We thank the reviewer for constructive criticism and comments which have significantly improved our manuscript. In the following, we provide point-by-point replies to all issues raised. The reviewer comments appear in black/italics, and our answers in blue/normal font.

In the paper "Estimating surface water availability in high mountain rock slopes using a numerical energy balance model" by Matan-Asher et al., the authors measured snow depth on a steep south-facing slope of the Aiguille du Midi (AdM) in the Mont Blanc Massif. They used their data to calibrate a gridded snow depth data set and used this data set to drive the energy balance model CryoGRID. The model results enabled the quantification of individual processes contributing to snowmelt and their temporal (seasonal) and spatial (elevation) variation. The author related the snowmelt to geomorphic processes as permafrost degradation and landslide activity (e.g. rockfall).

Studies on snow cover and snowmelt in high alpine environments are rare as these processes are very dynamic with high temporal and spatial variation, which makes these processes very difficult to assess. Therefore, the novelty of the approach is very high and the snow results are of interest to a scientific community working on hydrological and thermal research questions in Alpine environments. However, the manuscript has substantial shortcomings especially as it lacks to connect snowmelt to geomorphic processes, which is of major interest for readers of Earth Surface Dynamics. For example, the authors measured and modelled what happens on the surface but the manuscript fails to explain how snowmelt is related to thermal processes as permafrost or maybe better active-layer thaw or frost cracking. Furthermore, the link between thermal or hydrologic processes driven by meltwater and landslides is not clearly established in the introduction and later picked up in the discussion. The novelty of this paper is quantifying snow and snowmelt, and their influence on the energy balance, which makes the manuscript maybe more suited for "The Cryosphere".

The research on mountain permafrost is relatively new and fast-growing. Much research, including by some of the coauthors of this manuscript, is directed to better understand the connection between the thermo-hydrological and geomorphic processes in Alpine environments. This manuscript aims to decipher one of the most important and poorly constrained factors that control that connection - water input, and is focused on the novel modeling approach. It is for that reason that we decided, after much consideration, to submit this manuscript to this journal, and not to a cryosphere-oriented journal such as "The Cryosphere".

The reviewer rightfully lists several geomorphic processes that are directly influenced by water input. Following the reviewer's comments, we tried to make the connection to geomorphic process clearer in the first 15 lines of the introduction. However, we think that it is far beyond the scope of this contribution (or perhaps not even any single contribution but many) to cover thoroughly these geomorphic processes. We believe that further research of many alpine geomorphic processes will gain from our study. We added a more general sentence in the introduction section about the aims of the study to make them clearer: "This study is aimed to decipher the availability of surface water for surface and hydrological processes".

In addition, I got some major comments on (1) the structure, of the paper, (2) the inadequate presentation of the applied methods, (3) the presentation and discussion of results.

(1) The paper especially the introduction is poorly structured as it is separated into two sections. The first section focusses on water and rockwall instabilities and mixes up many terms (infiltration water, surface moisture) with different geomorphic (frost cracking, permafrost degradation) or mechanical processes (subcritical cracking) without explaining terms and processes sufficiently. Therefore, the links between hydrologic, thermal and geomorphic processes remain unclear. For example, as geomorphic processes occur at different rock mass depth, it remains unclear how permafrost degradation occurring on time scales > 2 years are linked to snowmelt occurring in spring or summer at the surface. The second section focusses on snow in steep rockwalls. These sections should be united in one introduction with one paragraph introducing clearly the objectives of the paper and the applied techniques to address the objectives.

Thank you for this constructive comment. We combined the introduction sections, as suggested, and the text was edited based on the reviewer's comments. Among the changes, it now includes a new paragraph that describes the research objectives and applied techniques: "This study is aimed to decipher the availability of surface water for surface and hydrological processes. To do so, we use a numerical energy balance model coupled with a state-of-the-art snowpack scheme, forced by field measurements, to simulate hydrological and thermal processes at the surface, and quantify the flux of excess water that is available for infiltration."

The study site should include more information on the Mont Blanc Massif, permafrost distribution and rockfall that the authors collected and published in numerous papers. They use the Aiguille du Midi to calibrate their model but model the snowmelt for higher and lower elevations. How representative is the AdM for rockwalls within the Mont Blanc massif? Can the authors provide more information on slope angles, rockwall distribution and rockfall for elevation ranges? The upscaling of results to different elevations is a key result but currently the consequences for thermal and geomorphic processes at regional scale are difficult to assess for the reader.

We absolutely agree with this comment and the section describing the study site was greatly elaborated with information on the geology, topography, permafrost distribution, and ongoing research.

(2) The method section raises more questions than providing answers. The authors produced a 3D point cloud and it is unclear how the data was collected (UAV)? If an UAV was used it would be interesting what kind of UAV? What kind of sensor was used (LIDAR, photo)? What kind of resolution have the point clouds? The authors seemed to calculate a difference model from the point clouds to quantify maximum snow cover and it would be of interest what the level of detection and the uncertainties are as the maximum snow cover is a key parameter for the modelling approach. More information on the data acquisition and processing is needed. How was the data georeferenced in a high alpine area with snow cover that prohibited the use of ground control points? What software was used for data processing? All the information is missing but necessary to understand the data set used to drive the energy balance model.

Thank you for pointing this out. The methods section on spatial analysis of snow depth was edited and the part about the use of UAV and point cloud production was elaborated, and it is now much improved. We also added a table that summarizes the characteristics of the UAV flights and point clouds generation.

The authors measured snow depth using time-lapse cameras in combination with snow poles, however, it remains unclear where the poles are located on the S-face. A mosaic figure with time-lapse photos could help to understand how this technique worked and visualize the snow cover dynamics through the year, which would be a very good result on its own. It remains unclear how long the time series is, what are the intervals between measurements. Furthermore, there they used data from the E-face in 2012 and this set up is not described at all.

Thank you for these useful suggestions. We added to section 3.2 the missing information on the measurements of temporal changes in snow depth:

"Time lapse cameras with temporal resolution of 4 images per day were used to monitor the height of accumulated snow using permanent measurement poles installed on our study site at Aiguille du Midi (Fig. 1, 4B). The snow depth data covers time periods between 2012-2015 and 2021-2022. Ten poles were installed in two areas of 20 m × 20 m each, near the boreholes on the SE face (Fig. 1) and on the east (E) face. The poles heights are 1-1.35 m and painted with colored bands of 0.1/0.2 m. Snow accumulation time series, with sub-daily resolution, were then produced by visually examining the images with an estimated accuracy of ~0.1 m, based on the ability to read the snow depth from the images. A snow depth time-series of the SE face field site, based on images taken between January 2012 and July 2012, from the same camera position, was used to calibrate the model constraints on snow accumulation and loss rates, and also compare with the maximum snow depth values obtained from the 3D photogrammetric point cloud models. A snow depth time series of the E face, from images taken between February 2012 and January 2015 (with gaps in data between June 2012 and March 2013) was used to validate the model.".

Figure 1 shows the location of the SE borehole and photos of two snow depth poles installed recently and the time-lapse camera. A new Figure S6 was added in the supplementary materials and shows a mosaic of different snow depth stages as they were documented by the time-lapse camera.

The energy balance is modelled using CryoGRID. The model is currently under review in a different journal and there is no information given how this model works. Currently it is a black box where you put data in and receive some results. The authors should provide much more information in the paper or supplementary on the physical basis of this model. Somehow this model uses forcing data and calibration data. The authors used the gridded S2M-SAFRAN dataset for a period 1958 to 2021 as "forcing data" but how do they used their own data (point clouds, time-lapse photos remains) remains unclear. Did they use it to calibrate the gridded data to the rockwall? What kind of surface resolution has the gridded dataset? How this dataset related to the measured data? The authors need to provide much more information how they link data to modelling and they should communicate clearly the uncertainties of their approach.

Thank you for this comment. Indeed, detailed descriptions of the Cryogird and CROCUS models exist in other preprints and publications. Following this comment, we added a more detailed description of the numerical models in section 3.3.1. The specific details on the data used for atmospheric forcing and calibration/validation are detailed in the specific sections: 3.3.2 (Forcing data) and 3.3.3 (Constraining snow accumulation and model calibration). The CryoGrid model description paper is in a relatively advanced stage with minor revisions on the preprint.

(3) The result section comprises several paragraphs and is much too short to represent the interesting results of the manuscript. The authors should focus more on the observed patterns that are clearly visible in the figures but not sufficiently described in the text. Furthermore, the results should be discussed in full detail. The discussion section on snow depth is too broad. The authors should provide more detail. How does their results compare to other studies? What is the key message of these studies and how they support your results? Section 5.2. on the gridded data set reads like an extended conclusion not like a discussion. The results are not compared to other studies or critically analyzed. In section 5.3, the authors claim that they fill a major knowledge gap without explaining what this gap is and what their add-on is on current knowledge. Again, they cite papers without providing the key message in the discussion or previously in the introduction.

This study presents a new model approach and results of a first attempt to quantify water balance on steep permafrost-affected rock slopes. The results include too numerous details to cover in this contribution, and we point out the ones that are most relevant, in our opinion, to the research questions and the interest of the journal's readers. We discuss the results in the discussion section with comparisons to other studies. We addressed all the results and patterns that we see as relevant and also those pointed out by the reviewers. We edited and reduced section 5.2 which discusses the applicability and limitations of the model setup. Section 5.3 was also edited with an elaboration on the knowledge gaps addressed in this study.

The final section on implications on geomorphology should be the chapter of major interest for the readers of Earth Surface Dynamics, however, as the processes link is not established in the introduction (see major comment 1), the links still remain unclear in the discussion.

The introduction section was edited to correspond better with the main results and discussed issues, and conclusions.

For minor comments, see attached pdf.

All minor comments were addressed.

We are grateful for the comments and suggestions provided by the reviewer which we have carefully considered when revising and improving our manuscript. In the following, we provide point-by-point replies to the key issues raised. The reviewer comments appear in black italics letters, and our answers are in normal blue font

Summary

The manuscript 'Estimating surface water availability in high mountain rock slopes using a numerical energy balance model' by Ben-Asher and co-authors presents a modelling framework to estimate water availability at the surface of steep rock walls in a high mountain region. The model combines an advanced snowpack model with a model of the rock thermal regime, and is constrained using in situ field measurements (snow depths and borehole temperatures). It enables the authors to compare the water availability at various aspects and elevations and to show its dependence on climate conditions.

I found this manuscript to be generally well written and interesting to read throughout. This idea of coupling a snowpack model with a representation of the underlying permafrost is appealing, and the results and discussion present an interesting perspective for the understanding of the stability of high mountain slopes in the current climatic context.

Despite the quality of the manuscript, I have a few major comments related to some of the methodological aspects, which makes me question the calibration-scheme and transferability of the model. Furthermore, I found that some crucial elements were missing in the results and discussion, such as the seasonality and slope dependence of some of the parameters, or the conditions leading to the formation of an ice crust. I have also listed a number of more minor comments below. I hope that the authors will find these useful.

Major comments

*1. **Uncertainty and use of snow depth measurements:** More details are needed on how the point clouds were derived (method, number of images, geo-referencing…). What is the uncertainty from the DEM differencing? Has this been assessed in any way (ex. Using stable terrain)? This is particularly important as snow surfaces can be difficult to map. Based on Fig. 4B I find it difficult to believe that the local snow depths were made with an accuracy as low as 5 cm – the lack of scale does not help, but a more detailed description of these measurements would also be welcome. A comparison between the point measurements and the DEM differencing would be interesting and it would also be useful to give more details on which measurements were used for the calibration. Shouldn't some measurements be used for the calibration and others kept for the validation of the model?*

Thank you for this comment. The section about snow depth mapping (3.1) was considerably elaborated, including a new table that summarizes the characteristics of the UAV flights, point clouds, and uncertainties. The reported accuracy of ±5 cm is based on our experience with visually differentiating snow depth changes between the middle of a colored scale (for example the middle of a 10 cm long colored section) or between two scales (for example where color scales meet). This gives an error range of 10 cm, or ±5 as we report in the manuscript. To avoid confusion we changed the reported error to 10 cm. We inserted an image of a measurement pole in Figure 1 and added a time-lapse images series in the supplementary materials (supp. Fig. S6) that shows different stages of snow accumulation.

*2. **Validation:** The modeling scheme is understandably quite hard to validate. It seems that at the moment it relies mostly on near-surface temperature data, which feels a bit far-reaching from the water availability at the surface. A better description of the influence of this variable in the model (and of the CryoGrid modeling scheme in general) and its link with surface water would be welcome, as well as some discussions around this point in the discussion section.*

Thank you for pointing this out. Model validation is a principle problem in mountain environments and quantifying near-surface water contents is not routinely done. The validation relies on both near-surface temperatures and snow accumulation measurements, and not mostly on near-surface temperatures. No other output of the model can be compared with field measurements. Snow depth measurements are directly linked with water availability and near surface temperatures are also closely linked, especially at near freezing conditions. The description of the validation process was edited and we hope it is clearer now. We also added a new figure to the supplementary materials (supp Fig. S4) that shows the results of the validation of snow depth measurements on the east face site. We added the following sentence in section 3.3.3: “Two of the model outputs can be compared with field measurements and were used for calibration – snow depth, which has a direct influence on water availability, and near-surface temperature which can indirectly influence the water mass balance by controlling sublimation, evaporation, melting and refreezing of the snow.”

*3. **Transferability to other aspects:** The comparison of the E and SE face is limited to the calibration/validation scheme, but it would already be interesting to see what the results show for these two locations where there is data available. It looks (Fig. 3) as if the model may not be working as well for the SE face as for the E face (cold bias), is this the case and why? Does this not influence the transferability to other aspects? It would also be important to describe how the model is adapted to other aspects – is the DEM ‘rotated’ and how is the flux calculation updated? Depending, what are the underlying implications for the model in its current state? For example, are the changes in sky-view factor accounted for?*

Thank you for this important comment. We realized that the way we used the data from the SE and E sites for calibration and validation was not clear enough. The modeled near-surface temperatures of the E face site give better results than the site on the SE face when compared with data from the boreholes. The reason for that is that the E borehole was originally installed in a sub-vertical wall that does not accumulate much snow. This reduces much of the complexity of the energy balance and gives better predictions of rock temperatures. This is now addressed in sections 3.3.3:
“The location of the validation site on the E face shares many characteristics with the SE face borehole (i.e. elevation, rock type, climate) and includes the required datasets that were used in the calibration – snow depth poles, a time lapse camera and near surface temperature measurement in a 10 m deep borehole. However, for technical reasons, the borehole on the E face was originally installed in a sub-vertical wall that does not accumulate snow. We thus compared the near-surface temperature measured at the E face with the modeled temperature with a low snowfall multiplication factor value of 0.1 (10%) (Fig. 3C), and measured snow accumulation with the calibrated value of 0.25 (25%) (supp. Fig. S4).”
and in 4.1: “The predictions of the near-surface rock temperatures on the E face were made with snow free conditions and provide good correlation with field measurement (Fig. 3B). The reason for that is the location of the E borehole in a sub-vertical wall that does not accumulate snow and reduces much of the complexity of the surface energy balance calculations and the subsequent uncertainty.”.

In the model scheme, the forcing data is adjusted to the topographic parameters such as elevation, surface gradient, and aspect direction. The sky view factor was adjusted between the SE and E sites. In the comparison of north-south aspects and elevation changes, only the aspect direction was changed to isolate its influence on the model output.

*4. **Seasonality & slope dependence of parameters:** I would expect the snow parameters (depth, multiplication factor) to depend on the season (influence of temperature on snow characteristics) and slope, while here it seems that only one value was used for the whole domain and the whole period? Fig. 4c especially seems to indicate a seasonal effect. I am* *also wondering if relying on snow depth instead of snow water equivalent does not have an influence on the calibration scheme?*

These are very important and interesting points. However, data on snow water equivalent in steep permafrost-affected rock slopes does not exist and cannot be used to calibrate the model. It is however one of the main outputs of the model. Our approach to constraining snowfall accumulation on steep permafrost-affected rock slopes contains many simplifications, like every model, and does not address many possible factors. To our knowledge, however, it is the most detailed attempt to simulate snow accumulation and water balance in such a complex environment and field settings. We hope to see more studies in the future with more accurate models and empirical data and hope that this manuscript will lead the way there.

*5. **Conditions of ice crust formation:** This seems to be a key element for the water availability, and is presumably a main output of the modelling, but the conditions of formation of this ice crust are barely mentioned. It would be interesting to know the processes causing this ice crust formation and if it is systematic under the applied climatic conditions. Similarly, one of the main results of the paper seems to be that the sublimation is a very important process at this elevation, but there is no description of how these fluxes are represented in the model?*

In section 3.3 we describe our model approach to define the formations of an ice crust. We added the following sentence with a reference to the relevant paper: "In the CryoGrid model, snow density is controlled by compaction, metamorphism, refreezing and water retention processes (Vionnet et al., 2012)". We also address some of the limitations of the model to simulate the ice crust and the effect on the snow hydrology in section 5.3: "We demonstrate previously suggested control of snow hydrology on water availability in high elevation steep rock slopes, via the formation of an ice crust layer that can profoundly lower the local rock surface infiltration capacity (Woo and Heron, 1981; Woo et al., 1982; Marsh, 2005; Phillips et al., 2016). Our approach to simulate the formation of the ice crust and its influence on the snow hydrology is likely over simplified and ignores possible lateral fluxes and formation of impermeable layers in upper parts of the snowpack.".
To address sublimation, we added the following sentence in section 3.3.1: "Snow surface mass fluxes are also computed with the consideration of energy balance and include latent heat fluxes from evaporation and sublimation following an approach by Boone and Etchevers (2001).".

*6. **Use of CryoGrid model:** The model used is described in a very vague way. The CryoGrid component especially, which does not seem to be commonly used in the literature, lacks details. At this stage I do not really understand what it is used for, except for the calculation of the heat conduction and therefore the surface energy balance at the rock-snow transition. But doesn't CROCUS already have a similar scheme? I was expecting that CryoGrid would also be used to represent the water-rock interactions, at least in terms of rock permeability that could lead to saturation. This is briefly discussed at the end of the manuscript, but it would be nice to actually include this in the modelling scheme, and test the influence of different rock permeability values. One could then test if water availability or rock permeability is the limiting factor.*

Thank you for this important comment. We edited Section 3.3 and subsections 3.3.1, 3.3.2, and 3.3.3 and elaborated on the description of the model. In the discussion section 5.3, we discuss the results of the effective snow melt fluxes and compare them with values of hydraulic conductivities of fractured rocks, and show that snow melt can actually be a limiting factor.

Line-by-line comments

Abstract

L21: remove comma.

Done

L22: I would suggest using mwe everywhere.

For simplicity and consistency, we prefer to keep the units of [m] for water fluxes. The following sentence was added in section 4.3 to clarify: “All water fluxes are reported in units of m (i.e. water equivalent of volume per area - m^3/m^2).”.

L21-29: This part would benefit from being reorganized and condensed to make the main results clearer.

We absolutely agree. This section was edited.

Introduction

L64: how is the snow water equivalent derived from the depth? Is this an output of the modeling or the result of some density assumptions?

A very good point. Snow water equivalent is calculated by the model. We rephrased the sentence to: “This information is essential to accurately model the snow water equivalent amount at the rock slope surface”.

Study area

L75: Is it really necessary to use an acronym for Aiguille du Midi?

We replaced all AdM acronyms with full name.

L82: please spell out ‘temperature’ throughout the text.

Done. T is now only used in the figures.

L84: where these surveys used in this study? Can more details be provided? A recap table listing all the datasets used, their characteristics and how they are used would be very useful.

More details were added together with references to the relevant methods sub-sections and figures.

L84: ‘surveys’ is plural.

Corrected.

L75: The fact that the study site is located on Aiguille du Midi makes me wonder whether there could be some ‘human’ influence on the survey domain? Thinking for instance about snow blowing/shoveling?

Thank you for this comment. We added the following line to address it: “It is located below a confined section of the touristic structure and it is not frequented by skiers and alpinists. There is thus minimal man-made influence on the natural processes of snow accumulation.”

Methods

L92-93: more details on the UAV surveys are required (UAV type, height, ground sampling resolution, number of images, georeferencing…).

This section was substantially elaborated and a table was added with summary of UAV flights and processing information.

L94: 2022 has been a very low snow year. Do you know how representative is this for previous years?

The survey was done in the winter of 2021/2022 which had relatively high snowfall. In any case, we are interested in the representation of typical high snow cover and not an extreme event.

L95: may be true in terms of height, what about SWE?

We do not have measurements of SWE and we are not familiar with a reliable technique to measure it in the field. We use snow depth as a parameter in the calibration process of our model, which calculates the amount of SWE in the snowpack and snowmelt.

L95: Can you use the stake measurements to show this?

Yes. We added the following sentences: “Data from an on-site time-lapse camera (see section 3.2) and from a meteorological station at Chamonix show that substantial snowfall events occurred on the 25th to 27th of December and 5th to 7th of January. We assume that

the 10 days period without snowfall prior to the survey was sufficient for redistribution and compaction processes to take effect and that processes of mass loss from the snowpack are either by sublimation or snowmelt."
L97: specify rock slope (vs snow slope).
Done
L111: missing parenthesis
Done
L112: can you be more specific than 'several'?
Done.
L113: Can you show the location of the poles in a figure?
Added in figure 1
L117: constraints
Done
L122: Can this be shown in a figure? Along with stake readings and UAV measurements?
We decided to omit the comparison of modeled snow depth with the measurements in La Requin and Aiguille Rouge. Stake readings are displayed in Figure 4C (red crosses) and UAV measurements are shown in Figure 2.
L126-127: this would read better in a table. Also, for this study it seems that only the near surface temperature is used, correct?
Yes. The list of sensors depths is irrelevant and was omitted.
L136-137: This sentence is unclear. In general more details (including in the Supplementary & Supplementary figures) would be welcome for the description of the modeling scheme.
This section was elaborated. Supplementary figure S2 shows an illustration of the model's components.
L147: End of the sentence reads a bit weird.
Done

L148: specify cell size of the reanalysis product.
Done.
L148: 'well fitted for our needs': can you be a bit more specific?
Done
L152: what about 2022? Is this not the studied year?
Data for 2022 is not yet available for the analysis
L157-158: English could be improved.
Done
L165-169: this part reads a bit unclear and would benefit from being rewritten.
Done
L183: 'range' repeated. English could be improved.
Done
L192-193: please show in figure.
Added reference to Fig. 2 and Fig. 4.
L194: be more specific than 'satisfying'. These R2 values seem quite low….
The comparison between sites and the reliability of the data is problematic. We, therefore, decided to omit this part.

Results
L224: I get lost between 'total' and 'net' snowmelt, with the terminology changing between parts of the text and some of the figures.
The terms are now consistent with net snowmelt.
Discussion
L282: you need to be more specific than just stating 'robust'.
Added: "as mean snow depth decreases from 0.8 m to 0 m when slope angle increased from 45° to 75°".
L335: could be useful to show the equation here. Can this not be included in the modeling scheme? I find that this would be very interesting.

Equation added. The current model focuses on surface heat and water balance. Further work on the influence of fracture density and the anisotropy of the hydraulic conductivity is in progress using a model that is more adequate for subsurface thermo-hydrogeologic processes.

L416: availability of weather data?

Weather data is available from the public S2M-SAFRAN repositories.

Figures

Figure 1: Could you increase the size of the images showing the borehole and the TL camera? Are the while circles AWSs used in this study? It would be good to mention this somewhere, and indicate Mont Blanc with a different symbol. Please use letters for the different parts of the figure.

Increased the size of photos, deleted the white circles, and added letters.

Figure 2: Could you show somewhere the DEMs & DEM differencing?

We added a new figure to the supplementary materials – supp. Fig. S5, that shows the projected images taken by the UAV before, and after a snowfall, and the derived snow thickness.

Table 1: What method was used to calculate the sky view factor?

A grassGIS tool was used to calculate a sky view factor raster from a DEM, and a mean value was used.

Why is this value fixed for the whole survey area? Shouldn't it be dependent on the local topography?

The model domain is 1D and only a single value can be used in each simulation. The sky view factor in the study area used to parameterize the model, on the SE face, should not vary significantly since the surrounding topography and slope angles do not vary much.

I am getting a bit lost – was the maximum snow depth calibrated or measured?

Maximum snow depth was measured. The description in section 3.3.3 was confusing and it is now edited.

What does 'For slope angle 45°' mean?

This remark was confusing and deleted. It was intended to address the fact that max snow depth depends on the slope angle. It is not relevant since both parameters were fixed in the simulations.

Is the model not accounting for longwave from surrounding terrain?

How is this calculated?

It is calculated in a relatively simplified way, based on the sky view factor, incoming longwave radiation from the atmosphere, and air temperature. It assumes that the local topography that blocks some of the sky view (1-SVF) emits longwave radiation as a black body.

I am surprised to see so few parameters listed, a model description is really needed to make sense of this.

The model includes tens of parameters. We include in the list the ones that we specifically constrained and most relevant for the model outputs that we present. The model is also described in more details now in the text, in section 3.3.1.

Figure 3: Could you zoom in to one specific year in addition to showing the whole time series? What is this cold bias on the SE face? Would the E & SE faces not be impacted differently by winds (snow redistribution)?

Figure 3 now also includes the measured and modeled temperatures on the E face, the SE face, and zoom to a single year.

The cold bias in figure 3 is a limitation of the calibration scheme, which we limited to only two parameters. Changing thermal properties of the ground would have improved the modeled temperatures but we limited ourselves to using known parameters from the literature (Table 2).

We have not thought about the influence of wind differences between SE and E faces. Strong differences in wind speed could influence the value of the snowfall multiplication factor, but we do not see its influence in the validation process.

Figure 4: As for Fig. 3, zooming in into 1 year would help with the readability. B) scale missing. C) Model works less well for May-July, seems that the seasonality should somehow be accounted for. Where were the measurements taken? Are these at all the stakes or one specific stake? Axis labels missing. L206 caption: 'optimum' needs to be replace with some statistical metrics.

Fig. 4A was removed. Fig. 4B was moved to Fig. 1A and a scale was added. In Fig 4C (now Fig. 4) Measurements taken on the SE face, in the study area, near the borehole. The data is from a single pole. Axis labels were added.

Figure 8: font size needs to be adjusted. Check for other figures as well.

Done

Figure 10: Can you also show the correlations with water availability?

It is possible but we do not see a reason to do that. We show that water availability decreases non-linearly with altitude and rockfall probability has a clear maximum at an altitude of 3300-3600 m a.s.l. They are obviously not correlated and we address this point in the discussion section.

Figure S2: more details needed. How is the runoff obatined?

The use of the term runoff is confusing and not a good choice because infiltration rate in the ground is not computed – only the amount of water that is available for infiltration. We changed the term 'runoff' to 'excess snowmelt'.