

Response to reviewers

We highly appreciate the constructive feedback from both reviewers. As a result of valuable suggestions, many changes have been done. Below are the exact comments from the reviewers along with our response, where our response is indicated by green colour.

Reviewer 1

General comments

The manuscript presents a series of sensitivity experiments conducted with a kilometre-scale numerical weather prediction model with the aim of disentangling the effects of glacier loss on local climate. Changes in surface cover type and elevation associated with complete glacier loss are investigated as are combined effects for a more realistic glacier loss scenario. The effect of glacier lakes under a complete glacier loss scenario are also presented. The results indicate that the topographic changes make the larger difference to local and regional climate than surface type changes. This is through direct adiabatic effects of surface lowering as well as indirect effects on regional moisture flux and orographic processes. The introduction and study objectives are well set out. The model and simulations are adequately described. Appropriate model evaluation is made, though additional figures and tables could be included in supplementary material to aid closer interrogation of the results and increase the robustness of the study. The length and spatial resolution of the simulations provide a robust basis for interpretation and attempts are made to further understand how the effects vary with wind regime. The figures are of a good quality and quantity. The conclusions are supported by the results, though the discussion of the implications for future glacier climate interactions is, necessarily, qualitative.

There are some aspects of the methodology that could have implications for the results shown that need attention -namely whether the topography was additionally smoothed in some simulations and whether the NOAH land surface model adequately represents the energy and mass balances of glacier ice and snow surfaces.

A major limitation of the study is that it does not consider the effect of changing snow cover during winter, spring and summer that would accompany glacier retreat (only the extent of exposed glacier ice surfaces). Because of this, full effect of changing surface type on the local and regional climate has not been assessed. This should be discussed further in Section 4.

Overall, the study provides some useful results and with revisions to the text and additional details should make a good contribution to the literature surrounding glacier-climate interactions. There are more opportunities for analyses with the datasets, but perhaps these could come in a later manuscript that could also include explicit pseudo-

global warming simulations to quantitatively compare the competing effects of changing glacier ice cover, topography and regional climate.

We thank the reviewer for providing valuable and thorough feedback that has improved the manuscript in several ways.

Line comments

13 – there are earlier studies looking at the effect of including glaciers in RCM simulations which is essentially the opposite comparison to what is done here. E.g.

<https://link.springer.com/article/10.1007/s00382-009-0685-6>

This is a relevant paper that we were not aware of. We have added it as a reference to the statement about changes in albedo associated with glaciers in lines 13-14.

58 “is therefore representative for many other glacierised areas of the world.” This is perhaps overstated as the overarching climate (maritime, continental, polar, temperate, tropical) will have a large bearing on what aspects of glacier-topography interactions are important. This is mentioned at the end of the conclusions, but this statement should be tempered here and discussed further section 4.

We agree that this sentence should be weakened and added “with a temperate climate” in line 58. With this modification, the sentence now points mostly to the ice cap representing different glacier types (e.g., valley glaciers vs. plateau glaciers) rather than different climate zones. The impact of glacier characteristics (in valleys vs. at high elevations) is already discussed in Section 4.3 before discussing potential impacts from glaciers in other climate systems.

67: “Higher model resolution will likely also result in limited additional scientific insight” agreed but this doesn't negate the fact that the finest scales aren't well represented and that simulations that include these scales may yield different results from the interactions that occur. Please revise.

This is a fair point. To acknowledge the value of increasing model resolution, we have added “While higher model resolution can improve the representation of finer scales and their interactions with larger scales” to the sentence in lines 67-68.

85: “mild and wet winters and cool summers” some regionally average values here would be insightful to those not as familiar with the region (e.g., seasonal temperature range, total annual precipitation).

This is a good suggestion, but also a challenging task as regional variations are huge due to complex topography. We haven't found any good sources for meaningful regional averages and decided to instead indicate numbers from some relevant weather stations in the region in lines 87-89.

101: It is worth noting here that all the simulations are performed with the same boundary conditions i.e., there is no global warming induced regional changes in temperature or precipitation.

We agree with the reviewer and included "In all simulations" in line 106 to avoid potential confusion.

113: Please clarify if small glaciers appear in the default runs as well or just the Future glacier outlines run?

This is a good point. Small glaciers are only present in the control simulation. We added "only included in the control experiment" in line 120 to clarify this.

119: Did you consider running a 2100-future with lakes? Or will the lakes only form with full glacier retreat?

There will be some smaller lakes appearing with the 2100 glacier extent, but most of the potential future lakes are located at upper parts that will not be present in the 2100 scenario. With the limited impact by the lakes and the challenges representing them well, as well as the high computational cost running these experiments, we therefore decided to not run such an experiment.

123: Some more detail on the land surface model is appropriate here (e.g. what is the albedo of glacier ice in the NOAH scheme? How is snow handled?)

This is a good suggestion. We provided some more information in lines 145-148.

123: The Noah (and Noah-MP) model has known issues related to cold biases and poor representation of glaciers, snowpack and frozen ground. how might this impact the results of the study? Some further discussion is warranted – some papers of use may be:

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2010JD015139>

<https://www.sciencedirect.com/science/article/pii/S0165232X24000302>

<https://agupubs.onlinelibrary.wiley.com/doi/epdf/10.1029/2019JD030823>

<https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1029/2025JD044230>

<https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1029/2025JD044191>

We agree with the reviewer that the impact of these issues should be better addressed and added a brief description of these limitations in lines 145-146, including some of these references. The issue is further addressed in lines 278-284.

134: A table with station name, location, elevation, surface type, variables measured, measurement dates, % data gaps would be very useful to the reader.

A table with most of the requested information is included as Table 2. Within the operating period specified in this table, there are nearly no data gaps at most stations. We therefore decided to mention the few noteworthy data gaps in lines 163-165 instead of in the table.

154: It would be useful to indicate which valley was studied previously and how it relates to the AWS locations used here.

Agree. A note about this has been added in line 179.

161: unclear and long sentence. Is the prevailing wind from the South? and how does this relate to the highest elevation and the SM and SB stations? Do you mean “in the lee of the prevailing southerly winds (as shown at the models highest elevation...)”?

We agree that this sentence is unclear and too long and realised that some details are not needed. Hopefully, the revised sentence in lines 186-188 is better.

166: “Modelled snow over the entire ice cap (not shown)” any reason for not showing this – it would be a useful addition to the paper especially given the focus on surface cover

The main reason is brevity, but we are happy to provide this and have added it as Figure A1 now, with a reference to it in line 192.

170: I don't think you can say that temperature is well represented as biases are significant - 'adequately' perhaps given the terrain.

We agree that this phrasing should be reformulated and revised the sentence to “Modelled temperature is also adequately represented given the complexity in terrain and land use” in line 195.

171: Figures A1 and A3 need to be in the main body of the manuscript and the mean values from Figure A2 in a table.

We agree that this information could be moved to the main body of the manuscript and to a table. The figures are now included as Figures 3 and 4 and Table 3.

176: The best performance at the glacier station seems to be in winter with summer not performing as well. please revise

That is correct and not clear from our previous statement using the word “partly”. We have now expanded the sentence to explicitly state “while the temperature of the glacier station is best represented in winter” in lines 204-205.

Figure 2: the extent of the ice surface is confusing between this figure and the next two (which use dots for ice loss between simulations). Consider using a different symbol in this figure (e.g., an x or cross) or a different symbol in Figures 3 and 4

We acknowledge the issue and have changed the black dots to blue lines.

183: “wind speed is too high” A table or figure showing histograms of wind speed for each site would be very useful for model evaluation. The wind roses are too small to compare directly. Overestimation of wind speed by NWP is well known, but it is useful to quantify, especially given the paper concerns the interaction of wind systems with surface type and topography.

We agree that it is valuable to quantify the model overestimation of wind speed and decided to add numbers for mean bias and absolute errors in the updated Figure 3. We refer to these errors in lines 211-212.

191: Presumably ‘surface air temperature’ is meant here (as opposed to surface skin temperature that is also commonly called surface temperature). Please add ‘air’

We agree that surface temperature is not correct and changed it to 2 m air temperature in line 223.

Figure 3/4: The changes in temperature (and in some areas precipitation) outside of the glacier domains in the 2100-volume and no-ice-volume simulations indicate very similar patterns. Are these related to elevation changes in the model? e.g., were these surfaces smoothed further than the no-ice-surface and control simulations? If so, what are the implications for the results and other comparison? If not, what could be the mechanism for such a result?

This is an important point. The smoothing option in WRF is identical in all experiments. However, we realized that there is a difference in the terrain outside the ice cap when comparing the control experiment to the experiments with reduced elevation over the ice. The input DEM files before conversion to WRF binary format are identical away from the ice cap, but the preprocessing of static fields has induced unexpected changes in the terrain. We do not believe it is related to the smoothing procedure as it should in principle only affect grid cells close to the ice cap. We have investigated other reasons for the shift without any success. Unfortunately, we do not have resources to rerun the experiments.

Despite this, we believe the results are still valid, as elevation changes nearly cancel on average. At the bottom of this letter, changes in elevation are shown along with changes in temperature and precipitation (the latter taken from Figures 5c and 6c) for the no-ice-volume experiment relative to the control experiment. These figures show that most changes in temperature outside the ice cap are likely associated with the local elevation changes. This is only partly true for precipitation, as larger areas in the east and south are exposed to only positive and only negative changes, respectively. The different response for precipitation suggests that the smaller differences in elevation outside the ice cap are less important for the spatial distribution of precipitation than the large elevation changes at the ice cap associated with glacier recession. We therefore argue that our main findings are important and robust, though evaluation of results outside the ice cap must be done carefully and only on larger spatial scales not focusing on individual valleys, etc.

We have described the issue in lines 132-139 in the methods and have adjusted statements about changes outside the ice cap in lines 238-240, 244 and 249-252.

While looking at this, we realized that we also need to add some more description of a few other minor preprocessing stages of the DEMs. This is done in lines 127-132. This preprocessing over/near the ice cap has not affected the terrain shift outside the ice cap.

Figure 3/4: The blue frame is exceptionally hard to see in the figures. It also makes the squares appear purple and thus not aligned with the colorbar scale in Figure 3. Consider zooming this panel in and/or using a similar symbol to the other panels to denote surface type change.

We have struggled to visualise the lake grid cells in a satisfactory way. Based on both reviewers' suggestions, we decided to use black frames instead of blue frames. We want to avoid using dots to highlight that the changed surface type is not the same as in the other subfigures (i.e., from ice/snow to rock). Other symbols hide too much of the colors behind. We have considered zooming in to this panel, but argue that it is best to keep the scale the same in all subfigures, as these frames will anyways be easier to see when these figures (now Figures 5/6) are scaled up in full width if the paper gets accepted for publication (which we are not allowed to do in this version of the manuscript).

Figure 3/4: A distance scale on the figure would be useful when distances are referred to in the text

This is a good suggestion. We have now implemented this in the first subpanel of these two figures (now Figures 5/6). We also implemented it in Figure 2.

197: "likely mainly related to the albedo feedback" do you mean surface heating of bare surfaces not covered in snow? Please expand a little here.

Yes, we have clarified this in line 230.

212: “more moisture available for precipitation further inland” it would strengthen your argument if you could show this by comparing integrated moisture flux metrics for each simulation?

Due to limited capacity for storage, we unfortunately only have vertical data saved at four pressure levels, which we consider insufficient to estimate the vertically integrated moisture flux. Instead, we added a statement and reference about the role of orographic precipitation when describing the climate in the study area in lines 90-91.

214: “The local pattern of changes in precipitation on the western side is similar to the changes in temperature...” as per first comment for figure 3/4 - are these related to changes in smoothed topography between the runs?

We have spotted a change in the topography which is explained in more detail in the response to the comment about smoothed topography above. This sentence has therefore been modified.

224: consider adding "TB' to figure 3a and 4a so the reader does not have to refer to figure 1.

We have considered it but fear that there is already too much information in these figures and decided to leave it as it is not a very important statement.

228: it is worth highlighting here that the simulations only deal with the topographic/surface cover changes and you haven't done any psuedo-global warming simulations with different boundary conditions to assess the combined effect with regional climate changes.

We agree that repeating this important information here is valuable and included this in lines 263-265.

230: “same emission scenario” do you mean the same emission scenario as the projected glacier outlines? please clarify.

Yes. We have clarified this in line 267 now.

234: “feedback effects” where are these discussed? Please expand

We agree and have now expanded this statement in lines 271-275.

236: “that impacts by changes in land use are more than one order of magnitude smaller than those related to changes in elevation”. Agreed but this only takes into account changes in glacier ice and not the seasonal snowpack changes that lead to the loss in glacier ice. Changes to snowpack will have a large impact on winter and spring land

surface-atmosphere feedbacks. This may also extend to summer in the higher elevation areas that currently generate a deep snowpack. Please revise.

These points are fair and are now added to the text in lines 278-281.

239: “inclusion of new lake surfaces can still have a larger impact than ice surface removal through their impact on moisture fluxes”. This interpretation would come better after the results are shown. You also later discuss how the change in both temperature and precipitation is limited so this sentence seems a bit contradictory. Please revise.

We agree and have revised this in lines 285-287.

254: “coupling would also strengthen the robustness of the findings”. Agreed this is one of the major limitations of the study that needs more discussion. Perhaps posit some hypotheses as to mechanisms that may change the direction or magnitude of the interactions for future studies to assess.

We agree and have added some indications of how impacts on climate from glacier recession may feed back on glacier evolution in lines 271-275.

269-271: Confusing - expressing the changes as a fraction of the mean precipitation in given wind directions could help here. Or stating the mean values in the text, as they are hard to interpret from the figure.

To avoid this confusion and because these numbers are very small and unimportant, we decided to remove the last confusing part of this sentence in lines 318-319 and instead update Figure 7 (previously Figure 5) including the interdecile range, which highlights the smaller spread in precipitation during northerlies and easterlies compared to southerlies and westerlies.

270-271: are the mean changes for all wind directions statistically significant given the large scatter and small mean values of precip?

Yes, the mean changes are statistically different from zero despite the small mean values and the large spread, with p-values below 10^{-6} . We have included a brief statement about this in lines 314-315.

279: these results are for the no-ice-volume experiment, so are they relevant to a smaller ice cap? Please revise

Yes, we have checked that the same conclusion holds for the 2100-volume experiment and have clarified this in lines 326-328 along with a reference to the new Figures A2 and A3.

281: “other sensitivity experiments” results for these need to be shown - perhaps in appendix or supporting material.

Yes, we agree and added the figures in the appendix (Figures A2 and A3) along with some more details on minor differences in these other sensitivity experiments in lines 333-341.

291: this sentence is a bit confusing - you say RCMS indicate precipitation increase in the previous sentence, but that RCM show negative trends in future snowfall here? Presumably this is due to temperature increase. If so, please note this is due to temperature or revise otherwise.

Yes, it’s due to increased temperature. This is now clarified in lines 349-351.

295: “Improvements in climate projections due to inclusion of glacier recession” will the glacier surface height changes be resolved in the RCM? Please discuss.

Yes, we argue that the horizontal extent of these elevation changes is large enough to be resolved in RCMs. We added a sentence about this in lines 356-358.

305: Would be good to discuss Salerno et al. (2023) here too.

Good point. We added some comparison to this study in lines 368-370.

Figure A1: needs station labels or a legend for easy reference

Station labels are included in the title of the updated figure (now Figure 3), where only the six wind roses relevant for comparison between observations and model are included.

Figure A1: Side by side plots for the 6 AWS locations would be more helpful for model validation. Or histograms of wind speed.

Yes, we agree, and have changed the figure accordingly (see the new Figure 3).

Figure A2: what timescale is the MAE calculated over – from the figure it looks like it could be MAE of monthly means but perhaps it is MAE of daily or hourly means?

It is MAE of monthly means, which we argue is good for such a study focusing on regional climate and long term trends. We have specified this in the caption of Table 3 (which has replaced Figure A2).

Figure A2: Figures A1-A3 should be in the main body of the manuscript. That being said, figure A2 hints at clear seasonal pattern that is better shown in Figure A3, so Figure A2 could be excluded and table with the bias and MAE at monthly and hourly timescales included.

We agree and have moved Figures A1 and A3 to the main body of the manuscript (as Figures 3 and 4). We also excluded Figure A2 and moved the absolute errors in this figure to Table 3 instead, including now also the bias errors.

Figure A3: is this mean bias error? or mean absolute error?

It is mean bias error. We have specified this in the ylabel and caption of Figure 4 (updated figure number).

Editorial comments

52: “models are a potent”

Fixed.

88: here and throughout - please avoid using parentheses to denote alternative statements. write out in full i.e. "estimate an 2.8 °C increase in mean annual temperature and 10 % increase in precipitation when comparing 1991-2020 to 2071-2100 under a high emission scenario (SSP3-7.0) (Dyrrdal et al., 2025)"" or “estimate increases in mean annual temperature and precipitation of 2.8 °C and 10 %, respectively, when comparing 1991-2020 to 2071-2100 under a high emission scenario (SSP3-7.0) (Dyrrdal et al., 2025)”

This is a choice of style that we are fine with changing throughout the manuscript. The relevant lines are now updated.

101: as per comment for line 88

Fixed.

163: “Oldedalen is also associated”

Fixed.

Figure 2 caption: as per comment for line 88: “MG and JD are from 2021 and 2016-2019, respectively.”

Fixed.

179: sentence is too long - please split

Fixed.

Figure 3 caption: as per comment for line 88.

Fixed.

195: refer to the run name “When the ice surface is removed, but the elevation is unchanged (no-ice-surface run), changes in...”

Fixed.

200: refer to the run name “...corresponding surface-lowering (no-ice-volume run), changes...)

Fixed.

206: confusing sentence - do you mean “Along with the overall warming in the no-ice-volume run, the annual snow-to-rain ratio decreases over the ice cap, so that the reduction in snowfall is more than twice as large as the reduction in rainfall over the ice cap” ?

Yes, fixed.

225: “at these locations” which other locations are used to construct the mean values detailed?

These values were not mean values but approximate values for Tunsbergdalsbreen. We agree that the location and these values should be clarified and have adjusted the text accordingly in lines 259-261.

225: as per comment line 88 – “the change in temperature and precipitation is -1K and +9%, respectively. However, the signal...”

Fixed.

259: “...from between the southeast...”

We are unsure what exactly the reviewer wants us to change but decided to clarify the boundaries for the wind direction bins in lines 308-309.

265: as per line 88 comments: “-1.5 and -2.6 mm on days with southerlies and westerlies, respectively

Fixed.

268: as per line 88 comment

Fixed.

286: use 'temperature' in place of 'air' as per line 191

Fixed.

Reviewer 2

This paper investigates how the thinning and disappearance of the Jostedalbreen ice cap in Norway alters the surrounding regional climate. Utilizing the Weather Research and Forecasting (WRF) model, the authors perform several sensitivity studies and find that glacier recession leads to a warmer and drier local environment, mainly due to lower surface elevations and reduced orographic lifting. There are some limitations in this study, for example they only use current climate for forcing. However, the limitations and its implications in this study are discussed and addressed satisfactory. This paper is very well written and conclusion are supported. I have only minor comments and suggestions.

We thank the reviewer for reading through the manuscript and providing valuable feedback that has improved the manuscript.

Lines 155 and 172-174. The authors mentions that the model underestimate the 2 m air temperature at the glacier station (NB). I am wondering if the modeled glacier albedo has some impact. In Noah-MP, the glacier surface grid points are treated as "old snow" if modeled snow has melted. Thus, in grid points that normally should represent bare ice, with a low albedo (~0.2-0.3), Noah-MP uses an albedo of ~0.5. This could potentially lead to colder modeled surface temperatures compared to real conditions when the seasonal accumulated snow has melted.

This is a good point that we have decided to implement in the discussion of the cold bias in lines 181-182 and 199.

Lines 64-65. The word "this" is used three times in these two lines. I suggest rephrasing to "...forced winds were partly resolved by the 1 km model resolution...."

This is a fair point. We modified the sentence accordingly.

Figure 3. I found the use of different colors for the elevation band (contours) taking away from the underlying difference plot. I suggest using only one color for the contours.

We agree that there is a lot of content in this figure and the next one. However, after testing with black contours instead, it became very difficult to evaluate where valleys and elevated areas are located. As this information is important for the interpretation, we decided to keep the elevation contours in colors. We believe that the information is less overwhelming when the figure gets scales up in full width if the paper gets accepted for publication (which we are not allowed to do in this version of the manuscript).

The blue outlined lake grid cells and the red temperature increase make the lakes just look like purple boxes. I wonder if using grey or black outlines would help?

We have struggled to visualise the lake grid cells in a satisfactory way. Based on both reviewers' suggestions, we decided to use black frames instead of blue frames.

Line 197. Please define the albedo feedback

We agree and modified line 230 accordingly.

Line 248. I suggest rephrasing to "...new lakes during large parts of the year"

We agree and modified line 296 accordingly.

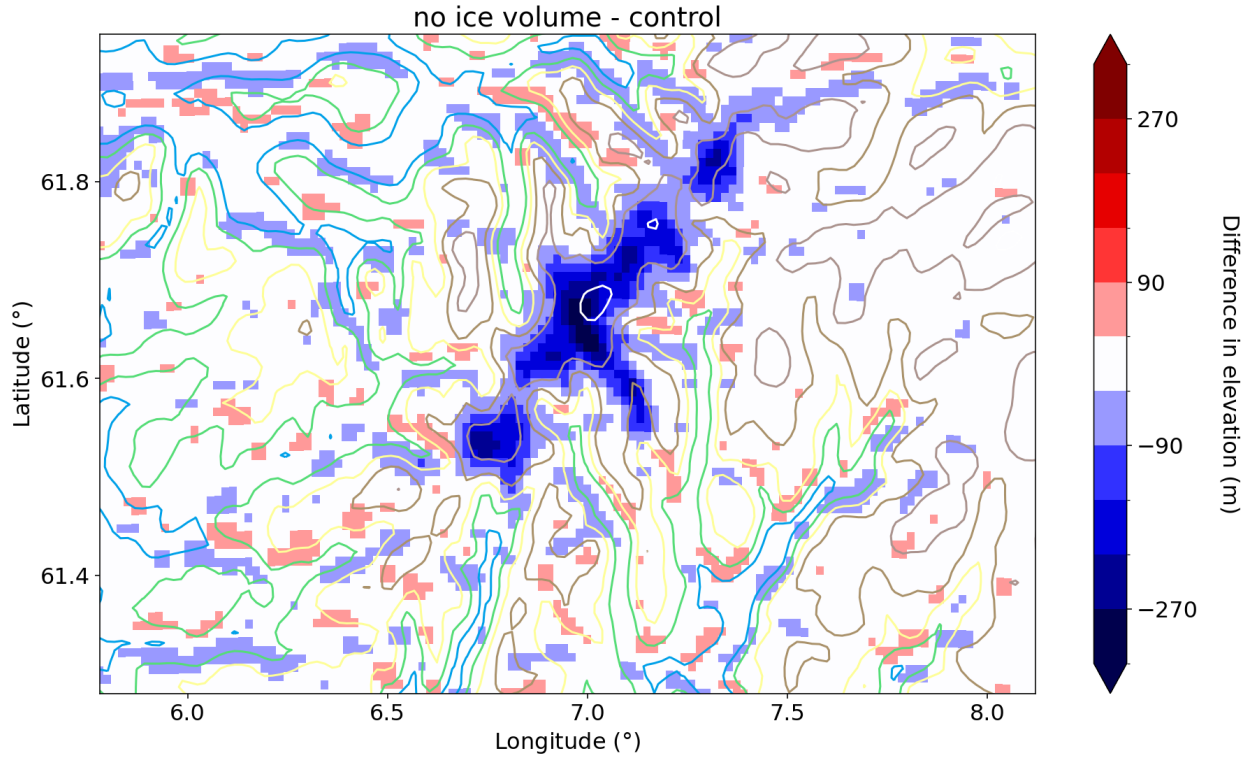


Fig. R1: Change in elevation for no-ice-volume experiment relative to the control experiment.

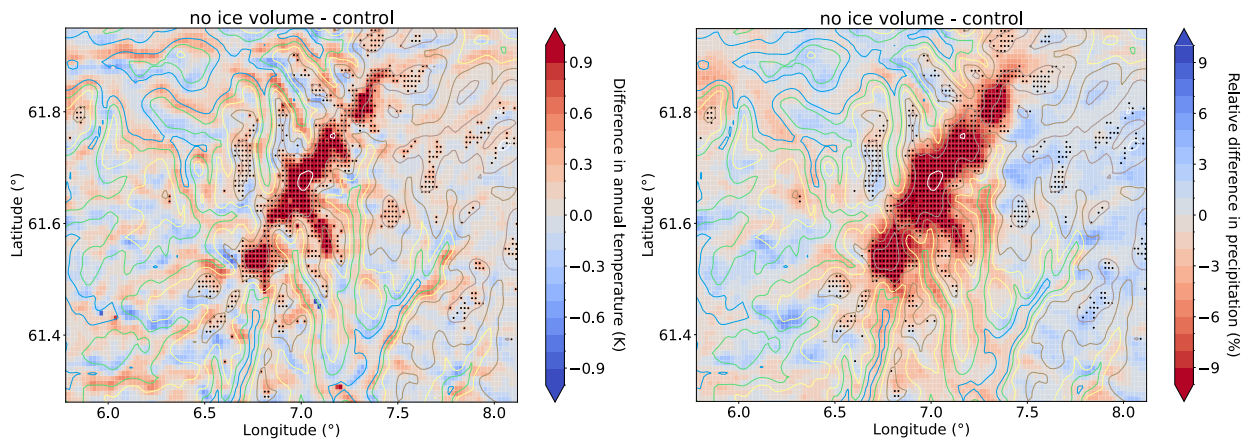


Fig. R2 and R3: Change in temperature and precipitation for the no-ice-volume experiment relative to the control experiment. Similar to Fig. 5c and 6c in the updated manuscript. Note that negative elevation changes (blue in Fig. R1) correspond well with positive temperature changes (red in Fig. R2) and relatively well with negative precipitation changes (red in Fig. R3, particularly in northwest), as expected.