Response letter to Referee #1

Our responses are written in blue font.

(Examples of) line numbers in the **revised manuscript** that contain changes according to the comment are given in red font.

General Comments

1. As stated on L135-136, the "downhill end of the transect represents the valley bottom and allows for water accumulation and potential ponding on the surface". According to Figure 2, this downhill boundary is a no-flow and no-heat flux boundary but there is little justification for this no-flow condition. It looks like in Figure 1c that these hills ultimately flow into a river so are instead, flow boundaries. It may make more sense to represent these as flux or constant pressure boundaries. As no-flow boundaries, I worry that they are artificially blocking heat transport and accumulating water, which is why the rightmost column in Figure 4 does not make sense with the rest of the modeled cross-section. If these no-flow boundaries are not affecting the model outputs, it would be helpful to see a comparison of model outputs with and without the no-flow downstream boundary in the supplements. If this downslope no-flow boundary does change results, please revisit much of the results text, including your third conclusion.

Response: Thank you for this important comment. The use of no-flow and no-heat flux boundaries on the vertical sides of the domain is intentional. The conceptualization is that the no-flow boundary condition represents a watershed boundary on the uphill side and a symmetry boundary condition on the downhill side, the latter corresponding to flow accumulation from both sides of a symmetric V-shaped hillslope valley transect. We realize this was not clearly stated in the manuscript and have made efforts to clarify this in the revised model description section.

We are not attempting to simulate a valley that ends in a perennial stream or river, where a constant head boundary could be suitable. Instead, the surface mesh of the model allows for ponding and ice/snow accumulation to occur. Surface water ponding can be combined with a spill-over threshold condition to limit excessive ponding, thereby corresponding to a maximum depth of a surface water body. This implementation thereby avoids the need for a constant head boundary, and allows for surface water to form as a result of the flow and energy balances and the hydro-meteorological forcing of the system. The maximum depth of the surface water body is then the height of the spill-over condition. However, in our simulations, no surface ponding occurs. This is due to the relatively dry conditions of the site and because evapotranspiration allows for much water to be removed from the system prior to saturating the active layer.

However, we acknowledge that a V-shaped geometry is not so representative for typical valley transects on Svalbard. We have therefore redesigned the model domain to resemble a simplified U-shaped geometric conceptualization, by extending the mesh to account for a flat valley bottom subsequent to the slope (see Fig. 2 in the main text and Fig.1 in this letter); the flat part is an addition of 16 meters, represented by 8 mesh elements/columns). This means that heat and water can now move out of the lowermost slope-column into the flat part of the domain, corresponding to the valley bottom. The same no-flow boundary conditions are applied to the rightmost vertical edge in order to produce a symmetry boundary for the downhill side, and as before, ponding and ice/snow accumulation is allowed to occur on the surface mesh of the domain. For the analysis and presentation of results, we still only consider the first 50m of the domain (the sloped part of the domain).

We have evaluated potential boundary effects of proximity to the right edge of the domain (the downslope side) by examining yet another domain configuration, consisting of a flat extension of only 4 meters (2 columns). That case yielded similar results as the 16-meter extension used in the main revised simulations. Thus we conclude that a 16-meter extension for representing the valley bottom is more than sufficient to be a safe distance from the right symmetry boundary and to avoid any boundary/edge effects.

This revised model domain configuration has changed some of our results, specifically they show a reduced moisture accumulation on the downhill side compared to the previous model, and subsequently a dampened cooling effect in the downhill section. However, the uphill warming effect is largely unchanged. We have adjusted the text and figures in the manuscript accordingly.

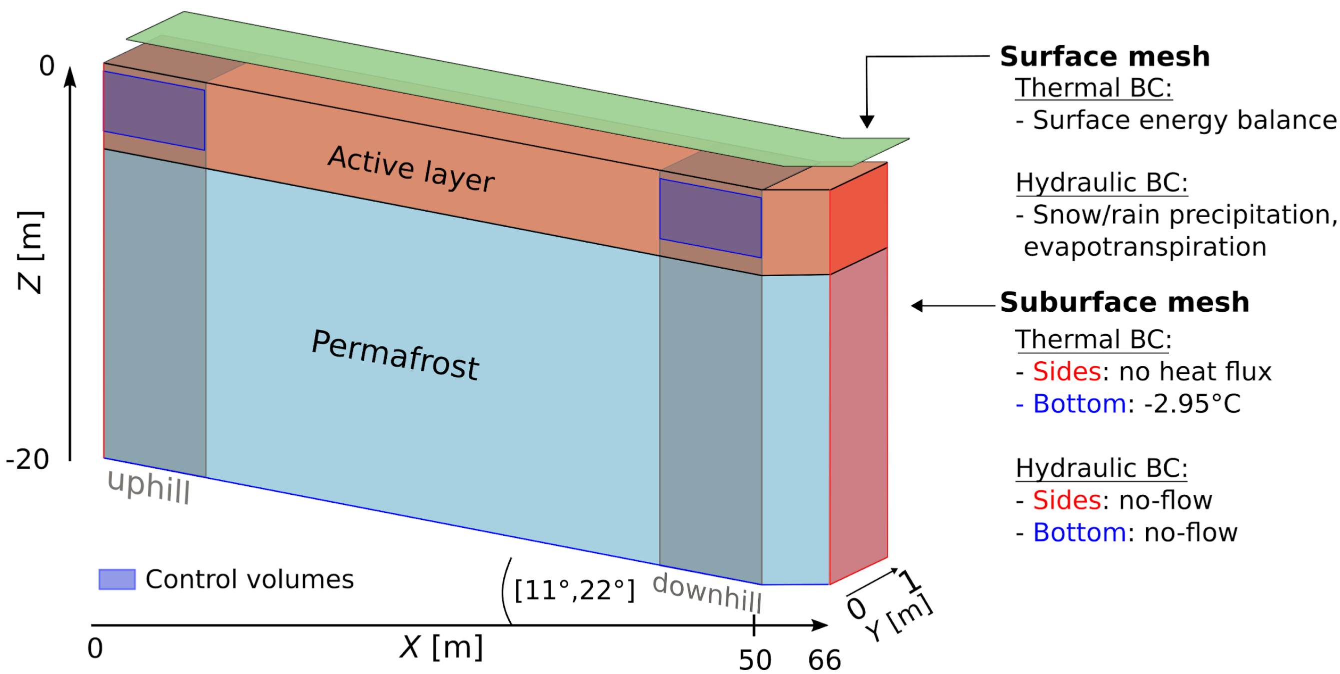

Figure 1: Conceptual representation of the surface and subsurface modeling domain. Grey shaded areas on either side of the transect indicate the uphill and downhill observation locations, red indicates the sides of the model, blue boxes represent the control volumes (CV) and a blueline at the bottom indicates the bottom boundary. Thermal-hydraulic boundary conditions (BC) on the surface, sides and bottom are listed on the right.

E.g.: L144-147 and L160-166

2. The presented models are referred to as idealized but are based on field data from Adventdalen, Svalbard which makes me wonder why a calibration was not performed? I think that uncalibrated models can be useful thought experiments, and I understand that calibrating and validating a model can be taxing. However, I question the validity of using conclusions from a model that is not calibrated to existing field measurements, especially using a model that is in the middle

ground between an uncalibrated generalized model and a model that is calibrated. At a minimum, the authors need to suggest how the model results may compare to field observations of similar sites.

Response: Indeed, our model is in the middle ground between an idealized and a site-specific/calibrated model. Since we do not have subsurface data from any of the slopes in the area, it is not possible to conduct a proper calibration. However, as mentioned in the text, a similar site in the Adventdalen valley bottom (the UNISCALM site) has been investigated previously by Schuh et al. (2017). We refer to this as a 'similar site' and expect similar results for our reference case (with no slope). In the study by Schuh et al. (2017), model results showed good agreement with subsurface observations (especially temperature and thaw depth) over a time period from 2000 to 2014, even though model parameters were derived from literature. Furthermore, their simulated active layer depths of around 100cm is consistent with the measured active layer depths in that area (see technical comment no. 5). In our simulations we also obtain similar active layer depths. Since the time duration of the subsurface observation dataset of the UNISCALM site does not overlap with the available hydro-meteorological dataset, a calibration for the flat model case is not realistic. We have added a clarification in the paragraph that describes the material properties, which indicates that these properties have been found to accurately represent the subsurface at the UNISCALM site.

E.g.: L155-159

3. It is unclear how relevant these findings are to permafrost landscapes throughout the Arctic. How often are there hillslopes of a constant slope without valleys and lateral (cross hillslope) water flow? Even more basic, what percent of the Arctic is sloped terrain? Any additional information that could be provided to aid in the upscaling of these results outside of Svalbard would be beneficial.

Response: Thank you for this great idea. To address the question how representative the slopes are throughout the Arctic, we studied slopes in different regions (countries with administrative areas in the Arctic) around the Arctic based on a digital elevation model (ArcticDEM, 10m resolution). To perform the necessary calculations, we developed an algorithm which classifies the slopes according to four categories of different inclination with GIS software. We only consider areas of continuous permafrost in this evaluation. A figure with the results that has also been incorporated in Fig. 1 in the main text can be found below (Fig. 2 in this letter).

We added information on the methodology as well as the results for the individual pie charts into the supplementary information of the revised study. We found that, as expected, most of the landscape is comparably flat (0-5°). However, slopes in both categories defined in our paper, 5-15° and 15-25°, are represented in every region and range between 12-30% and 2-14%, respectively. Regions, which stand out in terms of significant proportion of land area with moderate to steep slopes include Yukon, Greenland and the Far East Federal District in Russia, together with Svalbard. Thus we feel it is important to study permafrost and active layer dynamics in hillslopes, certainly for Svalbard, but also for the Arctic in general.
Furthermore, we did not investigate complex topographies like cross hillslope water flow as the idea is to simulate a hillslope along a preferential flow pathway.

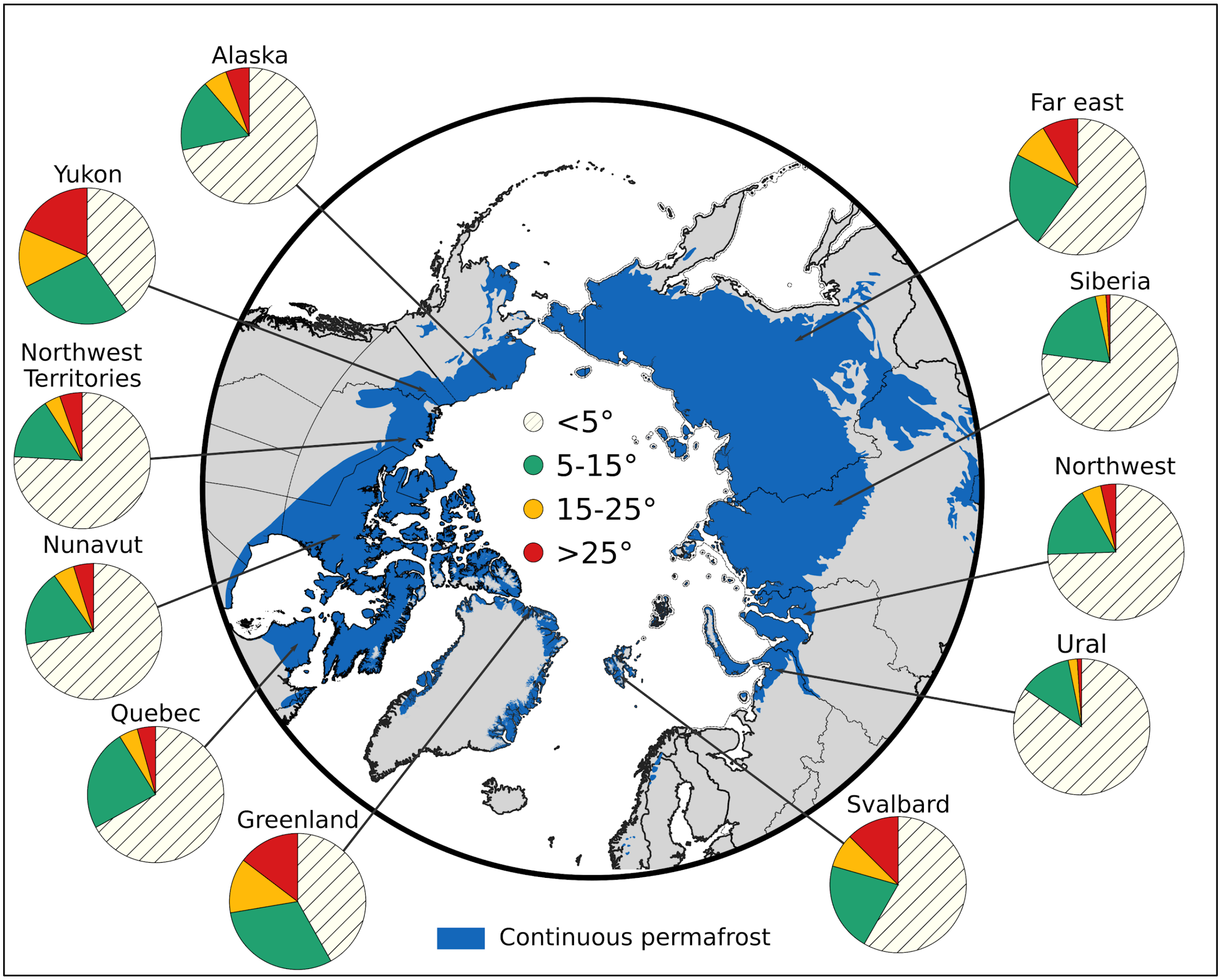

Figure 2: Classification of slopes around the Arctic according to the classification in this manuscript. Grey areas show land masses, blue areas indicate continuous permafrost areas and each pie chart represents a different administrative region in the Arctic. Slopes have only been calculated for areas of continuous permafrost and within the extent of the ArcticDEM.

E.g.: L132-137 and L454-457

4. The assumptions of the study, especially the modeling assumptions, should be specifically stated in a separate section. For instance, this simulation doesn't include an organic layer, but organic layers exist in many permafrost landscapes and have very different thermal properties from mineral soil (this goes back to if these results can be upscaled or not). Is it reasonable to model hillslopes in only two-dimension? I think it can be but the reason for doing so needs to be stated and supported with other peer-reviewed papers.

Response: Thank you for this comment. We agree that the justification about the model domain setup should be clarified; we have made efforts to improve this in the revised manuscript, specifically we added an additional study about 2D slope simulations to support our study (Jan and Painter 2020 and Jafarov et al. 2018).

Even though it is correct that an organic layer is common in permafrost landscapes, we decided to exclude this from our simulations for mainly two reasons. First, the material along slopes (at least in Svalbard) is often much less organic than in the valley bottoms and sometimes completely absent (depending on the steepness), and we wish to keep identical subsurface textures between the different slope inclination cases to enable consistent comparisons. Second, we briefly looked at the effects of organic layers on our preliminary investigations and saw that it made substantially more difficult to understand and untangle different processes; including subsurface heterogeneity would be beyond the scope of this analysis but we are considering pursuing this line of investigation in future efforts.

E.g: L156-157 and L151-152

5. The main text needs to be revised for clarity. The figures are attractive and easy to see, which is appreciated, but many of the figures need additional annotations or subfigures to help with comprehension. It is also unusual to have the results and discussion sections combined. I would highly recommend separating these sections so you can have a more thorough discussion section where you interpret your results and compare them to other peer-reviewed studies. As is, this combined section is quite long and hard to digest. I have pointed out some specific examples below where the text and figures need to be revised for clarity.

Response: Thank you for this feedback. We have opted for combining text on results and analysis/discussion because we find this to be the most efficient and clear way to present and understand these findings. This study uses a physically-based numerical model to both analyze and explain system behavior, and due to the coupled nature of many of the physical processes and quantities we feel the most convenient and clear way to understand the findings is by a combined presentation of results with analysis and discussion.
We have made efforts to improve the text and presentation by incorporating the specific and technical comments below and restructuring the text where needed for better readability. We have also revised some figures and several of the figure captions, to help improve the clarity, and included additional information, which we hope helps to interpret them more easily.

See line numbers in the following comments

<u>Specific Comments</u>

1. L6, How representative are these hillslopes of Arctic landscapes as a whole?

Response: We have analyzed hillslope inclinations of Arctic landscapes, please see our response to the general comment no. 3 above.

L132-137

2. L15, Since this study only considers one slope versus a 'hilly' landscape, I would hesitate to draw this conclusion about hilly terrain.

Response: Replaced 'hilly' with 'sloped'

L15

3. L29, Rather, permafrost degradation has been found to increase groundwater discharge into surface waters, not decrease the seasonal variability.

Response: Permafrost degradation has been shown to specifically reduce seasonal variability in groundwater discharge, please see:

Frampton, A., Painter, S., Lyon, S.W., Destouni, G., 2011. Non-isothermal, three-phase simulations of near-surface flows in a model permafrost system under seasonal variability and climate change. Journal of Hydrology 403, 352–359. https://doi.org/10.1016/j.jhydrol.2011.04.010

Frampton, A., Painter, S.L., Destouni, G., 2013. Permafrost degradation and subsurface-flow changes caused by surface warming trends. Hydrogeology Journal 21, 271–280. https://doi.org/10.1007/s10040-012-0938-z

To clarify this statement the citations have been updated in the revised manuscript.

No changes.

4. L41, How much topography is 'more topography'? Is there are slope cutoff? Be specific.

Response: Removed 'and landscapes with more topography'

L41-42

5. Section 2.1, What are typical active layer depths at the study site?

Response: Added a sentence: Active layer depth in the area ranges from 90 to 110cm (measured between 2000 and 2018; Strand et al., 2020)

L107-108

6. L107, How far is this from the weather station in km?

Response: Added information in brackets: *Precipitation data was retrieved from the long-term weather station at Longyearbyen airport (9 km west of the Adventdalen weather station; 78.24◦N 15.51◦E)*

L112-113

7. L118, What is the hillslope length?

Response: Added information in brackets: *To inform the model, the same forcing dataset is used for the entire model domain (50m transect length) without accounting for temperature lapse rates between the lower and upper part of the transect.*

L120-122

8. Figure 2, It would be helpful to show node locations.

Response: We agree that the node location is an interesting additional information. Unfortunately, the mesh resolution is too high to meaningfully include it into the original figure (Fig. 2 in the main text) without making it excessively cluttered. However, we will include a figure showing the three different meshes in the supplementary material, and also provide the mesh files (.exo format) in the data repository.

L150-151 and 507-511

9. L136, What is the depth of the mineral soil?

Response: The depth of mineral soil is 20m, which is the domain depth (homogenous throughout the domain). We added a clarification of this, as follows: *All cases assume a homogeneous material throughout the model domain consistent with mineral soils typically encountered in the area.*

L155-156

10. Table 1, Thermal conductivity values are low for a mineral soil, I would expect closer to 2.7 W/mK.

Response: Thank you for catching this. This was indeed a mistake, as we intended to use the same material properties as in Schuh et al. (2017). We have re-run the simulations with liquid saturated thermal conductivity = 1.7 W/mK , and dry thermal conductivity = 0.27 W/mK (following Woo 2012 (Table 2.1) as described in Schuh et al. 2017) and updated the parameters in Table 1. This correction has changed our results only slightly; we now obtain deeper active layer depths which in fact are even closer to typical measured active layer depths of the general valley bottom region in Adventdalen, as presented by Strand et al. and Schuh et al. (around 1m).

Table 1

11. L140, What makes the no flow lateral (right and left) boundaries? Are they a watershed divide? Seems unlikely given the flat lateral topography.

Response: Thank you for your question. Indeed, the vertical boundary on the upper (left) side represents a water divide, while the vertical boundary on the lower (right) side represents a valley bottom and is a symmetry boundary for flow accumulation (and not necessarily ending in a surface water body), please see our response to general comment 1 above. Given the length of the slope-transect (50m), the definition of a water divide on the upslope boundary might not be applicable to all slopes but serves as a suitable compromise between conceptual representation of a hillslope and computational effort for analysis. We further discuss this issue in the outlook section (Section 3.7). To clarify this, we have added a sentence in the simulation configuration section

L160-166

12. Figure 3, I'm confused about what these plots are showing. It would help with clarity to first plot the temperature time series for the steep, medium, and flat simulations for both uphill and downhill and then plot these differenced values. Would also be helpful to annotate this figure, for example, you could write 'uphill warmer' above the x-axis in (a).

Response: We included additional information in the figure caption to help guide the interpretation. Unfortunately, it is not possible to include the time series in the main figure, but we agree that it is useful to see it as well. As a solution we added a figure (Figure S3) including the time series in all depths for up- and downhill locations in the supplementary information.

L215-216

13. L192, What about the large November peak at 0.75 m in Figure 3a?

Response: The temperature differences in November are particularly high close to the permafrost table as this part of the active layer freezes faster and gets overall colder. This is likely due to higher thermal conductivity when air temperatures drop in winter and the presence of the permafrost, which is acting as an additional heat sink. We added a sentence in section 3.1 to describe this in more detail.

L223-224

14. Figure 4, Label color bar with units. Which is the uphill side? Horizontal distance=0 m? Label this on the figure. Why is the far-right column in each subfigure so different than the other columns? It looks like a potential modeling error due to the no-flow boundary conditions that do not physically make sense. This difference was pointed out in section 3.2 but is concerning. Also, are the two dotted lines in the October subfigures showing the presence of a horizontal talik?

Response: We added labels for the color bars and added two labels on the x-axis indicating up- and downhill, where x=0 m corresponds to the uphill side and x=50 m to the downhill side. The last column receives water from uphill and therefore exhibits different behaviour; however we have now changed the geometry of the domain by including a 'valley bottom' and have addressed concerns of potential boundary effects, please see our response to general comment 1 above. A horizontal talik during freeze-up is indeed present and indicates two-sided freezing, and the December image of Figure 4 in the main text shows that this talik is not permanent; this is now clarified in the main text.

Figure 4

15. L279-280, This is likely due to the no-flow boundary on the downslope side, and is not realistic if there was a river or otherwise at this boundary.

Response: Please see our response to general comment 1 above, where we address the concerns of domain geometry and boundary conditions.

See general comment 1

16. L284-286, What about the role of specific heat, where specific heat is higher for saturated soil than unsaturated soil? This may explain your results on L390-391.

Response: Thank you very much for this suggestion, we agree that it is a potentially important factor. We unintentionally overlooked discussing heat capacity. As pointed out in the comment, moisture distribution leads to differences in saturation between the up- and downhill sides of the domain, which in turn leads to increased heat capacity in wetter areas and decreased heat capacity in dryer areas. This is now added to Figure 6 in the main text. As a consequence of differences in heat capacity, dryer areas both warm up and cool down faster, because less heat is needed to cause a temperature change (on either side of the zero degree curtain). We also added an additional figure in the supplementary information (Fig. S5) that shows differences in heat capacity in a 2D plot analogous to Figure 4 in the main text. We adjusted the text in several places to incorporate this information.

E.g.: L295-299

17. L326, Vertical diffusion of what? Heat diffusion? Be explicit even though you go on to reference heat.

Response: We added 'heat' for all the different occasions of 'diffusion' in the text.

All occasions of 'heat diffusion' in the text

18. L430-440, Why are these processes more relevant for a high-Arctic hillslope setting if they are for sites with no topography?

Response: We realize that the formulation in this paragraph was not clear. We rewrote parts of the paragraph to clarify our intended meaning. The main message we wish to convey relates to the difference in processes between a discontinuous permafrost landscape versus a continuous permafrost landscape; while heat advection is found to alter the temperature regime in discontinuous permafrost landscapes (Sjöberg et al., 2016, Shojae Ghias et al. ,2019), advection of heat does not play a significant role in continuous permafrost landscapes with a similar topography and small hydraulic gradients (Kurylyk et al., 2016). The difference to our hillslope study is that the distribution of water actually impacts the temperature differences between two locations along the slope (uphill vs. downhill) rather than the advection of heat itself.

L434-446

Technical Comments

19. L2, What is 'its'? Permafrost?

Response: Since the sentence was a bit confusing, we changed it to: *Modeling the physical state of permafrost landscapes is a crucial addition to field observations in order to understand the feedback mechanisms between permafrost and the atmosphere within a warming climate.*

L2

20. L4, Delete 'want to'.

Response: Deleted.

L5

21. L6, Indicate that these are the 'steep' and 'medium' cases.

Response: Adjusted.

L7

22. L50-54, These sentences seem out of place and are too short to form a paragraph. Add more studies, incorporate them into the previous paragraph, or remove them.

Response: We have merged the paragraph to the end of the previous paragraph. This part is intended to highlight processes in the high Arctic, as compared to studies mentioned before that mainly focus on sub-Arctic systems.

L50-54

23. L55, What is the length of the hillslopes? I imagine hillslope length will alter results.

Response: Slope/transect length is mentioned later on in model design but added now as well to the end of the introduction part. Slope length is also an important factor to consider, which is addressed in the outlook section (section 3.7)

L120-122 and L457-459

24. L55, Add 'two-dimensional'.

Response: Added.

L62

25. L54-73, Condense into one paragraph, move study site details to study site section.

Response: Condensed paragraphs into one paragraph and moved information about the location of Svalbard and respective active layer deepening to the study site section.

L107-110

26. L68-69, Delete, repetitive.

Response: Deleted.

L67-68

27. L71, To 'what' extent.

Response: Adjusted.

L70

28. L71, I think replacing 'inclination' with 'slope' would make this easier to understand as it uses the more common term.

Response: Thank you for your suggestion. However, we played around with different word combinations and we think that hillslope inclination should be clear enough for readers to understand that the different inclinations (11 and 22 degrees) refer to the 'slope' of the hillslope.

No changes

29. L72, Typo, 'to'.

Response: Corrected.

L70

30. L89 and L93, Citation typo.

Response: Corrected.

L91 and L95

31. L112, This is the mean snow and rain for 2013-2019, correct? It's unclear as written.

Response: We clarified the sentence: *The resulting average sum of rain (160mm) and snow (170mm water equivalent, total precipitation=330mm) for the period 2013-2019 was then redistributed to equal daily amounts during the rain- and snow period, respectively.*

L117-119

32. L118-119, Change to 'slope'.

Response: Changed.

L124

33. L149, What does 'field values' mean here?

Response:We rephrased the sentence to: *The model output is given as cell values in selected cells of the model domain.*

L178

34. L162, Typo ‘initialization’.

Response: Corrected.

L192

35. L186, Delete ‘significant’ are these trends statically significant?

Response: Removed.

L217

36. L187-188, Highlight these times (thaw and freeze up) in Figure 3 with shading or otherwise.

Response: Added shading to Figure 3.

Figure 3

37. Table 2 and 3, Can move to supplementary material.

Response: Moved to supplement and changed text to refer to tables.

L231-232

38. L199-203, Redundant, can be removed.

Response: Removed.

removed, see comment 37 above

39. L205-206, I don’t think you can draw this conclusion from the presented data, save this for the discussion.

Response: Agreed. The text has been reworked and moved in the revised manuscript.

Text has been removed

40. L207, What do you mean by ‘inversion of temperature differences’? Please reword.

Response: Sentence has been removed with paragraph (in response to comment 39 above).

Text has been removed

41. L207, Again, I don’t think this necessarily indicates this conclusion, remove.

Response: Agreed, removed

Text has been removed

42. Figure 5, Delete panel (b), panel (a) is clearer and shows a very similar result.

Response: Thank you for your suggestion. After revisiting the figure and the corresponding text, we agree that the Figure 5b does not add anything new to the results. We therefore removed that panel and modified/simplified and partly removed the corresponding text accordingly.

Figure 5

43. L279, I'm not sure what lateral gravitational water flow means exactly since water flows vertically due to gravitational attraction.

Response: Removed 'lateral'.

L284

44. Throughout, Refer to as 'heat' diffusion.

Response: Added 'heat' to the occurrences of diffusion.

Added 'heat' to all occurrences of 'heat diffusion'

45. L337, This is an important point to make.

Response: Agreed.

L352-355

46. Section 3.7, Add a more descriptive header.

Response: Changed to 'Outlook'.

Section 3.7

47. L435-436, Also see McKenzie and Voss, 2013.

Response: Added McKenzie and Voss, 2013 for literature comparison.

L436

References cited in this letter:

Frampton, A., Painter, S., Lyon, S.W., Destouni, G., 2011. Non-isothermal, three-phase simulations of near-surface flows in a model permafrost system under seasonal variability and climate change. Journal of Hydrology 403, 352–359. https://doi.org/10.1016/j.jhydrol.2011.04.010

Frampton, A., Painter, S.L., Destouni, G., 2013. Permafrost degradation and subsurface-flow changes caused by surface warming trends. Hydrogeology Journal 21, 271–280. https://doi.org/10.1007/s10040-012-0938-z

Jan, A. and Painter, S. L.: Permafrost thermal conditions are sensitive to shifts in snow timing, Environmental Research Letters, 15, 084 026, https://doi.org/10.1088/1748-9326/ab8ec4, https://iopscience.iop.org/article/10.1088/1748-9326/ab8ec4, 2020.

Jafarov, E. E., Coon, E. T., Harp, D. R., Wilson, C. J., Painter, S. L., Atchley, A. L., and Romanovsky, V. E.: Modeling the role of preferential snow accumulation in through talik development and hillslope groundwater flow in a transitional permafrost landscape, Environmental Research Letters, 13, 105 006, https://doi.org/10.1088/1748-9326/aadd30, http://stacks.iop.org/1748-9326/13/i=10/a=105006?key=crossref.ea8d38a9a41cbb120144acdd5d1d4d37, 2018.

Kurylyk, B. L., Hayashi, M., Quinton, W. L., McKenzie, J. M., and Voss, C. I.: Influence of vertical and lateral heat transfer on permafrost thaw, peatland landscape transition, and groundwater flow: Permafrost thaw, landscape change and groundwater flow, Water Resources Research, 52, 1286–1305, https://doi.org/10.1002/2015WR018057, http://doi.wiley.com/10.1002/2015WR018057, 2016

Schuh, C., Frampton, A., and Christiansen, H. H.: Soil moisture redistribution and its effect on inter-annual active layer temperature and thickness variations in a dry loess terrace in Adventdalen, Svalbard, The Cryosphere, 11, 635–651, https://doi.org/10.5194/tc-11-635-2017, https://www.the-cryosphere.net/11/635/2017/, 2017.

Shojae Ghias, M., Therrien, R., Molson, J., and Lemieux, J.-M.: Numerical simulations of shallow groundwater flow and heat transport in continuous permafrost setting under impact of climate warming, Canadian Geotechnical Journal, 56, 436–448, https://doi.org/10.1139/cgj-2017-0182, http://www.nrcresearchpress.com/doi/10.1139/cgj-2017-0182, 2019.

Sjöberg, Y., Coon, E., K. Sannel, A. B., Pannetier, R., Harp, D., Frampton, A., Painter, S. L., and Lyon, S. W.: Thermal effects of groundwater flow through subarctic fens: A case study based on field observations and numerical modeling, Water Resources Research, 52, 1591–1606, https://doi.org/10.1002/2015WR017571, http://doi.wiley.com/10.1002/2015WR017571, 2016.

Strand, S. M., Christiansen, H. H., Johansson, M., Åkerman, J., and Humlum, O.: Active layer thickening and controls on interannual variability in the Nordic Arctic compared to the circum-Arctic, Permafrost and Periglacial Processes, p. ppp.2088, https://doi.org/10.1002/ppp.2088, https://onlinelibrary.wiley.com/doi/10.1002/ppp.2088, 2020.

Woo, M.: Permafrost Hydrology, Springer Berlin Heidelberg, Berlin, Heidelberg, 2012.

Response letter to Referee #2

Our responses are written in blue

(Examples of) line numbers in the **revised manuscript** that contain changes according to the comment are given in red font.

General comments

1. Based on what I read, the main message is because of variable saturation, hillslopes experience different thermal regimes with warmer upslope areas and cooler down slope areas. The paper seems to focus on how increased moisture in the down slope area causes increased evaporative cooling. However, the overall thermal state of the domain is slightly warmer. I'm left wondering why are sloped simulations overall warmer? Only at the end of the manuscript when the authors do an additional sensitivity study by adding more precipitation do we see a general cooling effect of the entire domain. This then suggests that it is a water balance mechanism caused by slope, i.e the system tips the evapotranspiration into an energy limited system rather than water limited and as more water is added. And that area of the domain with more evaporative cooling outweighs the domain that is water limited. However, the water balance verses degree slope, which appears to control the overall thermal state of the entire domain is not discussed.

Response: Thank you for the valuable comment. We realize that the downhill cooling effect received too much attention and that the overall warming of the slope as compared to the flat case has fallen short. We explain the uphill warming effect by increased infiltration (heat advection), less evaporative cooling and overall lower heat capacity as compared to the flat case. In the text, we briefly mention that vertical heat advection plays a major role in the differences between the up- and downhill section. While the uphill experiences less evaporation and more infiltration, the downhill experiences notably more evaporative cooling . We added a figure with a simple water balance (difference between precipitation and evaporation; P-ET) below (Fig. 1 in this letter and Fig. S6c and d in the supplementary material). It shows that overall evaporation is higher in the downhill side and lower in the uphill side (Fig. 1a,b in this letter), which in turn leads to positive values in the summer water balance (P-ET) in the uphill side (Fig. 1c in this letter), i.e. more rain than evaporation, and negative values in the summer water balance on the downhill side (Fig. 1d in this letter), i.e. more evaporation than rain.

E.g. L374-378 and L490-499

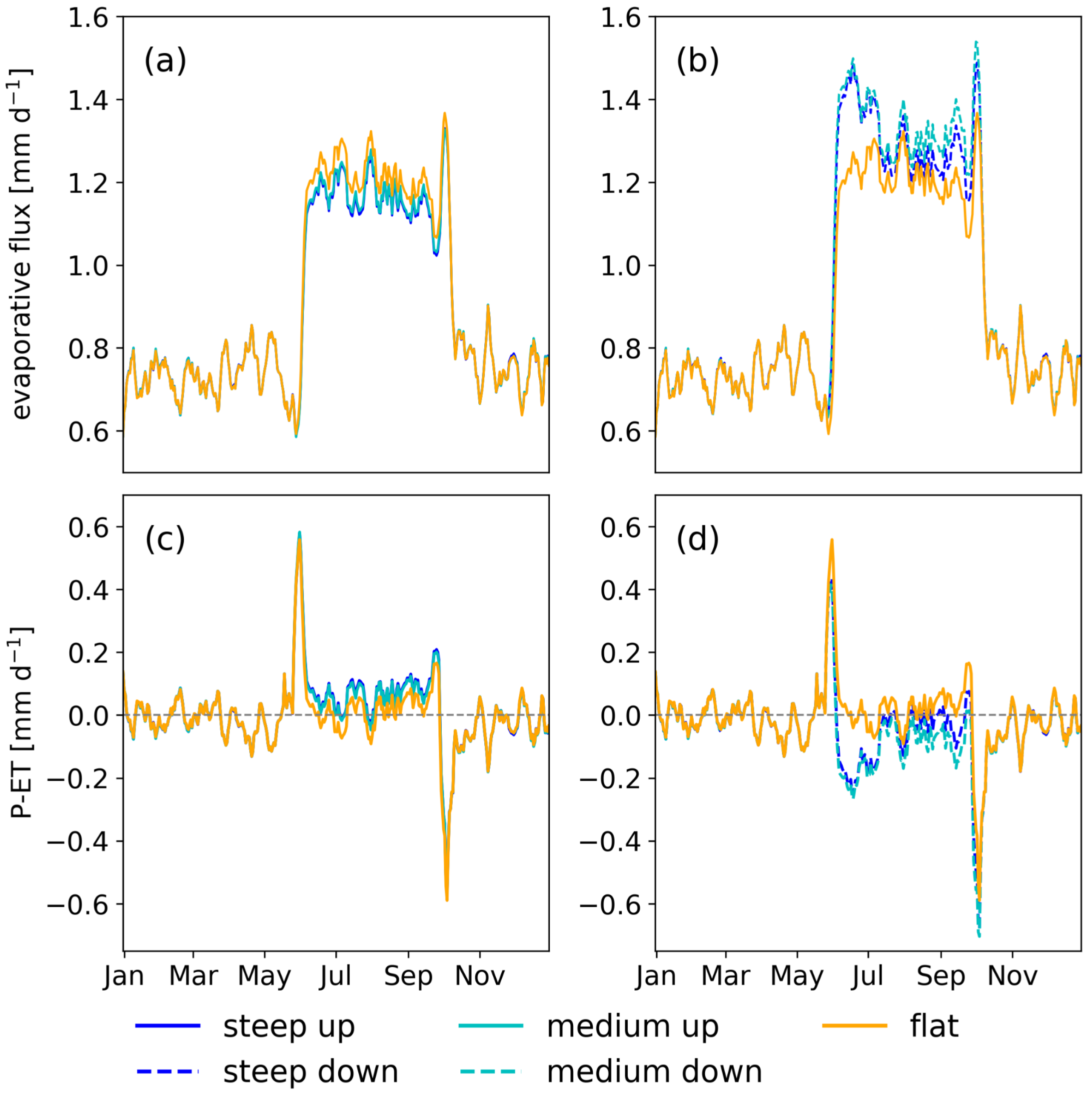

*Figure 1: Evaporative flux (**a** and **b**) and net infiltration (precipitation-evaporation; **c**, **d**) at the surface on the uphill (solid lines; **a** and **c**) location and the downhill (dashed lines; **b** and **d**) location. Daily values are averaged over a 7-day window. Blue, cyan and yellow represent the steep, medium and flat case, respectively.*

2. Similarly – and at a much smaller scale, Abolt et al., (2020) found (using the same ATS model) that rims in polygonal ground warm more in the summer due to drier conditions and associated weakened evaporative cooling, which then provides energy laterally to the cooler saturated troughs in the summer (see section 5.2 of Abolt et al., 2020). However, what determines if a saturated area is cooler or not also depends on the mass of water present. If enough water is present, especially on the surface, then a Talik will begin to form, as demonstrated in a 1D column by Atchley et al., (2016-section 4.2) and by Abolt et al., (2020) for wide troughs. This is because the timing of phase change during freeze up when snow is building can cause wet areas to stay warm throughout the winter, especially as the amount of water increases because it then requires a lot more energy loss to cross the freeze curtain. The difference with the study presented here is: 1) vary little surface inundation occurs due the surface runoff boundary condition and the assumed energy equilibrium at the downhill domain boundary condition. This assumption may not capture the thermal influence of the saturated

condition beyond the boundary of the domain. And 2) very little snow accumulation occurs, which would otherwise insulate the more saturated areas during freeze up. Given that the simulations with added precipitation showed an overall decrease in ALT, this work might suggest that increased evaporative cooling affect may outweigh the increased energy loss necessary to cross the freeze curtain. However, given the larger thermal hydrology work in literature, it would be beneficial discuss these tradeoffs as well as discuss how influential an appreciable snowpack may change the results. i.e there could be a combined warming in the dry areas (little evaporative cooling) and persistent warm winter conditions in wet areas from insulative snowpacks.

Abolt, C.J., Young, M.H., Atchley, A.L., Harp, D.R. and Coon, E.T., 2020. Feedbacks between surface deformation and permafrost degradation in ice wedge polygons, Arctic Coastal Plain, Alaska. Journal of Geophysical Research: Earth Surface, 125(3), p.e2019JF005349.

Atchley, A.L., Coon, E.T., Painter, S.L., Harp, D.R. and Wilson, C.J., 2016. Influences and interactions of inundation, peat, and snow on active layer thickness. Geophysical Research Letters, 43(10), pp.5116-5123.

Response: Thank you for this comment. As you mention, the formation of a potential talik is not so relevant for this study site since water does not start pooling above ground, even in the cases with twice the amount of precipitation, and snow cover is overall very shallow, due to the very dry climate in Adventdalen (the hydro-meteorological forcing conditions are taken from site data). However, in general ponding could occur, and surface water formation would in turn likely trigger talik formation. We have attempted to discuss various effects mentioned in your comment in the Outlook section (section 3.7) of the revised manuscript (previously "Further implications"), where we have adjusted the text and incorporated citations to Abolt et al. 2020 and Atchley et al. 2016 in this part.

L465-475

Minor comments:

1. L58-59: In this case the benefit of numerical modeling probably has less to do with the remoteness of the study area, and more to do with a characterized sensitivity study as well as being able to dissect the energy fluxes across the full domain, something that would take tons of sensors to do in the field.

Response: Thank you for this comment. Indeed, we agree and have adjusted the text accordingly.

L57-60

2. L72: Change 'Tho' to 'To'

Response: Corrected.

L70

3. L89: (Painter, 2011) is outside of either sentence, I think it goes with the previous one.

Response: Corrected.

L91 and L95

4. L118: Omit one of the 'a steep case'.

Response: Corrected.

L124

5. L147-148: Why would you want to maintain a constant (same) snow accumulation across the hillslope domain, and is that realistic? This is likely to have a strong effect of simulations results.

Response: In this study we wish to investigate the effects of slope inclination on the hydrothermal conditions of the active layer. To enable clear and consistent comparisons we keep the boundary conditions and the hydrothermal forcing identical between the different cases, and enabling snow redistribution would potentially obfuscate comparisons. Also, since the site conditions are dry with very little snow precipitation, the effect of snow insulation and melt are minimal. However, adding snow redistribution would certainly be interesting for sites with more snowfall, as the lower end of the slope could then represent a snow pit/trap, and under those conditions much of the snow could end up on the lowermost part, and would then likely influence subsurface conditions locally. We provide additional clarification and justification of our model design in section. 2.2 “Simulation configurations”.

L173-177

6. L153-161: Note that these CV locations are right at the domain boundary, and therefore subject to any edge effects of the model domain. The assumptions and implications of being on these domain boundaries needs to be discussed in more detail. This might be especially problematic with the downhill CV, given that the no flow (energy) boundary condition assumes equilibrium with a larger body of water or saturated area.

Response: Thank you for the comment. We address this question in more detail in our response to Reviewer 1 (please see General comment 1 of Reviewer 1). This has led to a slightly modified model domain, which more clearly represents the valley base and allows for energy and water fluxes out of the slope. The location of the CVs on the outermost boundaries of the domain was intentionally chosen to capture the most extreme values (drying on uphill side, and accumulation on downhill side). Now with the revised model domain, the downhill side CV is not on the right-most boundary anymore. Aslo, potential boundary effects are evaluated (see response to General Comment 1 of Reviewer 1). The intention of the placement of the CVs has been clarified in the main text, in the Section 2.2.

E.g. L144-147 and L160-166 and General comment 1 of reviewer #1

7. L207-208: This sentence needs to be more specific. What do you mean by inversion of temperature differences? Differences between uphill and downhill observations? Or differences between sloped and flat? Also, what are the different processes responsible here?

Response: We restructured the text and removed the paragraph that contains this unclear formulation.

Text has been removed

8. Figure 3 needs more explanation in the caption. Hard to interpret it at a glance.

Response: We have made attempts to clarify this figure and caption text; we extended the caption text with an explanation indicating what positive and negative values mean in each of the panels. Further, we added the original time series from which the differences are derived, and placed this in the supplementary information (Fig. S3).

Figure 3

9. L215: “The upper three panels in each figure…”. I assume you are talking about Figure 4 here, but it is not clear. I suggest phrasing this as, “The upper three plots in each panel…” as ‘figure’ refers to figure 1 through 9 in the paper, ‘panel can refer to a and b, and plot is the sub plot of each panel.

Response: Thank you for the suggestion. We adjusted text according to the comment.

L238-240

10. Figure 7: I like this figure, or at least what it is attempting to convey. However, I think it could be improved, or perhaps simplified. Is the story in the time series of the flux? Is it necessary to show the whole time series? If not, I would suggest just showing the cumulative flux, or maybe the difference of the fluxes between the cases. That may help with the scale issue. Additionally, the location of the representative volumes in relation to the fluxes going in and out is confusing, i.e, the lateral energy flux going in the downhill volume (positioned right in the domain) is found left in the figure. This means the reader (me) has to mentally flip the image.

Response: Thank you for the comment on this admittedly complex figure. We tried to simplify the figure according to your very useful feedback. Below you can find the figure with the cumulative sum (Fig. 2 in this letter), however this unfortunately leads to a significant loss of information in terms of direction of fluxes (specially for the vertical fluxes). The new final figure includes adjustments to the moving average (90 day window) and placing of the uphill and downhill panels as well as an adjustment to the titles (Fig. 3 in this letter and Figs. 7 and 8 in the main text). We aimed for the best compromise between correctness and simplicity. This should improve readability.

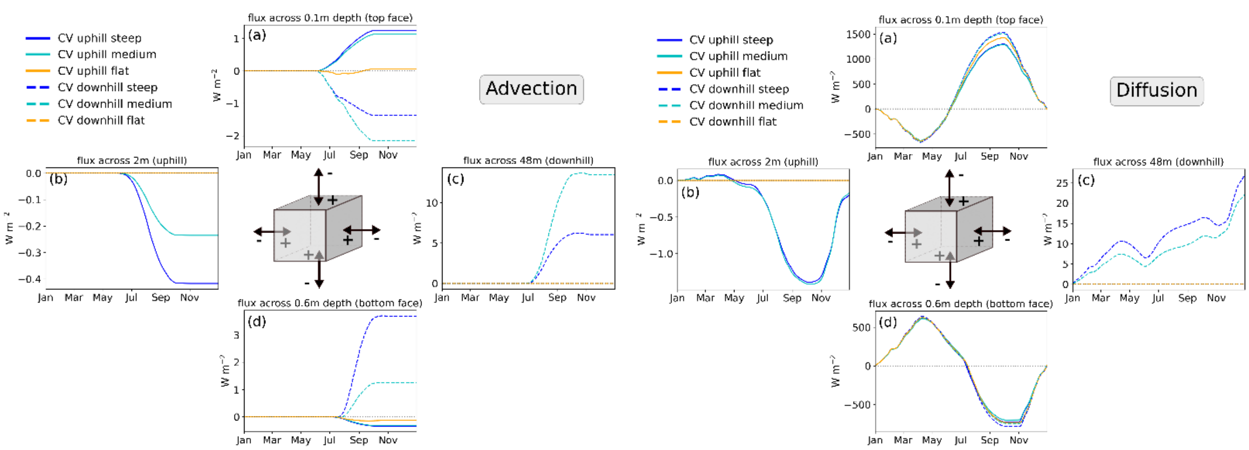

Figure 2: Advection and diffusion of heat through the control volumes (cumulative sum) (not used)

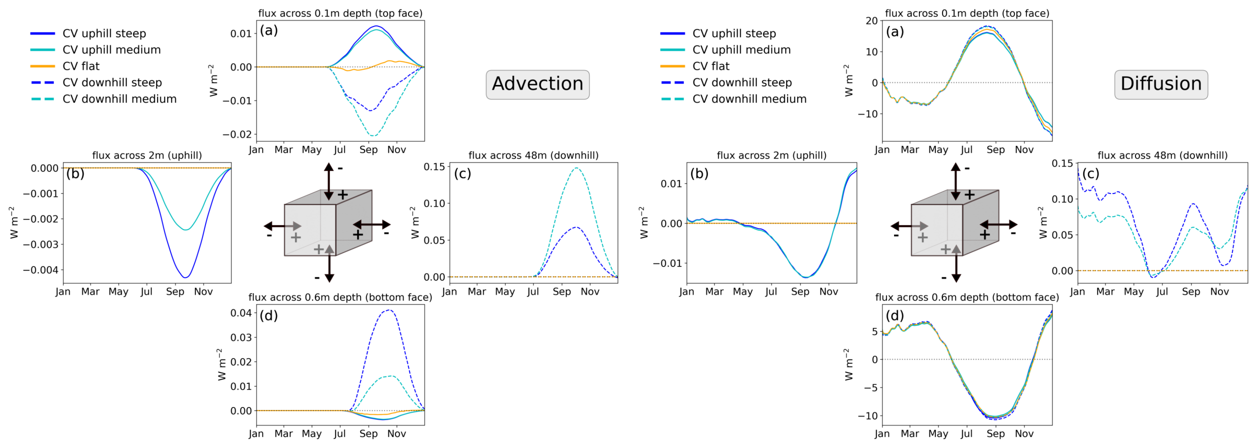

Figure 3: Advection and diffusion of heat through the control volumes (90 day moving average) (used in revised manuscript as Figures 7 and 8)

Figure 7

11. Figure 8: These are pretty small fluxes. Is this figure necessary? I think this paper as a whole makes a decent argument that groundwater dynamics are important in determining the thermal state of the hillslope, even if advective fluxes only play a small role. In other words, the influence of groundwater dynamics happens in other processes. I would suggest focusing as much as possible on those processes rather than advection.

Response: Thank you for this remark. Even though the values are fairly small, especially vertical advection plays a big role in the differences between up- and downhill (namely evaporation and infiltration). As described in the text, the uphill section is mostly dominated by infiltration rather than evaporation, which on the other hand dominates in the downhill section.

No changes

12. L462: “as compared to the flat case (0.75cm).” is confusing. Does the flat case change in the simulations? I thought the flat case provided a reference datum and therefore should be 0.

Response: Thank you for catching this typo. This sentence was supposed to refer to the active layer depth of the flat case in absolute values and should say 0.75m instead of cm. So the baseline active layer depth was 75cm and was increased in the steep case by 5cm (as stated in the sentence) to 80cm. Note that the active layer depth has changed to max. 1.03m in our new simulations, mainly because we have corrected the thermal conductivity values (please see our response to Comment 10 by Reviewer 1). Thus, the values have been updated in the revised text. For completeness, we have added the maximum active layer depth for the medium case as well, so all three cases have their respective absolute maximum active layer depth.

L481 and L486-489

13. Figure 9: “The sign convention adopted is positive values represent heat entering the CV and negative values leaving the CV.” This would mean that I should see the evaporative cooling affect? Correct? Heat and mass leaving the downslope CV during summer?

Response: Your assumption is correct. However, we are only showing the lateral mass flux here to explain the relationship between advective heat transfer and mass flux. To see mass fluxes in vertical direction, please refer to Figure S7 (in the supplementary information), which includes mass fluxes in vertical direction and shows the influence of evaporation and infiltration.

No changes

14. Section 3.6: This section tests the effect of overall precipitation and demonstrates the overall effect of increased evaporative cooling – at least I think that is the purpose. However, as written it seems to play only a minor role in the manuscript (the text devoted to this seems like an after thought), and there is no figure associated with this text that actually illustrates the point other than those in the supplementary information. It would be better to have a figure in this text.

Response: Thank you for these suggestions. We revisited the section and added a thaw-depth-plot with both scenarios (S0R0 and S2R2) analogous to Figure 5a in the main text (panel b of Fig. 5 in the main text has been removed). The new plot (see Fig. 4 in this letter) is now part of the main text (Fig. 10 in the main text) and is removed from the supplementary material. We have also improved and simplified the text around the sensitivity study.

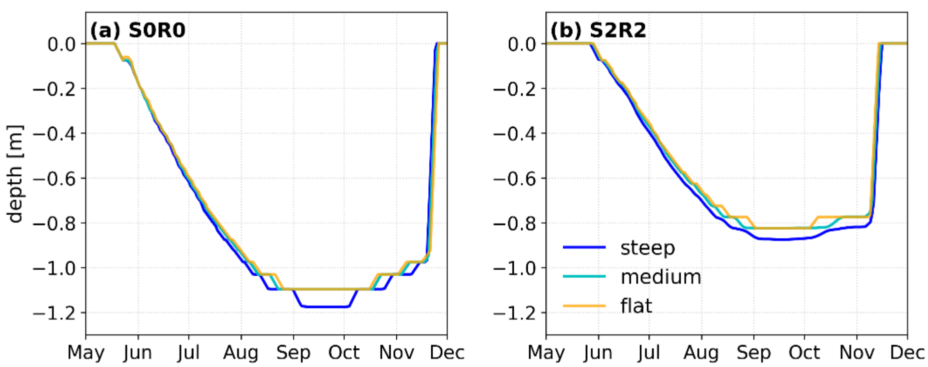

Figure 4: Spatial mean active layer depth for scenarios S0R0 (a) and S2R2 (b). The thaw depth indicates the transect-average thaw depth of each case (steep, medium, flat)

Section 3.6

15. L424-L432: This paragraph provides a good summery of what was found. It seems like it would fit better in the conclusion section.

Response: Thank you for the suggestion. We agree and have moved this part of the discussion to the conclusions section.

L477-480

16. L458-46: "This downhill cooling effect is up to about 1.2 C for steep (22°) and 0.6 C for medium (11°) inclinations across a lateral distance of 50m representative for valleys in Adventdalen, Svalbard." Seems to me that the bigger story here is not the increased downhill cooling caused by the evaporative flux, but the

increased uphill warming presumably caused by the lack of an evaporative flux because overall the entire domain is warmer than the flat case.

Response: We agree that the focus of the study was too much shifted towards the downslope cooling effect while the uphill warming has fallen a bit too short. We revised the manuscript accordingly to put the uphill warming more into the focus and highlight that the warming actually causes the active layer to be deeper. Furthermore, the downhill cooling effect is moderated with the revised model domain.

Several occurrences in the text, e.g., L490-499

17. The conclusion section is not very impactful. The paragraph in L424-432 seems to be a much better conclusion paragraph. Also, after reading the paper several times, I am confused as to why the entire sloped domains are over all warmer than flat domains? The focus appears to be more on the evaporative cooling effect, yet the domains are overall warmer unless precipitation is increased.

Response: Thank you for the tip. As mentioned above, we have merged the paragraph from L424-432 to the conclusions section. Indeed, the summer warming of the sloped cases compared to the flat case is mainly attributed to additional heat through infiltration and overall lower moisture content causing a lower heat capacity and reduced evaporation. We now also address heat capacity and more clearly emphasize the overall warming effect on the uphill section of the domain (see general comment 1 above).

Section 4