Author’s response

We would like to thank both reviewers for the thoughtful suggestions to our manuscript. In the following, we give the author’s response with line numbers referring to the revised manuscript. We included the replies to the reviewers in this document and the revised manuscript with marked changes, where we addressed all major and specific comments of the reviewers.

The manuscript has been revised with the following major changes:

- RC1: The first concern was the validation of the model and the link to the two study sites. We account for this by 1) adding an objective, statistical measure of the model performance and 2) vastly improving the validation at the rock glacier site.
- RC1 and RC2: Both reviewers suggested extensive restructuring and editing of the “Methods” chapter. We followed this suggestion and adopted a clear structure where we present first a general description of the used model, followed by a description of the model forcing data, and finally information about the model setup, parameters and model scenarios.
- RC2: The reviewer suggested improving the “Introduction” chapter by providing better links between existing literature, driving mechanisms and how our study addresses those. We restructured the chapter for clarity and included additional references.
- RC2: The reviewer suggested performing a sensitivity test to porosity. It is included in the Supplement.

In addition, we made changes in the manuscript to further comments of the reviewers, which can be found in the replies to the reviewers.

We hope that the revised manuscript is suitable for publication in Earth Surface Dynamics.

Kind regards,

Cas Renette (on behalf of all authors)

Response to Anonymous Referee #1

We would like to thank the reviewer for the detailed and useful comments, which have helped to improve the quality and readability of our manuscript. In the following, we provide a reply to the points discussed by the reviewer as well as the changes in the manuscript.

The comments of the reviewer are written in **bold**, the extracts of the manuscript in *italics* with changes highlighted in *blue*.

Major comments:

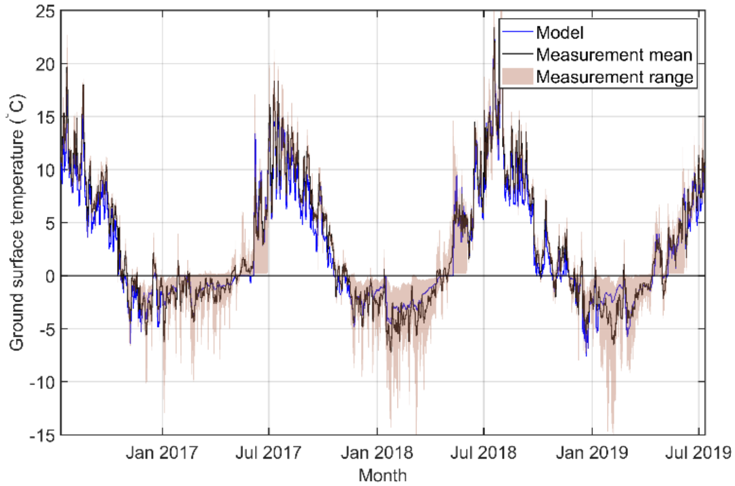
1 The idea of the model experiment – testing the impact of drainage on the ground thermal regime – is very interesting and the approach seems sound and straightforward. However, the model validation and the links to the two sites in Norway are weak. For one of the sites there are observed temperatures available to compare with the model results but these data are not used to validate the model results in a robust way. Instead, validation is only carried out visually and no objective statistics are used to evaluate the model results. For the other site, there are no data on ground temperatures but only observed ground surface temperatures. The validation and comparison to observations from the Norwegian sites thereby adds next to nothing to the modeling study. My suggestion would be to either remove these sites from the manuscript, to add some statistical analysis based on the available ground temperature data, and/or to find a rock glacier site with available ground temperature data which could be used for a real validation of model results.

The two Norwegian field sites are of high importance to the study. They are used for validation with the available data sets and the comparison of observed and modelled data shows that the model can capture the main characteristics of the seasonal evolution of measured temperatures. We agree with the reviewer that temperature data from boreholes would be beneficial. However, as these do not exist for Norwegian rock glaciers, Ivarsfjorden is a well suited field site for this study, as it is well documented by a previous study (Lilleøren et al., 2022) and near-surface ground temperatures are recorded by several temperature loggers. Furthermore, the presence of the rock glacier landform itself, as well as geophysics investigations on ground ice presence, allows to constrain the permafrost evolution at least in a qualitative fashion (Lilleøren et al., 2022), and his can also be compared to the simulation results. We therefore decided not to remove the site from the study, but improve validation. To do so, we have followed the suggestion to include a statistical analysis of the model performance at both sites. Secondly, the model validation for the rock glacier site (Ivarsfjorden) has been redone in a much improved and more robust manner. Please find below the changes in our manuscript.

We added a more detailed description of the temperature loggers at Ivarsfjorden to explain the setup for model validation:

Line 287-291 : At Ivarsfjorden, we considered 11 temperature loggers within the rock glacier outline (Fig. 1d in Lilleøren et al., 2022), of which all except for one are placed on the relict surface of the rock glacier (Fig. 2a in Lilleøren et al., 2022). On the relict surface, deposition of finer sediment in between blocks is more likely than on the active surface, due to the lack of movement. Here, the blocks with sediment stratigraphy is considered appropriate and used as starting point for the calibration.

Temperatures resulting from the best-fitting model setup are now presented in a similar way as Juvvasshøe, showing the model can reproduce daily ground surface temperatures in a satisfactory manner.



Line 349-352 : Figure 4: Daily modelled and measured ground surface temperatures in Ivarsfjorden from July 2016 to July 2019. The shaded area indicates the minimum to maximum range of measured daily values from 11 loggers (based on Lilleøren et al., 2022), while the black line represents the mean value of all loggers.

At both of the sites, the bias and RMSE are calculated for daily averages.

Line 291-293 : At both sites, the root-mean-square-error (RMSE) and bias are calculated in order to provide an objective measure of the model fit. At Juvvasshøe this was accomplished for daily values at 0.4 m and 2 m depth, while at Ivarsfjorden the mean daily ground surface temperature of the loggers within the rock glacier outline is used.

Line 328-330 : This configuration used drained conditions, although differences with undrained conditions are minimal for this stratigraphy. For daily temperatures at 0.4 m depth, the RMSE and bias are 2.1 °C and -0.6 °C, respectively, while they are 1.2 °C and -0.7 °C at 2 m depth.

Line 340-341 : The best-fitting model configuration was found to be the blocks with sediment stratigraphy and a snowfall factor of 1.0, resulting in an RMSE of 1.3 °C and a bias of -0.4 °C.

2 The methods section would benefit from restructuring and extensive editing, because it is rather confusing as currently written.

We agree that the method chapter was not clearly enough structured and required restructuring and clarification. In the revised manuscript, we have completely restructured the Methods section for clarity.

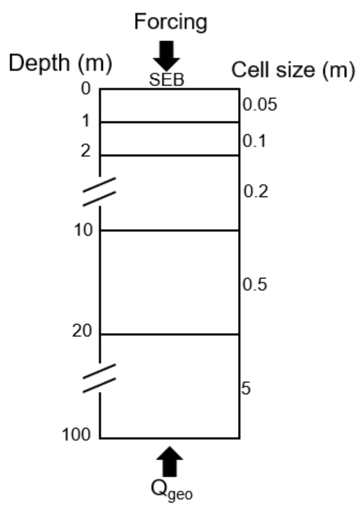
Section “3.1 The CryoGrid community model” contains a general description of the used model, not covering any site specifics or parameter values. We removed extensive descriptions of CryoGrid capabilities which are not used in our model setup. This section is followed by “3.2 Downscaling of model forcing”, a description of the model forcing data, which gives the reader information about the time period and time resolution of the model forcing. Section “3.3 Model setup” now covers the model setup and parameters, which were previously scattered throughout the chapter. As suggested by the reviewer, we created separate subchapters for the Validation runs (3.3.1), Equilibrium runs (3.3.2) and Transient runs (3.3.3). Specific improvements / clarifications in the Methods will be highlighted at the corresponding review comment.

2.1 Model description. It would be helpful to start the model description with a presentation of the mesh used: is this a vertical column (1D) mesh? What is the thickness and cell size of the domain?

Some of this information is available in other parts of the methods section, but starting off by clarifying such basic facts would help the reader to envision the model setup.

In the process of improving and clarifying the methods section, we followed the reviewer's recommendation and included a schematic in the beginning of the methods section, that shows the model domain with its depth, cell sizes and boundary conditions. In this manner, the reader is directly informed about how the model domain looks.

Line 154-155: We use a one-dimensional model column with a domain depth of 100 m (as in e.g. Westermann et al., 2016; Schmidt et al., 2021) and grid cell sizes increasing with depth (Fig. 2).



Line 165-168: Figure 2: Schematic of the model grid, indicating cell sizes at different depths and upper and lower boundary conditions. As upper boundary condition, the surface energy balance (SEB) forced by near-surface meteorological data is used. The lower boundary condition is provided by a constant geothermal heat flux at 100 m depth.

2.2 Snow model. A list of processes included in the model is provided, but there is no explanation of how these are represented in the model. If these are explained in previous publications, please clearly refer to those for the specific processes listed. For example, "the physical effect of wind drift on the snowpack" is included, according to the text, but later it is stated that the redistribution of snow by wind is phenomenologically represented using the snowfall factor. So, what effects of wind on the snowpack are actually included? Does the snowfall factor allow for variable distribution of snow on the domain, or is there also a redistribution of snow over time?

We agree with the reviewer that better explanations and referencing is needed regarding the implemented snow processes in the model. We therefore refer to Vionnet et al. (2012) for defining equations of the Crocus snow model, and to Zweigel et al. (2021) for implementation in CryoGrid). Furthermore, we improved the explanation of the snow model in the manuscript, especially clarifying the snow redistribution.

Line 189-197:The snow model used in this study was introduced by Zweigel et al. (2021) and is based on the Crocus snow scheme (Vionnet et al. 2012) which accounts for snow microphysics and is designed to reproduce a realistic snow pack structure (see Vionnet et al. 2012 for defining equations; Zweigel et al. 2021 for implementation in CryoGrid). Snowfall is added with density and microphysical properties derived from model forcing data, in particular air temperature and wind speed. The snow density evolves due to compaction by the overburden pressure of overlying snow layers, as well as wind compaction and refreezing of melt- and rainwater (Vionnet et al., 2012). The amount of snowfall from the forcing data can be adjusted by a so-called snowfall factor, sf, with which the snowfall rate from the model forcing is multiplied. With this, the effects of wind-induced snow redistribution on ground temperatures can be *represented at least phenomenologically (Martin et al., 2019), using sf < 1 for areas with net snow ablation and sf > 1 for areas with net deposition.*

2.3 The description of the different model runs (validation, equilibrium, and transient) is rather confusing and split up in different sub-sections of the methods description. I would suggest to clarify the purpose of the model runs and explain them in a separate sub-section. The validation runs are runs validated based on visual fit, which is not a very robust metric for model validation. It seems like the purpose of validation runs was to determine (calibrate?) the proper sediment stratigraphy for each site. But how were other parameter values determined? Table 1 presents some physical properties, but not e.g. Kh. Snowfall factor is for some reason tested in the equilibrium runs (why?). In general, the purpose of testing the snowfall factor sensitivity is rather unclear to me. The outcome of this test is not mentioned in the abstract, but the current text does not suggest that this is pure model calibration. L230 suggests that snowfall factors were also tested in validation runs, but this is not mentioned previously.

As mentioned in the response to major comment 2, we followed the suggestion to split the different model runs in sub-sections and clarified their purpose. The manuscript includes now the sections "3.3.1 Validation runs", "3.3.2 Equilibrium runs" and "3.3.3 Transient runs" and the purpose of the model runs has been clarified. Please refer to the corresponding sections of the revised manuscript for all changes. Specific comments are addressed below.

<u>Model validation</u>

We clarified the goal of the model validation:

*Line 271-272**:** Validation runs are set up to show that the model can reproduce key characteristics of the thermal regime at the two sites in a satisfactory manner (based on available observations).*

We agree with the reviewer that a visual fit is not sufficient, therefore we included a statistical analysis of the model performance and significantly improved validation for the rock glacier site (for changes in the manuscript see response to major comment 1).

We now describe that snowfall factors are tested in the model validation:

Line 282-283: Manual adjustment of the ground stratigraphy (porosity and thus mineral content) and snowfall factor are performed until a good fit with daily measurements is achieved.

<u>Equilibrium runs</u>

It is important to test different snowfall factors, as the thermal regime is highly sensitive towards snow conditions and the snow cover shows a high spatial variability in Norwegian mountains. Therefore, we investigated the sensitivity of ground temperatures by running the equilibrium runs with a range of snowfall factors, to estimate the threshold snow amount for permafrost existence in different model scenarios. We added further explanations to the revised manuscript by including previous studies and highlighting the purpose of the model runs.

Line 295-301: The goal of equilibrium runs is to investigate the sensitivity of the ground thermal regime towards ground properties and drainage conditions, using both the undrained and drained setup for the three idealized stratigraphies (Table 1), which result in six scenarios. As the heavily wind-affected snow cover is a key source of spatial variability in ground temperatures in the Norwegian mountains (Gisnås et al., 2014; Signåa et al., 2016), the model is run for a range of snowfall factors between 0.0 and 1.5 (Table 2) for each scenario. This analysis allows us to identify the magnitude of the thermal anomaly that the subsurface drainage induces at various amounts of snow, as estimate the threshold snow amount for permafrost existence in the six scenarios.

The outcome of this sensitivity test is now also included in the Abstract and Conclusions

Line 24-25: The thermal anomaly increases with larger amounts of snowfall, showing that well drained blocky deposits are less sensitive to snow insulation than other soils.

Line 589-591: The largest anomalies occur in simulations with a thick winter snow cover as ground temperatures in well drained blocky deposits are less sensitive to insulation by snow than other soils.

Model setup

We have provided further details on the choice of the hydraulic conductivity and the parameter d^{lat} and also present further sensitivity studies in a Supplement:

Line 245-255: To investigate the effect of subsurface drainage ground temperatures and ground ice conditions, we distinguish undrained and drained scenarios by using two different values of d^{lat} (Eq. 1) for in the idealized stratigraphies. A d^{lat} value of 10^4 m is used for undrained cases, which emulates conditions at a flat surface, resulting in a to a good approximation one-dimensional water balance, where only surface water is removed. For the drained cases, a d^{lat} value of 1 m is used, which results in well-drained conditions which are typical in sloping terrain. For the saturated hydraulic conductivity K_H, a fixed value of 10^{-5} m s^{-1} is used for all stratigraphies, although the true hydraulic conductivities almost certainly differ between stratigraphies. However, the key parameter controlling lateral water fluxes in Eq. 1 is in reality the "drainage timescale" K_H/d^{lat} [s^{-1}], which is varied by four orders of magnitude between K_H/d^{lat} = 10^{-5} s^{-1} (d^{lat} = 1 m, well-drained conditions) and K_H/d^{lat} = 10^{-9} s^{-1} (d^{lat} = 10^{-4} m undrained conditions). As the study setup is designed to analyse these two "confining cases", it is sufficient to only vary d^{lat} and leave K_H constant for simplicity. Further sensitivity tests for d^{lat} and K_H are provided in the Supplement.

2.4 The forcing data used for spinup and simulation runs are also not presented in a clear manner. The spinup and initialization procedure and data vary between the sites and the runs and the motivation for the choice of spinup periods is not always clear. Perhaps a table presenting the basic details of each run would be more helpful. Also not clear in the current text: Is forcing data downloaded and downscaled for the two sites (e.i., two separate series of data)? What is the time resolution (daily? monthly?) of the data used for simulations and for validation of simulation results?

We restructured the section "3.2 Downscaling of forcing data" and made distinct changes to the explanations, so that the procedure is now presented in a clearer manner. Specific comments are addressed below.

We present a table that gives an overview about the simulation types and includes information about the model forcing.

Line 265-268: Table 2: Overview of basic model settings for the different simulations types. A spin-up of subsurface temperatures is achieved by repeated simulations for the spin-up period (until a stable temperature profile is reached), before the actual model run for the simulation period is conducted. "Idealized" stratigraphy and drainage refers to three subsurface stratigraphies (Table 1) combined with two types of drainage conditions. See Sect. 3.3.1 and Sect. 3.3.3 for details.

Simulation type	Site	Spin-up period	Simulation period	Stratigraphy and drainage	Snowfall factor
Validation	Juvvasshøe	1951-2010	2010-2019	Best-fit	0.25
	Ivarsfjorden	1951-2016	2016-2019	Best-fit	1
Equilibrium	Juvvasshøe	2000-2010	2000-2010	Idealized	0.0, 0.25, 0.5, 0.75, 1.0, 1.5
	Ivarsfjorden	1962-1971	1962-1971	Idealized	0.0, 0.25, 0.5, 0.75, 1.0, 1.5
Transient	Juvvasshøe	1962-1971	1951-2019	Idealized	0.25
	Ivarsfjorden	1962-1971	1951-2019	Idealized	1

A separate ERA5 dataset is downloaded from Copernicus for both of the sites, using the nearest grid cells to the corresponding location. For both sites, the corresponding dataset is then downscaled with TopoScale. The time resolution of the downloaded ERA5 forcing data is three hours. This has been clarified in the text.

Line 204-207: ERA5 output is provided as interpolated point values on a regular latitude-longitude grid at a resolution of 0.25° at an hourly frequency, both at the surface level, corresponding to Earth's surface as represented in the reanalysis, and at 37 pressure levels in the atmosphere from 1000 to 1 hPa. We considered data for the reanalysis period from 1951 to 2019 at three-hourly resolution.

Minor comments:

1 L155-164: Seepage face is at atmospheric pressure, but does that mean that the seepage face is located at Zwt? Could that sentence just be removed and eq. 1 explains all? Where are Kh values from? Eq. 1 mirrors Darcy's law but values seem made up, and dz is based on the grid z. Explain the rationale behind this model and why it was chosen. (alternatively, if Kh and dlat are unknown, these two parameters could have been replaced by one single parameter.)

We agree that the rationale and the description of the seepage face was not clear. The entire paragraph was reformulated for clarity

Line 173-185: We use a one-dimensional model setup, but simulate lateral drainage of water by introducing a seepage face, i.e. a lateral boundary conditions for water fluxes representing flow between saturated grid cells of the model domain and a stream channel (or the atmosphere) to which the water can freely flow out from the subsurface (e.g. Scudeler et al., 2017). Using the elevation of the water table, z_{wt} (computed as the elevation of the uppermost saturated grid cell), lateral water fluxes F_i^{lat} are derived for all saturated unfrozen grid cells i below the water table (i.e. at elevations $z_\mathrm{i} < z_\mathrm{wt}$) as

$$F_i^{lat} = -\mathrm{K}_H \frac{z_\mathrm{wt} - z_\mathrm{i}}{d^{lat}},$$

where K_H is the saturated hydraulic conductivity, d^{lat} is the lateral distance to the seepage face and the flux is determined by the difference between the hydrostatic potential (proportional to z_{wt}) of the water column and the gravitational potential of free water at the elevation of each cell (proportional to z_i). Note that Eq. (1) is an approximation for small changes of the water table and small outflow fluxes for which the potential in the saturated zone can be approximated by the hydrostatic potential. The parameter d^{lat} is used to control the strength of the drainage, with small distances resulting in a well-drained column, while high values lead to suppressed drainage.

The concerns of the reviewer regarding the K_H values are addressed in major comment 2.3

2 L282: But table 1 does not include any tested case with a porosity of 0.2!! Either the methods section does not completely describe what was done in the validation runs or there is a typo here?

As part of the improved methods chapter, we stated that the ground stratigraphy (porosity) is manually adjusted to find a good fit between measurements and simulated ground temperatures. The best fit was then used for model validation. We made the following changes to the text.

Line 282-283: Manual adjustment of the ground stratigraphy (porosity and thus mineral content) and snowfall factor are performed until a good fit with daily measurements is achieved.

3 L296-312: The model results suggest that the differences in model setups have very little impact on MAGST and that overall there seems to be a slight cold bias in the model. What then does this validation add to our understanding, if the purpose of the study is to investigate the impact of drainage on ground temperatures and ground ice?

The comparison to near-surface ground temperatures is clear evidence that the model can capture key processes, in particular the insulating effect of the snow cover, in a satisfactory way. We argue that is a prerequisite for being able to perform a meaningful sensitivity analysis for the effect of subsurface stratigraphies and drainage conditions. For the Ivarsfjorden site, the simulated ground surface temperatures indeed do not show a large spread between the different scenarios, but the good agreement with measurements shows that we capture the key processes relevant for the near-surface temperatures (i.e. snow cover, surface energy balance, etc.) Secondly, we show that (with the correct near-surface temperatures) it is possible to explain permafrost presence until today in the *blocks only, drained* scenario which fits e.g. to observations of cold air exiting the rock glacier (Lilleøren et al., 2022). Without the comparison to near-surface temperatures, one could simply select a low value for the snowfall factor which would likely also explain permafrost presence, but such simulations would not provide agreement for near-surface ground temperatures. Sect. 5.1 now contains an in-depth discussion on this issue:

At Ivarsfjorden, simulations with full snowfall yielded a similar performance for the ground surface temperature, approximately reproducing the mean of measurements at 11 sites. A statistical evaluation at both sites indicated a cold bias of the model of approximately -0.5 °C which we considered acceptable, considering the spatial variability of the ground thermal regime at both sites (see Gisnås et al. 2014 for Juvasshøe). At Ivarsfjorden, the transient simulations are in broad agreement with observations at the rock glacier which indicate that permafrost has been present in the recent past (Lilleøren et al., 2022). Permafrost conditions are simulated for all stratigraphies during model spin using the cold period 1962-1971 for which temperatures are closest to Little Ice Age conditions when the rock glacier was likely active.

Line 447-465: At Ivarsfjorden, simulations with full snowfall yielded a similar performance for the ground surface temperature, approximately reproducing the mean of measurements at 11 sites. A statistical evaluation at both sites indicated a cold bias of the model of approximately -0.5 °C which we considered acceptable, considering the spatial variability of the ground thermal regime at both sites (see Gisnås et al. 2014 for Juvasshøe). At Ivarsfjorden, the transient simulations are in broad agreement with observations at the rock glacier which indicate that permafrost has been present in the recent past (Lilleøren et al., 2022). Permafrost conditions are simulated for all stratigraphies during model spin using the cold period 1962-1971 for which temperatures are closest to Little Ice Age conditions when the rock glacier was likely active.

Within the model setup, in particular the exact ground stratigraphy and other poorly constrained parameters, such as the albedo, give rise to uncertainties. While the real porosity of the ground is unknown, sensitivity tests show a maximum of 0.4 °C differences in simulated ground temperatures between the highest and lowest porosity values tested (Supplement). Only at Juvvasshøe, the stratigraphy has been described from the borehole (Isaksen et al., 2003), while no thorough evaluation of the subsurface stratigraphy is available for Ivarsfjorden. Lilleøren et al. (2022) described the site as a complex creeping system with inhomogeneous subsurface properties. Most of the rock glacier surface is described as 'relict' (Lilleøren et al., 2022) with sand and gravel in between blocks. For these 'relict' areas, the simulations for the blocks with sediment stratigraphy, in which near-surface permafrost fully or partially degrades, could indeed represent the thermal state adequately. This is supported by the validation run with the blocks with sediment stratigraphy which yielded a good fit with ground surface temperature measurements at sites largely located on this 'relict' surface (Lilleøren et al., 2022). Two areas are described as 'fresh' which could indicate lateral movements due to the presence of ground ice. These contain larger blocks and could thus be better described by the blocks only stratigraphy for which permafrost and ground ice still persist at the end of the simulations.

Please also refer to Major comments 1 and 2.

4 Section 4.1: This section would greatly benefit from some objective measure of model fit instead of just some conclusions from visual inspection of plotted temperature curves.

We agree with the reviewer and included a statistical analysis based on the available temperature data at both of the sites (see major comment 1).

5 Section 4.2: It is not surprising that snow insulates the ground from winter cooling, generally leading to higher ground temperatures. More relevant than a comparison of snowfall factors, would be to check if observed snow depths and ground temperatures are recreated with the forcing data used here. When reading this section, I get curious about how much snow is generally observed at these two sites and does the forcing data capture the range of observed snow, or is a snowfall factor scaling needed to "correct" the forcing data

It is true that the insulating effect of snow on ground temperatures is well known. However, as the snow conditions show a high spatial variability in Norwegian mountains and measurements have shown that permafrost presence is often associated with a low snow cover, it is important to investigate the sensitivity of ground temperatures towards snowfall factors. Consequently, we consider the testing of snowfall factors to be relevant for this study. See as well the response to major comment 2.3.

The observed snow depth cannot be recreated directly with the forcing data, as wind redistribution plays an important role, especially at Juvvasshøe. Therefore, a snowfall factor was applied and fitted for model validation. This is described in the results of the original manuscript:

Line 321-324: The snowfall factor for this model setup is 0.25, i.e. incoming snowfall is reduced 75% in order to capture the effect of snow ablation due to wind drift. This resulted in mean annual maximum snow depths of 34 cm, in broad agreement with observations from the site (Iskasen et al., 2003) and earlier modeling studies at the site (Westermann et al., 2013).

We understand that a constant snowfall factor cannot represent the temporal evolution of the snow cover. This is discussed in section "5.1 Limitations of the model setup".

Line 492-498: In this study, we have performed a sensitivity study with respect to the amount of snow (by modifying the snowfall factor, Sect. 4.2), but simply scaling snowfall cannot represent the true time evolution of the snow cover due to wind redistribution (e.g. Liston & Sturm, 1998; Martin et al., 2019), possibly resulting in differences between observed and simulated temperatures. Nevertheless, it seems unlikely that the exact time dynamics of snow ablation and/or deposition events strongly affects the dependence of the thermal anomaly in the blocks only, drained scenario on overall winter snow depths. We therefore conclude that the significant negative thermal anomaly for the blocks only, drained scenario is likely robust in the light of the model uncertainty.

Response to Anonymous Referee #2

We would like to thank the reviewer for the detailed and useful comments, which have helped to improve the quality and readability of our manuscript. In the following, we provide a reply to the points discussed by the reviewer as well as the changes in the manuscript.

The comments of the reviewer are written in **bold**, the extracts of the manuscript in *italics* with changes highlighted in *blue*.

Major comments:

1. Describe in more detail the rationale for each scenario.

We follow the suggestion of the reviewer and describe the rationale of the model scenarios in more detail. This includes (1) a restructuring of the methods section with a more thorough explanation of the simulation types (3.3.1 Validation runs, 3.3.2 Equilibrium runs, 3.3.3 Transient runs). (2) Furthermore, we explained our choice of ground stratigraphies after each description of *blocks only, blocks with sediment* and *sediment only*:

Line 220-237: The blocks only stratigraphy consists of a coarse block layer with 50% porosity and air-filled voids which is assigned low field capacity of 1% (Table 1), i.e. the surfaces of the coarse blocks retain only little water. This idealized stratigraphy is designed to represent an active rock glacier where finer sediments resulting from weathering and erosion processes are transported towards the tongue of the rock glacier. Furthermore, Dahl (1966) observed that blockfields on slopes more often do not contain a fine sediment fraction between the blocks in northern Norway, so that the blocks only stratigraphy can also represent active blockfields. The second stratigraphy, blocks with sediment, is designed to represent blocky terrain where the voids are filled by finer sediments. This is often observed in blockfields on more flat surfaces, which are more likely to retain finer sediment within their pores (as in Isaksen et al., 2003 and Dahl 1966). We again consider coarse blocks with 50% porosity (as for the blocks only stratigraphy), but as the voids are filled with fine sediments (which again are assumed to have 50% porosity), the overall porosity is only 25%. Furthermore, a significantly higher field capacity than for the blocks only stratigraphy is assigned as more water can be held in the finer pores of the sediment fraction. Finally, the sediment only stratigraphy serves as a control scenario for a soil without blocks. It contains sediment with 50% porosity and a high field capacity due to the water holding capacity of the fine-grained sediment material. For all stratigraphies, bedrock (3% porosity and saturated conditions, e.g. Hipp et al. 2012; Fabrot et al. 2011) is assumed below 5 m depth, which is in line with observations from Isaksen et al. (2003) at Juvvasshøe. Finally, none of the stratigraphies contain soil organic matter. We emphasize that the stratigraphies are in qualitative agreement with field observations of air and sediment-filled block layers in Norway, but the assumed porosities of 50% for both the block layer and the sediments represent idealized scenarios.

(3) We explain the reason why the snowfall factor was varied in the different model scenarios:

Line 285-287: this site is extremely exposed to wind and most snow is blown away (Isaksen et al. 2003; Westermann et al., 2013), the snowfall factor is stepwise decreased to values below one to inprove the model performance.

Line 293-301: As the heavily wind-affected snow cover is a key source of spatial variability in ground temperatures in the Norwegian mountains (Gisnås et al., 2014; Gisnås et al., 2016), the model is run for a range of snowfall factors between 0.0 and 1.5 (Table 2) for each scenario. This analysis allows us to identify the magnitude of the thermal anomaly that the subsurface drainage induces at various amounts of snow, as well as estimate the threshold snow amount for permafrost existence in the six scenarios.

2 The effects of drainage, snow, soil moisture, etc. have been studied for other permafrost sites, but may not be particularly for mountainous regions, the physics does not change from low- and moderate-relief regions to mountainous regions. How does this study connect with existing

literature that studied the effect of snow, soil moisture, etc. on permafrost thermal regime? A better referencing is needed

We restructured the introduction (see major comment 3) and added more references to improve the connection of this study to previous literature. An example with extended references is given below.

Line 40-45: Snow is an important factor in governing ground temperatures and permafrost distribution within an area (e.g. Zhang et al., 2001; Zhang 2005; Goodrich, 1982), especially in mountain areas where permafrost is often associated with a shallow snow cover (e.g. Gisnås et al., 2014; Luetschg et al., 2004). The influence of soil moisture is complicated as it has an impact on the surface energy balance (e.g Liljedahl et al., 2011), the thermal characteristics of the soil (e.g. Göckede et al., 2017), and freezing/thawing dynamics (e.g. Hinkel et al., 2001; Hinkel and Outcalt, 1994), which can lead to both lower and higher ground temperatures.

Furthermore, we included additional references in the discussion:

Line 515-520: This timing of the ground ice formation is strongly different from all other scenarios, for which ground ice mostly forms in fall/early winter due to refreezing of the water contained in the active layer (e.g. Hinkel et al., 2001). This refreezing of the active layer can take several months and is further delayed if a significant snow cover forms during this period, which leads to overall higher winter temperatures in the permafrost due to the insulation (Zhang, 2005).

3 I would strongly suggest rewriting/reorganizing the Introduction (also Methods) section. The authors have done a good job in providing detailed background; however, it needs to be organized so the reader can follow it. Especially, I found a disconnect between the driving mechanisms and how this work is going to address those. The last two paragraphs in the Introduction section provide a slight background but that needs to be expanded.

We followed the suggestion of the reviewer and restructured the introduction. We start with a general section on permafrost and its controlling factors, including the subsurface stratigraphy. Second, we introduce the rock glacier landform and blocky terrain, followed by a presentation of the known controlling mechanisms for the negative thermal anomaly. This is followed by a paragraph on the representation of mountain permafrost in process models and mapping approaches, which typically lack a representation of key processes in blocky terrain and thus do not reproduce the thermal anomaly. We finish with explaining the goal of the study:

Line 105-108: The goal of the study is to evaluate to what extent the thermal anomaly in blocky terrain can be simulated by such a comparatively simple scheme which could in principle be integrated in larger-scale permafrost modelling and mapping efforts. In particular, we investigate the interplay with the seasonal snow cover and discuss the impact on the permafrost distribution in mountain environments.

The methods section has been completely restructured, resulting in new sub-sections to increase clarity. Section “3.1 The CryoGrid community model” contains a general description of the used model, not covering any site specifics or parameter values. We removed extensive descriptions of CryoGrid capabilities which are not used in our model setup. This section is followed by “3.2 Downscaling of model forcing”, a description of the model forcing data, which gives the reader information about the time period and time resolution of the model forcing. Section “3.3 Model setup” now covers the model setup and parameters, which were previously scattered throughout the chapter. As suggested by the reviewer, we created separate subchapters for the Validation runs (3.3.1), Equilibrium runs (3.3.2) and Transient runs (3.3.3).

4 There are lots of short (4-5 lines) paragraphs throughout the manuscript, and probably not needed and can easily be merged.

We agree and have merged a number of short paragraphs.

5 The example of CryoGrid processes provided (lines 134-135) is highly abstract. I am not expecting to provide all the details, but at least some details for a quick reference

Due to the new structure of the methods, we removed the rather abstract examples in the old manuscript, which are not relevant for the presented study. This avoids confusion for readers, who are not familiar with CryoGrid.

Line 152-154: CryoGrid is a simulation toolbox for ground thermal simulations that can be applied to a wide range of modelling tasks in the terrestrial cryosphere thanks to its modular structure (see Westermann et al., 2022 for details). It is mainly applied in permafrost environments, using the finite difference method to transiently simulate ground temperatures.

Instead, we fully concentrate on the model configuration and setup used in this study throughout the entire Methods section.

6 Paragraphs in the abstract? does the journal allow it, usually not seen/recommended?

We have removed paragraphs in the abstract.

7 There are many places where authors need to be specific. for example, L135: "Likewise, different process representations for the seasonal snow cover can be chosen." this needs to be expanded to mention specific snow processes rather than "different processes"

As mentioned in the major comment 3 and 5, we restructured the methods and carefully revised the explanations, so that unspecific wording is avoided. Furthermore, we removed irrelevant examples (see answer to major comment 5). In contrast, we are more specific in the descriptions of the model setup applied in the presented study (see section “3.3 Model setup”).

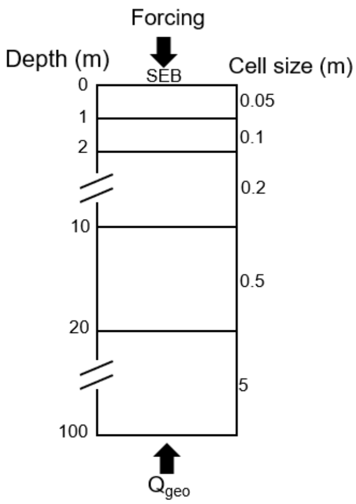
8 Sensitivity to computational domain depth and bottom boundary condition is needed. Provide details that why the domain depth of 5 m and the prescribed geothermal flux were chosen. Describe if the results are sensitive, they will be, to the domain depth and bottom boundary conditions. I understand this can get complicated but at least mention it in the text.

At 0-5 m depth, sediment with different characteristics (see model scenarios) are applied in the model. Below 5 m depth, bedrock is assumed. The model domain depth is at 100 m depth where a heat flux boundary condition (geothermal heat flux of 0.05 Wm^{-2}) is applied. A model domain depth of 100 m is a typical value in permafrost simulations focussing on decadal to centennial timescale which has already been used in previous studies in Norway (Westermann et al., 2016; Westermann et al., 2022; Schmidt et al., 2021). We clarified the differences between depth of the sediment and model domain depth in the manuscript.

Line 154-156: We use a one-dimensional model column with a domain depth of 100 m (as in e.g. Westermann et al., 2016; Schmidt et al., 2021) and grid cell sizes increasing with depth (Fig. 2). The lower boundary condition is provided by a constant geothermal heat flux.

Line 232-234: For all stratigraphies, bedrock (3% porosity and saturated conditions, e.g. Hipp et al. 2012; Fabrot et al. 2011) is assumed below 5 m depth, which is in line with observations from Isaksen et al. (2003) at Juvvasshøe.

Furthermore, we included a new figure to illustrate the model column:



Line 165-168: Figure 2: Schematic of the model grid, indicating cell sizes at different depths and upper and lower boundary conditions. As upper boundary condition, the surface energy balance (SEB) forced by near-surface meteorological data is used. The lower boundary condition is provided by a constant geothermal heat flux at 100 m depth.

9 A schematic of the model domain with boundary conditions, soil discretization, soil layers, etc. can help better follow the results.

We followed this suggestion and included a figure illustrating the model domain (see response to major comment 8). It gives an overview of grid cell sizes, domain depth and upper and lower boundary conditions.

10 Figure 7 shows results for transient runs (1951-2019). What is the air temperature gradient (or increase in the mean annual air temperature) over this period? and did the authors try to run detrended data to isolate the effect of temperature increase? Otherwise, this effect is not due to soil stratigraphy and drainage only. And since the porosity in the "Blocks with sediment" case is 25% (half of the two other cases), more degradation is not unexpected. Also, what caused the patchy low ice content in some of the subplots in Figure 7 (for example, top/bottom right)?

The air temperature change between the 1951-1960 and 2010-2019 means were 0.7 °C and 1.2 °C at Juvvasshøe and Ivarsfjorden, respectively. We included this information in the revised manuscript.

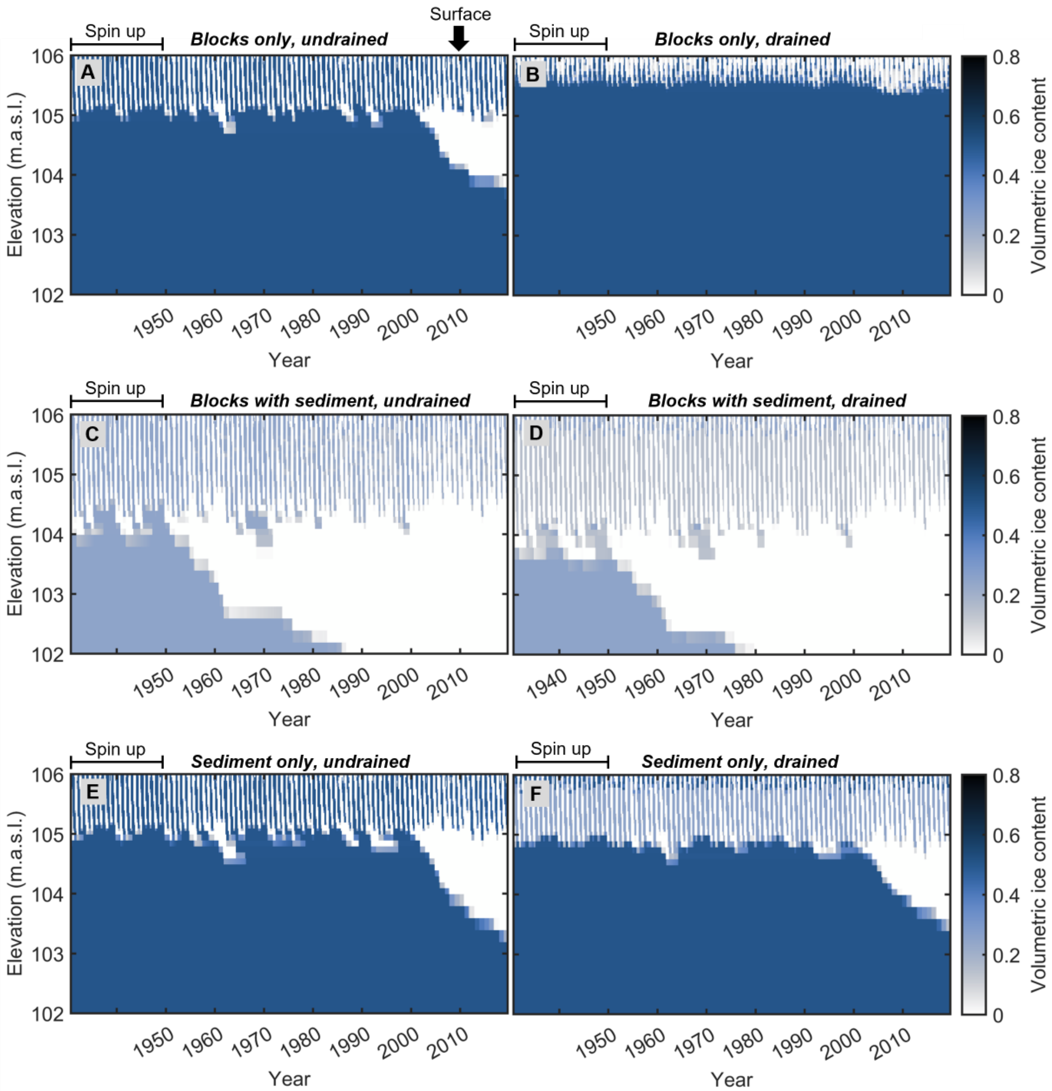
Line 402-404: The ERA5 reanalysis dataset allows us to simulate the evolution of the ground thermal regime and ground ice content from 1951 to 2019, during which mean air temperatures increased from -4.5 °C (1951-1960) to -3.8 °C (2010-2019) for Juvvasshøe and from 0.5 °C (1951-1960) to 1.2 °C (2010-2019) at Ivarsfjorden.

We did not run the model with a detrended dataset, as the goal is to investigate the effect of climate change (and thus air temperature increase) with the transient runs. The effect of soil stratigraphy and drainage is analyzed with the equilibrium runs.

We agree with the reviewer that more degradation is expected in the *blocks with sediment* scenarios as this stratigraphy features half of the porosity (and thus ice content) of the other two stratigraphies. We have now included this point in the text.

Line 413-415: The complete degradation in the blocks with sediment runs compared to partial degradation in all other scenarios (except blocks only, drained) is not unexpected since this stratigraphy has a 25% porosity (and thus ice content), compared to 50% in the others.

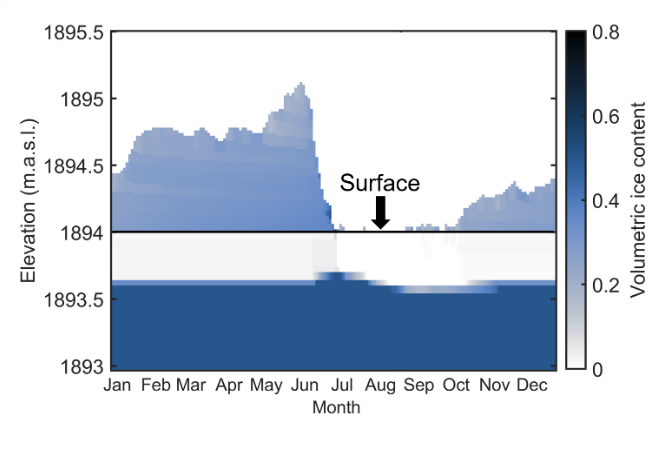
We thank the reviewer for pointing out the patchy lower ice content. We could explain this effect with an error in the initialization. We corrected the settings and performed the model runs again. The qualitative behavior the simulation results is the same as before, so this correction does not change any of the conclusions of the study.



Line 420-423: Figure 8: Modelled volumetric ground ice content at Ivarsfjorden between 1951 and 2019 for the idealized stratigraphies in undrained and drained conditions and sf = 1. The ground surface elevation is at 106 m.a.s.l. in the active layer, ice contents increase and decrease annually, corresponding to the active layer refreezing and thawing.

11 The authors kept referring to "at depth 5 m". Please draw/highlight surface elevation (datum) in the figures. For instance, what elevation would be “5 m depth” in Figure 7? It is not clear.

We follow the suggestion of the reviewer and highlighted the surface elevation in figure 7 and 8 (figure 6 and 7 in the old manuscript). Furthermore, we clarified the figure captions. For changes in figure 7, see response to comment 10. Changes in figure 8 are given below.



*Line 397-400**:** Figure 7: Modelled volumetric ground ice content in the upper 1 m of the ground (below 1894 m) and the snow cover (above 1894 m) for the blocks only, drained scenario, during one year of an equilibrium run at Juvvasshøe for sf = 0.75. Note the rise of the ground ice table in June after infiltrated snow melt water refreezes.*

12 The study is performed on an idealized column domain with a fixed surface datum. So, it would be easy for the reader to have the vertical scale in "depth" [0,5], instead of elevation, which I don't think is needed unless I am missing something.

The surface datum is indeed fixed in the model setup of this study. The forcing data is however downscaled for the exact surface elevations of the two sites. Since figure 7 shows both the snow cover above the surface and ground ice below the surface, we remain consistent and have elevation on the vertical scale. For clarity, we highlighted the surface elevation in both figure 7 and 8 (see major comment 10 and 11).

13 The focus of this work is to study the effect of soil stratigraphy (with drained/undrained conditions) on the soil thermal regime; however, no sensitivity study is performed on “soil stratigraphy”. For instance, how the blocks only scenario with porosities of 0.6 and 0.4 will affect permafrost conditions?

We performed a sensitivity study on different soil stratigraphies. The Supplement now contains results from additional simulations with a porosity of 0.4 and 0.6. For *blocks with sediment*, both the porosity of the blocks and of the sediment have been adjusted accordingly. The results show that ground temperatures at 2 m depth are within 0.4 °C between porosity 0.6 and 0.4. This means that the general results and conclusions are not influenced by small porosity changes.

Line 11-18 (in Supplement): Here, we provide the sensitivity of mean ground temperatures at 2 m depth for differences in porosity in the blocky layer (upper 5 m of the ground). Simulations are setup as in section 3.3.2 (equilibrium runs) but for three different porosities at a single snowfall factor. For blocks with sediment stratigraphy, we assume the porosity value for each the blocks and the sediment to be the same. For example, with porosity 0.4, blocks with 40% porosity, which are filled with sand which also has 40% porosity, resulting in a final porosity of 0.16 (0.32 for porosity 0.6).

Suppl. 2, Table 1: Equilibrium ground temperature (°C) at 2 m depth for the three idealized stratigraphies at three different porosities. The snowfall factors are the same as resulted from the model validation.

Site	Stratigraphy	Porosity 0.4	Porosity 0.5	Porosity 0.6
Juvvasshøe	*Blocks only*	-3.2	-3.2	-3.1
	Blocks with sediment	-3.0	-3.2	-3.4
	Sediment only	-3.7	-3.6	-3.4

(sf = 0.25)				
Ivarsfjorden (sf = 1)	*Blocks only*	-0.3	-0.4	-0.2
	Blocks with sediment	2.0	1.8	1.6
	Sediment only	1.8	1.6	1.5

Furthermore, the sensitivity towards the parameter d^{lat} has been tested and is presented in the supplement.

14 I would also suggest some of the sensitivity-related results, for example, section 4.2, to be moved to a supplemental document, and focus more on what is new here.

The insulating effect of the snow cover on the thermal regime has been studied before and is not a new finding in this study. However, the sensitivity towards the snowfall factor is an important finding in this study which we would like to keep in the main manuscript. Spatial variability of the snow cover is a typical phenomenon in the Norwegian mountains, and it is a key control for the existence of permafrost. In this study, we show that the snow threshold for permafrost existence strongly depends on the subsurface stratigraphy. Therefore, we analyze the magnitude of the thermal anomaly that the subsurface drainage induces at various amounts of snow and estimate a snow threshold for permafrost existence in the different model scenarios. To increase clarity, we made the following changes in the methods:

Line 297-301: As the heavily wind-affected snow cover is a key source of spatial variability in ground temperatures in the Norwegian mountains (Gisnås et al., 2014; Gisnås et al., 2016), the model is run for a range of snowfall factors between 0.0 and 1.5 (Table 2) for each scenario. This analysis allows us to identify the magnitude of the thermal anomaly that the subsurface drainage induces at various amounts of snow, as well as estimate the threshold snow amount for permafrost existence in the six scenarios.

and in the results:

Line 356-359: For snowfall factors of 0.75 and larger, the difference in ground temperature between blocks only, drained and the other scenarios is in the range of 1.1 °C and 1.8 °C at Juvvasshøe and in the range of 1.1 °C and 2.2 °C at Ivarsfjorden. This shows that the magnitude of the negative thermal anomaly increases with a larger amount of snowfall.

We also emphasized the importance of the sensitivity tests to snowfall factors in the abstract and conclusions:

Line 24-25: The thermal anomaly increases with larger amounts of snowfall, showing that well drained blocky deposits are less sensitive to snow insulation than other soils.

Line 589-591: The largest anomalies occur in simulations with a thick winter snow cover as ground temperatures in well drained blocky deposits are less sensitive to insulation by snow than other soils.

Finally, we removed the results of the sensitivity warming rates at different snowfall factors as this does not add enough to the objective of the study.

15 Also, in section 4.1 (line 307) authors mentioned all simulations used a single snowfall factor, however, later in section 4.3 they use different snowfall factors. This needs more explanation. Providing a table (which may not be in the main manuscript) listing all scenarios (other than the three listed in Table 1) will help the reader better understand it otherwise it is hard to untangle.

We agree that it was previously unclear which snowfall factors were applied in the different model scenarios. The revised manuscript includes a table, which gives the reader an overview of the simulations and applied settings, including snowfall factors.

Line 265-268: Table 2: Overview of basic model settings for the different simulation types. A spin-up of subsurface temperatures is achieved by repeated simulations for the spin-up period (until a stable temperature profile is reached), before the actual model run for the simulation period is conducted. "Idealized" stratigraphy and drainage refers to three subsurface stratigraphies (Table 1) combined with two types of drainage conditions. See Sect. 3.3.1 to Sect. 3.3.3 for details.

Run type	**Site**	**Spin up**	**Simulation**	**Stratigraphies**	**Snowfall factors**
Validation	Juvvasshøe	1951-2010	2010-2019	Best-fit	0.25
	Ivarsfjorden	1951-2016	2016-2019	Best-fit	1
Equilibrium	Juvvasshøe	2000-2010	2000-2010	Idealized	0.0, 0.25, 0.5, 0.75, 1.0, 1.5
	Ivarsfjorden	1962-1971	1962-1971	Idealized	0.0, 0.25, 0.5, 0.75, 1.0, 1.5
Transient	Juvvasshøe	1962-1971	1951-2019	Idealized	0.25
	Ivarsfjorden	1962-1971	1951-2019	Idealized	1

16 What type of soil retention curve is used in the study? Moreover, how sensitive are the results to hydraulic conductivity? I didn't find any mention of the role of spatial gradient (steepness). All these factors significantly impact drainage. Discuss.

We use a gravity driven bucket scheme, where water in excess of the field capacity infiltrates downwards. We clarified this in the revised manuscript:

Line 169-173: For soil hydrology, a gravity driven bucket scheme is used (Westermann et al 2022). Rainfall provided by the model forcing is added to the uppermost grid cell, while evaporation is determined by the surface energy balance calculations (note that we consider unvegetated surfaces and thus do not account for transpiration). Water that is in excess of the field capacity infiltrates downwards until either the water table or a non-permeable layer, such as a frozen grid cell is reached. If all grid cells are saturated, excess water is removed as surface runoff.

The description of the seepage face and the corresponding drainage has been revised and provides now a better overview of the influencing factors:

Line 173-185: We use a one-dimensional model setup, but simulate lateral drainage of water by introducing a seepage face, i.e. a lateral boundary conditions for water fluxes representing flow between saturated grid cells of the model domain and a stream channel (or the atmosphere) to which the water can freely flow out from the subsurface (e.g. Scudeler et al., 2017). Using the elevation of the water table, z_{wt} (computed as the elevation of the uppermost saturated grid cell), lateral water fluxes F_i^{lat} are derived for all saturated unfrozen grid cells i below the water table (i.e. at elevations $z_\mathrm{i} < z_\mathrm{wt}$) as

$$F_i^{lat} = -\mathrm{K}_H \frac{z_\mathrm{wt} - z_\mathrm{i}}{d^{lat}} ,$$

where K_H is the saturated hydraulic conductivity, d^{lat} is the lateral distance to the seepage face and the flux is determined by the difference between the hydrostatic potential (proportional to z_{wt}) of the water column and the gravitational potential of free water at the elevation of each cell (proportional to z_i). Note that Eq. (1) is an approximation for small changes of the water table and small outflow fluxes for which the potential in the saturated zone can be approximated by the hydrostatic potential. The parameter d^{lat} is used to control the strength of the drainage, with small distances resulting in a well-drained column, while high values lead to suppressed drainage.

In our study, we investigate only two extreme cases of essentially no drainage and very good drainage. A better description of the setup has been included in the manuscript:

Line 245-255: To investigate the effect of subsurface drainage on ground temperatures and ground ice conditions, we distinguish undrained and drained scenarios by using two different values of d^{lat} (Eq. 1) for in the idealized stratigraphies. A d^{lat} value of 10^4 m is used for undrained cases, which emulates conditions at a flat surface, resulting in a to a good approximation one-dimensional water balance, where only surface water is removed. For the drained cases, a d^{lat} value of 1 m is used, which results in well-drained conditions which are typical in sloping terrain. For the saturated hydraulic conductivity K_H, a fixed value of 10^{-5} m s^{-1} is used for all stratigraphies, although the true hydraulic conductivities almost certainly differ between stratigraphies. However, the key parameter controlling lateral water fluxes in Eq. 1 is in reality the "drainage timescale" K_H/d^{lat} [s^{-1}], which is varied by four orders of magnitude between K_H/d^{lat} = 10^{-5} s^{-1} (d^{lat} = 1 m, well-drained conditions) and K_H/d^{lat} = 10^{-9} s^{-1} (d^{lat} = 10^{-4} m undrained conditions). As the study setup is designed to analyze these two "confining cases", it is sufficient to only vary d^{lat} and leave K_H constant for simplicity. Further sensitivity tests for d^{lat} and K_H are provided in the Supplement.

In addition, we performed a sensitivity analysis to d^{lat} in the Supplement.

Line 20-27 (in Supplement): Here, we provide the sensitivity of mean ground temperatures at 2 m depth for differences in d^{lat}, which is the parameter used to control the drainage rate. Simulations are setup as in section 3.3.2 (equilibrium runs) but with five different values for d^{lat} and one snowfall factor. The increase of d^{lat} by one order of magnitude results in the same drainage rate as decreasing the K_H (saturated hydraulic conductivity) by one order of magnitude (see Eq. 1). sf = 1 is used, as differences between drainage rates are minimal for sf = 0.25 (Fig. 4).

Suppl. 3, Table 1: Equilibrium ground temperature (°C) at 2 m depth for the three idealized stratigraphies at five values of d^{lat}.

Site	Stratigraphy	d^{lat} 10^4 m	d^{lat} 10^3 m	d^{lat} 10^2 m	d^{lat} 10^1 m	d^{lat} 10^0 m
Juvvasshøe	*Blocks only*	0.3	0.0	0.0	-0.7	-0.9
	Blocks with sediment	0.3	0.3	0.3	0.2	0.2
(sf = 1)	*Sediment only*	0.3	0.3	0.3	0.2	0.0
Ivarsfjorden	*Blocks only*	1.3	0.3	0.1	-0.1	-0.4
	Blocks with sediment	1.8	1.8	1.8	1.8	1.8
(sf = 1)	*Sediment only*	1.7	1.7	1.7	1.6	1.6

Minor comments:

1. Line 13: effect of drainage on what? soil thermal regime? please explain

We added the explanation.

Line 13-15: Here we used the CryoGrid community model to simulate the effect of drainage on the ground thermal regime and ground ice in blocky terrain permafrost at two sites in Norway

2: Line 15: please explain here what type of model domain was considered? Is it a 2D generic hillslope or observed 2D/3D topography?

We added that model domain is one-dimensional.

Line 15-16: The model setup is based on a one-dimensional model domain and features a surface energy balance, heat conduction and advection, as well as a bucket water scheme with adjustable lateral drainage.

3. please clarify what does drained/undrained mean here. saturated or unsaturated conditions?

We shortly explained what differentiates the drained and undrained scenarios.

Line 16-18: We used three idealized subsurface stratigraphies, blocks only, blocks with sediment and sediment only, which can be either drained (i.e. with strong lateral subsurface drainage) or undrained (i.e. without drainage), resulting in six scenarios.

4. remove quotes

Agreed and the quotes have been removed.

5. I assume it means, there is more permafrost in the blocks only, drain case. How much in terms of percentage as compared to other two cases?

We rephrased this sentence in order to include a quantitative statement about ground ice loss. These percental decreases in ground ice content reflect the volumetric loss of ground ice between the active layer and the top of the bedrock. These numbers are also included in the results chapter.

Line 27-30: Finally, transient simulations since 1951 at the rock glacier site (starting with permafrost conditions for all stratigraphies) showed a 100% lowering of the ground ice table in the blocks with sediment, drained run, 37% lowering in the sediment only run and only 2% lowering in the blocks only, drained run.

and

Line 408-413: In the blocks only, drained scenario, the perennial ice table in the upper 5 m (so between the active layer and the bedrock) does not lower by a significant amount (2 % lowering), while the ice table lowers by 33 % in the blocks only, undrained scenario. The ice table in the blocks with sediment stratigraphy disappeared by 1985 and 1975 in the undrained and drained scenarios respectively. Finally, the sediment only simulations show an intermediate effect where the ice table has lowered by 41 % and 39 % for undrained and drained conditions respectively by 2019.

6. would be nice to provide a rough number here, like what is that limit you are *referring* to?

We included estimations of elevational permafrost limits from (Etzelmüller et al., 2003; Gisnås et al., 2017) for the region of the two study sites in the Introduction.

Line 50-52: In Southern Norway, the lower limit of mountain permafrost is estimated between 1600 m a.s.l. in the west to 1000 m a.s.l. in the east (Etzelmüller et al., 2003), while a similar west-east decrease from 800–1000 m a.s.l. to ca. 300 m a.s.l. in the east is observed in Northern Norway (Gisnås et al., 2017).

7. this effect? this is unclear. does it refer to the role of heat conduction, which is 'mostly' included in permafrost simulation, or does it say 'consider simulating permafrost below that assumed elevational limit?

We clarified that that it regards the subsurface water/ice balance.

Line 32-33: It is thus important to consider the subsurface water/ice balance in blocky terrain in future efforts on permafrost distribution mapping in mountainous areas

8. be specific here. not all mountain environments have permafrost

We include a clarification regarding the high latitude and/or high altitude of mountain permafrost environments

Line 36-38: Permafrost is defined as ground that remains at or below 0 °C for two or more consecutive years (Van Everdingen, 1998) and is a common feature at high elevations and/or mid-latitude mountain environments, where permafrost occurs even in mid- and low-latitudes (Gorbunov, 1978)

9.for a smooth flow, at least mention different types of permafrost here, such as continuous, discontinuous, sporadic.

We included an extra sentence that explains the different classifications of permafrost regarding aerial extent.

Line 38-40: Different permafrost zones are classified based on the aerial extent of permafrost presence. These zones are: continuous, discontinuous, sporadic and isolated, where the surface in underlain by permafrost in more than 90%, 50-90%, 10-50% and less than 10% of the land area respectively (Smith and Riseborough 2002).

10. again, what is that assumed elevation limit

We included estimations of elevational permafrost limits from (Etzelmüller et al., 2003; Gisnås et al., 2017) for the region of the two study sites in the Introduction.

Line 50-52: In Southern Norway, the lower limit of mountain permafrost is estimated between 1600 m a.s.l. in the west to 1000 m a.s.l. in the east (Etzelmüller et al., 2003), while a similar west-east decrease from 800–1000 m a.s.l. to ca. 300 m a.s.l. in the east is observed in Northern Norway (Gisnås et al., 2017).

11. not clear if this is authors' analysis of Juliussen and Humlum work or they are just refering to their work.

The Introduction has been restructured for clarity, it is now clear that we refer to the work of Juliussen and Humlum.

12. please provide some specific examples of applications

A selection of studies that were performed with CryoGrid are listed in order to provide the reader examples of previous work with the model.

Line 95-99: It largely builds on the well-established CryoGrid 3 model (Westermann et al., 2016), which has been used in e.g. peat plateaus and palsas (Martin et al. 2021), ice-wedge polygons (Nitzbon et al. 2019) *and boreal forests (Stuenzi et al. 2021) and has a broad range of applications, including the representation of lateral drainage regimes (Martin et al., 2019), representation of steep rock walls (Schmidt et al., 2021) and massive ice bodies.*

13. rephrase

The entire paragraph was rephrased.

14. mean annual ground?

This value of -2.5 °C covers the entire period of 2000 and 2004. We use the term mean ground temperature in the same way as the source of this value (Isaksen et al., 2007).

15. mean annual? I see the unit here is mm/yr (rate), but general practice is to provide "mean" in mm (amount)

Agreed and adjusted.

Line 118-119: The mean annual precipitation was estimated to be between 800 and 1000 mm.

16. I guess, Mean annual ground surface temperature? this is never defined before. please define it before using it.

We included the definition

Line 121-123: Isaksen et al. (2007) measured the difference between the mean annual ground surface temperature (MAGST) and mean annual air temperature (MAAT), which is the surface offset at exposed and less exposed sites in this area.

17. and how deep is the permafrost table? i.e., depth of permafrost from the surface at the exposed sites and/or sites with significant snow cover. If you have this information available it would be good to add.

We included information about the active layer thickness (and thus depth to the permafrost table).

Line 125-127: The thickness of the active layer increased from 215 cm in 1999 (Isaksen et al., 2001) to ca. 250 cm in 2019 (Etzelmüller et al., 2020).

18. it needs to be defined, if it has not been defined already

See comment 15, it has now been defined before.

19. how is this depth picked for constant water/ice content layer? and how is this depth related to the damping depth of the surface thermal signal?

This paragraph was completely rephrased. The comment referred to an example in the old manuscript, which was not relevant for the presented study. It is deleted in the new manuscript (see as well response to major comment 5).

20. this also needs some explanation. Like what type of snow processes? aging, compaction, density variations etc.? It would be hard for someone not familiar with CryoGrid to follow it.

This paragraph was completely rephrased. The comment referred to a rather abstract example in the old manuscript, which was not relevant for the presented study. It is deleted in the new manuscript (see as well response to major comment 5 and minor comment 20).

21. how do you compute the water table location? Explain

The water table is computed by taking the uppermost saturated grid cell. We clarified that in the description of the drainage scheme.

Line 176-177: Using the elevation of the water table z_{wt} (computed as the elevation of the uppermost saturated grid cell), lateral water fluxes…

22. above this water table?

Water below the water table is removed (and thus the water table lowers). Water above the water table is held by the field capacity or infiltrates downwards and is thus not removed with the drainage component. We clarified this in the manuscript:

Line 173-178: We use a one-dimensional model setup, but simulate lateral drainage of water by introducing a seepage face, i.e. a lateral boundary conditions for water fluxes representing flow between saturated grid cells of the model domain and a stream channel (or the atmosphere) to which the water can freely flow out from the subsurface (e.g. Scudeler et al., 2017). Using the elevation of the water table, z_{wt} (computed as the elevation of the uppermost saturated grid cell), lateral water fluxes F_i^{lat} are derived for all saturated unfrozen grid cells i below the water table (i.e. at elevations $z_{\mathrm{i}} < z_{\mathrm{wt}}$) as…

23. How is this 5 m depth picked or consistent with observations?

At Juvvasshøe, bedrock is observed at 5 m depth. We included a clarification. We do not have observations at the rock glacier in Ivarsfjorden. An estimate of 5 m is reasonable and most importantly assures consistency between the model runs.

Line 232-234: For all stratigraphies, bedrock (3% porosity and saturated conditions, e.g. Hipp et al. 2012; Fabrot et al. 2011) is assumed below 5 m depth, which is in line with observations from Isaksen et al. (2003) at Juvvasshøe.

24. if it is zero for all cases, it can be removed from the Table and just mention it in the main text.

We agree and have removed it from the table and included it in the main text

Line 234-235: Additionally, none of the stratigraphies contain soil organic matter.

25. have a separate column for the "three cases" instead of putting it under the "Depth" column

We agree, the table is adjusted.

26. Seems to me that Figure 2 does not support this statement, as at depth 4 m, the model fails to follow the observed trend particularly in the spring shoulder season for all years. Is it due to bottom boundary condition or snow model or thermal conductivities? How the authors ensured that the simulations won't be affected by the bottom boundary condition if a 5 m domain is used?

We assume the referee refers to a depth of 2 m. To address this and other comments, we have included a statistical analysis of model fit. At both of the sites, the bias and RMSE are calculated with daily values.

Line 291-293: At both sites, the root-mean-square-error (RMSE) and bias are calculated in order to provide an objective measure of the model fit. At Juvvasshøe this was accomplished for daily values at 0.4 m and 2 m depth, while at Ivarsfjorden the mean daily ground surface temperature of the loggers within the rock glacier outline is used.

Line 328-330: This configuration used drained conditions, although differences with undrained conditions are minimal for this stratigraphy. For daily temperatures at 0.4 m depth, the RMSE and bias are 2.1 °C and -0.6 °C, respectively, while they are 1.2 °C and -0.7 °C at 2 m depth.

Line 341-342: The best-fitting model configuration was found to be the blocks with sediment stratigraphy and a snowfall factor of 1.0, resulting in an RMSE of 1.3 °C and a bias of -0.4 °C.

Furthermore, we added a possible explanation for the mismatch of spring temperatures:

Line 330-335: There is a mismatch in the timing of spring temperatures at 2 m depth in several years, for which modelled temperatures increase later than measured values. This is likely a result of differences in the snow melt, as the snowpack dynamics resulting from wind redistribution is not completely captured by the snowfall scaling with a constant snowfall factor (e.g. Martin et al., 2019). Furthermore, the uppermost 1 m contain large stones and boulders, while the layer below is characterized by smaller stones and cobbles (Isaksen et al. 2003), so that a ground stratigraphy with two layers in the uppermost 5 m may further improve the performance of the simulations.

The model domain depth is 100 m, so that the lower boundary conditions does not affect the results in this study (see response to major comment 8).

27. how well modeled MAGST matches the observed for the other site?

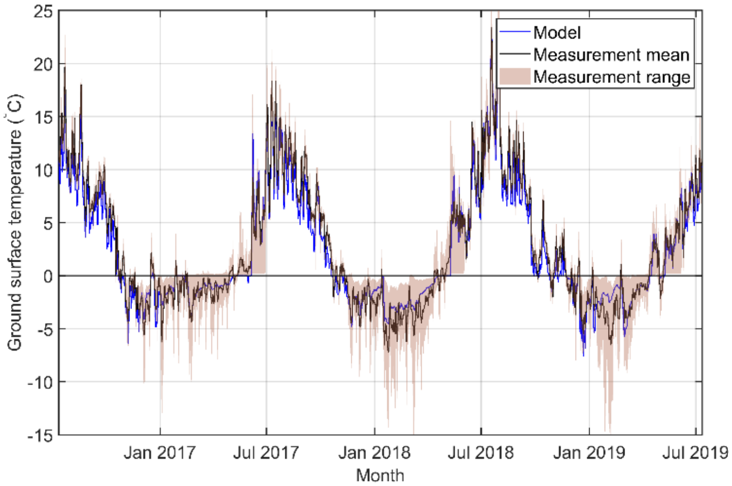
At Juvvasshøe, ground surface temperatures are not monitored, but we have near-surface measurements which are equally suited for model validation. The analysis/comparison is done at 0.4 m depth, and at 2 m depth.

28. I found it confusing. Capturing general trends in the ground surface temperature is no guarantee that soil thermal state is represented accurately due to the complex and nonlinear nature of the subsurface.

We agree with the reviewer and added a statistical analysis (RMSE, bias) for daily ground surface temperatures at Ivarsfjorden:

Line 291-293: At both sites, the root-mean-square-error (RMSE) and bias are calculated in order to provide an objective measure of the model fit. At Juvvasshøe this was accomplished for daily values at 0.4 m and 2 m depth, while at Ivarsfjorden the mean daily ground surface temperature of the loggers within the rock glacier outline is used.

Furthermore, we present a new figure with daily resolution for Ivarsfjorden:

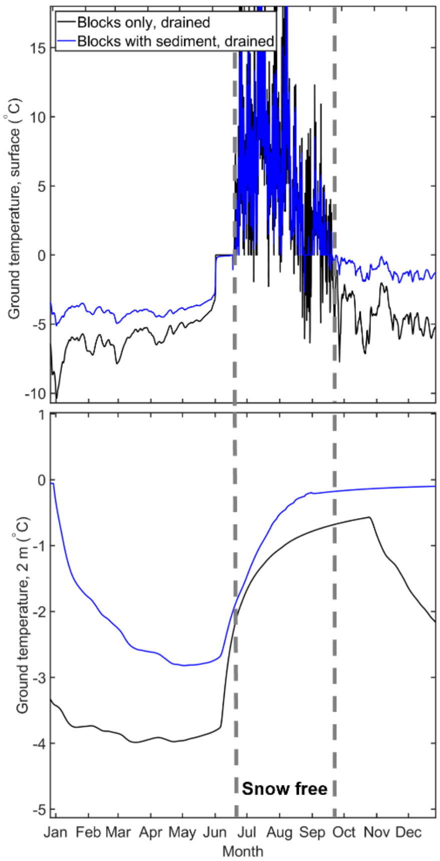


Line 349-352: Figure 4: Daily modelled and measured ground surface temperatures in Ivarsfjorden from July 2016 to July 2019. The shaded area indicates the minimum to maximum range of measured daily values from 11 loggers (based on Lilleøren et al., 2022), while the black line represents the mean value of all loggers.

Unfortunately, no boreholes with ground temperatures measurements exist in rock glaciers of similar interest in Norway. We therefore use 11 temperature loggers, which provide a good overview of the ground surface temperatures of Ivarsfjorden.

29. highlight the summer reason in Fig. 5

We highlighted the snow-free summer period in the figure.



Line 385-388: Figure 6: Modelled ground temperature at 0.05 m (top) and 2 m (bottom) depth for the blocks only, drained and blocks with sediment, drained scenarios during a year of an equilibrium run at Juvvasshøe for sf = 0.75. The snow-free summer season is highlighted. Note that the upper plot is truncated at 17 °C, maximum summer temperatures are 26 °C in both scenarios.

30. The upper plot seems truncated vertically. Either adjust it or mention it in the caption

We mentioned it in the caption.

Line 387-388: Note that the upper plot is truncated at 17 °C, maximum summer temperatures are 26 °C in both scenarios.

31. This needs more details, dry soils have low thermal conductivity, so how can it enable rapid refreezing? There is always a competition between the soil thermal conductivity and heat capacity.

We added a clarification on that in the revised manuscript:

Line 511-513: Dry soils have a lower thermal conductivity compared to wet soils, but the lack of latent heat release allows for rapid refreezing during fall which enables fast cooling of the deeper soil layers and thus leads to overall lower winter temperatures.

32. 1. What is the surface elevation? highlight it on the plots. 2. I wonder how was this model initialized that the authors got low water content around 104 m and above/below the water content is very different and high? 3. Same for bottom right plot. what is causing the patchy low water content areas?

1. The surface elevation is 106 m, which is the top of the plot. In order to clarify that this figure shows only the subsurface, we added to the figure caption and highlighted the surface in the plot. We did the same for other plots of ground ice and snow cover (see major comment 11).

2.& 3. The model is initialized with saturated conditions and an error in the simulation setup was corrected, removing the patchy ice contents (major comment 10).

33. Provide more details in the caption and letter the subplots for easy referencing.

We lettered the subplots for easier referencing. Furthermore, we provided more details in the caption:

Line 421-423: Figure 8: Modelled volumetric ground ice content at Ivarsfjorden between 1951 and 2019 for the idealized stratigraphies in undrained and drained conditions and sf = 1. The ground surface elevation is at 106 m a.s.l. in the active layer, ice contents increase and decrease annually, corresponding to the active layer refreezing and thawing.

34. This needs to be explored, that how much of subsurface warming is due to stratigraphy, soil moisture, and how much the change in the air temperature caused over the period 1951-2019, while keeping the effects of snow isolated as well.

It is not really possible to separate these effects in a highly coupled model like CryoGrid. Changes in air temperature, for example, also impact the snow cover through winter melt episodes which again change the insulating effect of the snow cover. As this study clearly shows, the effect on ground temperatures is then once again modulated by the subsurface ice dynamics, creating a system where all variables are coupled at least to a certain degree. While it would in principle be possible to perform experiments where the snowfall forcing of e.g. the first decade is looped (while all other parameters evolve as delivered by the ERA-5 reanalysis), we do not think that this adds anything to the analysis – also in the climate system (and the ERA-5 data which is a best-guess representation of it), there are correlations between snowfall and air temperature, so we would in this case investigate a highly artificial system which is difficult to relate to reality.

35. how this connects with the abstract and results? It tells that the main focus of this study was to investigate the effect of snowfall on the soil thermal regime, which I don't think is the case.

We agree with the reviewer and rephrased the sentence.

Line 581-584: In this study, we used the CryoGrid permafrost model to simulate the effect of blocky terrain on the ground thermal regime and ground ice dynamics at two Norwegian mountain permafrost sites (Juvvasshøe and Ivarsfjorden rock glacier). In particular, we investigated the effect of subsurface drainage, as typical on slopes, for three idealized stratigraphies, named blocks only, blocks with sediment and sediment only.