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.