haberkorn et al. (2015a,b) or Magnin et al. (2015).
43. 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?	We will cite Fig. 3b, c. Here 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.
44. 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”.	We will rework this sentence. Gruber et al. (2004a) and Noetzli et al. (2007) do not account for snow in idealized slopes > 50°.
45. 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)?	This sentence will be moved to the results section 4.3.3 and will be reworked for better understanding. In addition we will refer to Table 3. The MBE analysis was calculated between measured and modelled NSRT data for each of the 30 NSRT logger locations. The average MBE was than calculated for the entire N and S facing slopes while averaging all MBE of N facing locations and averaging all MBE of the S facing locations. Hence the average MBE error of -0.2 °C in the N slope and of -1 °C in the S facing slope include all N and S facing locations (30) and thus account for the various snow conditions in the rock walls. The MBE addressed in line 342 is only calculated between measured and modelled NSRT of one N (N3) and one S (R2) facing location lacking snow.
46. Lines 504-505: Isn’t the difference of snow free/snow covered faces between N/S aspects in the range of model uncertainty?	In the N facing slope the NSRT difference between snow-free and snow-covered scenarios is up to 2 °C, while the MBE is only -0.2 °C (please see lines 485-486 and comment above). In the S facing slope the NSRT difference

	between snow-free and snow-covered scenarios is up to 1.4 °C, while the MBE is up to -1 °C (please see lines 485-486 and comment above) and therefore close to the model uncertainty range. The differences given are calculated between modelled snow-free and modelled snow-covered scenarios (Fig. 5) and hence differences are relative. For both the point and spatial scale snow-covered scenarios are always warmer than snow-free scenarios.
47. 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.	This section will be better emphasized. Accordingly, the results will be improved and rewritten.
48. 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?	Please see answer 30. 'Validation' is correct, since independent measured NSRT data is compared to modelled NSRT data. In addition the error analysis is also based on this data.
49. Line 553: "50_", how this threshold has been defined? It appears for the first time in the conclusion.	This topic is addressed in the introduction (lines 60-63). It was in general assumed that wind and gravitational transport remove the snow from steep rock in slopes exceeding 50 to 60°. Please see also answer 44. The threshold of 50° will be stated in the last part of the introduction for better understanding.
50. Line 554: is "accurately" really appropriate when significant bias have been displayed?	Sentence will be reworked.
51. 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.	Another short chapter will be provided in the results (or at least a Table with simulation results of 0.2 m, 1 m and 5 m) in order to prove this statement.
52. Figure 1c: what are the peaks between 2930-2950 and 161750-161780 on the y and	The peaks in Fig. 1c are artefacts in the DEM due to the projection of overhanging rocks.

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.	Fig. 1c and 1d will be removed (comment 87 of referee 2) and replaced by a profile through the ridge for a better overview of the linear logger layout, elevations and slope angles. In Fig. 1a slope angle colours will be displayed in black and white to improve the visibility of the micro-topography. Fig. 1a then resembles a hillshade with an illumination angle of 90°.
53. Figure 2a: it is very difficult to read the legend, could you make it bigger?	Will be changed.
54. 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?	The line colours of the measured NSRT and the measured-modelled NSRT have different blue shades. They might be difficult to distinguish. Therefore we will change line colours. In addition we will modify the legend and will provide a legend which is consistent with the line style. The measured-modelled NSRT will not be moved to an independent plot, since these graphs shall only provide a quick overview on differences between measured and modelled results.

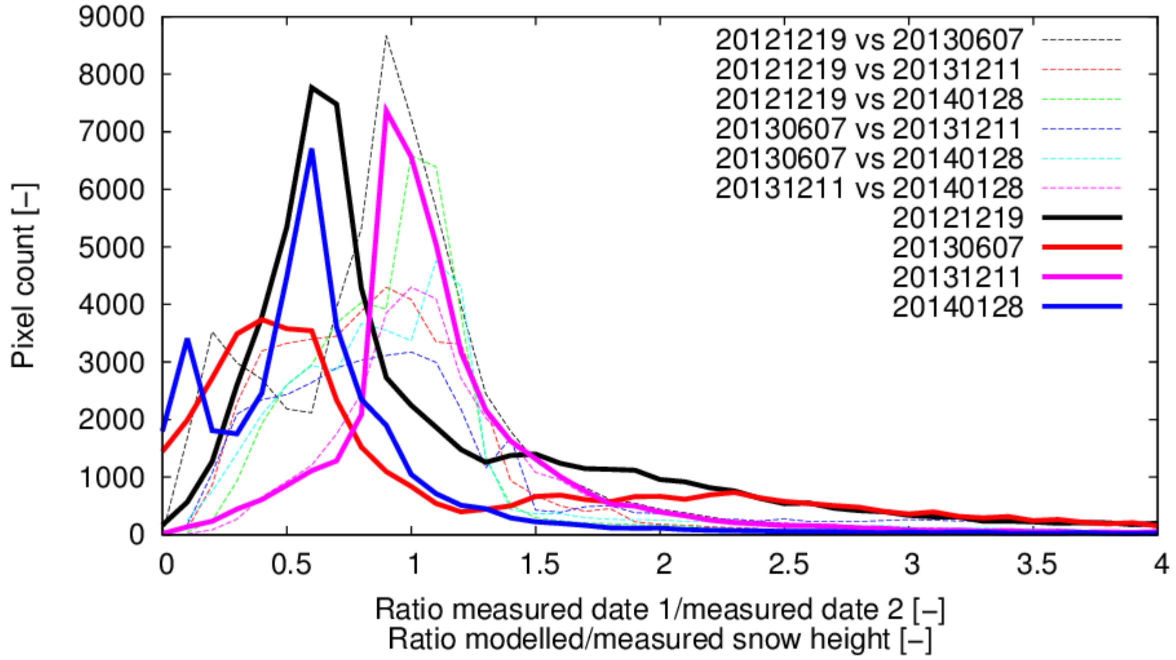

Fig. 1 for revision: Histogram for TLS data: solid lines illustrate the distribution of the ratio modelled/measured snow depth for the 4 TLS available. The TLS of 11 December 2013 (20131211, pink line) is centred by 1 (since this TLS was used for precipitation scaling). Dashed lines show a comparison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

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