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?

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.

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?

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?

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?

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.

Line-by-line comments

Abstract

L21: remove comma.

L22: I would suggest using mwe everywhere.

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

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?

Study area

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

L82: please spell out 'temperature' throughout the text.

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.

L84: 'surveys' is plural

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?

Methods

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

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

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

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

L97: specify rock slope (vs snow slope)

L111: missing parenthesis

L112: can you be more specific than 'several'?

L113: Can you show the location of the poles in a figure?

L117: constraints

L122: Can this be shown in a figure? Along with stake readings and UAV measurements?

L126-127: this would read better in a table. Also, for this study it seems that only the near surface temperature is used, correct?

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.

L147: End of the sentence reads a bit weird.

L148: specify cell size of the reanalysis product

L148: 'well fitted for our needs': can you be a bit more specific?

L152: what about 2022? Is this not the studied year?

L157-158: English could be improved.

L165-169: this part reads a bit unclear and would benefit from being rewritten.

L183: 'range' repeated. English could be improved.

L192-193: please show in figure.

L194: be more specific than 'satisfying'. These R2 values seem quite low…

Results

L224: I get lost between 'total' and 'net' snowmelt, with the terminology changing between parts of the text and some of the figures.

Discussion

L282: you need to be more specific than just stating 'robust'.

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.

L416: availability of weather data?

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.

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

Table 1: What method was used to calculate the sky view factor? Why is this value fixed for the whole survey area? Shouldn't it be dependent on the local topography? I am getting a bit lost – was the maximum snow depth calibrated or measured? What does 'For slope angle 45°' mean? Is the model not accounting for longwave from surrounding terrain? How is this calculated? I am surprised to see so few parameters listed, a model description is really needed to make sense of this.

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 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.

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

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

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