Our responses to the review comments are in this document, where the Editor's comments are shown in *blue italic text*, our responses are shown in plain black text, quoted text from the original manuscript are shown in *black italic text*, and any changes to the manuscript are shown in **bold black text**.

Dear Authors,
Thank you for your patience with my editorial decision, and for your replies and revisions in the second round of peer review. Both I and the reviewers think your manuscript can make an interesting and valuable contribution to TC. However, significant changes are still needed before publication can be considered. In particular, the methods remain hard to follow in places, contain unsupported statements, and need more details in order for this work to be reproducible. I highlight some issues and examples below, but encourage further revisions to improve readability, clarity and completeness where possible. The revised manuscript will be evaluated by a third reviewer prior to potential publication.
We thank the Editor for their detailed comments on the revised manuscript and for the opportunity to revise our manuscript further. The Methods section has been substantially revised to described the steps of the model testing and evaluation more explicitly. Additional information has been added on the analysis and use of meteorological data and the reference simulation and sensitivity experiments in the main text and Appendix. The entire manuscript has been edited to ensure that the text reads clearly and figures are accurately referenced.

Key issues:
1. Forcing data
The methods and results related to the downscaling are not described clearly enough to be easily followed or reproduced (e.g., line 234 gap filling of the AWS record -- when, which variables; line 246 seasonal adjustment of downscaled precipitation). In addition, some key related statements are not fully supported (e.g., lines 482-484 and lines 628-630). Please significantly revise the text, to provide an easily readable description of the methods, which reflects the finest timescales on which data were evaluated (daily averages for temperature and annual totals for precipitation, subdivided by monsoon activity) and better integrates the results shown in Figure 2 and Appendix A, including more specific figure references. In addition, please clarify where the representativeness of the selected time periods is established, as I was unable to find this information in Appendix A.
A new section has been added to the Appendix (Section A1) to provide details of the gap filling for the temperature and precipitation datasets. A new Sectoion 2.2 has been added to the main text to provide details on the AWS datasets and the reasons for the incomplete observations and how these were handled.
The Appendix now contains more detail on the results of the reference simulation.
The entire text has been updated and the figure referencing checked throughout to ensure that this is clear and consistent with the results as presented.

2. Projections beyond 2100
Both I and the second reviewer have concerns about the robustness and added value of extending the glacier simulations beyond 2100 given both the forcing data inhomogeneities and the lack of precipitation projections. Although the temperature data could be homogenized as suggested by the reviewer, I wonder if it makes sense to include these results at all, given the importance of precipitation changes in offsetting projected mass losses by 2100 that is emphasized in this study. In any case, this issue needs to be addressed more fully than being stated as out of scope.
We agree that the uncertainties associated with the simulations post-2100 CE are much higher, and that the focus of the manuscript is on the impact of precipitation amount and change in this variable, which is only projected until the end of the century, and have therefore removed the simulations beyond 2100 CE and the text and figure sections describing these to improve the focus of the manuscript.

3. Glacier modelling
- The vapor fluxes are calculated in COSIPY with respect to a clean snow or ice surface. How are the fluxes adjusted over debris-covered areas when they are exposed (re: line 438)?
This is beyond the scope of our modelling approach, and the following text has been added to Section 2.5 where the debris thickness-mass balance calculation is described:

"We note that Equation (2) represents an empirical calculation of the impact of supraglacial debris on glacier surface melt that is calibrated to observations of sub-debris melt rates for glaciers in the Central Himalaya (Rowan et al., 2021) and as such, changes in surface energy balance processes including vapour fluxes within the debris-covered section of the glacier are not included."

- Re: your reply to the reviewer's comment about model calibration: I may have missed it, but where exactly is the comparison between the observed and simulated mass balances underlying the calibration provided? Also, could an overview of the COSIPY parameter sensitivity results (perturbation tested and impact) be summarized in a table so that the information is easier to follow?
The comparison between simulated and observed mass balance for the 5-year present day time slice is shown in Fig. 6. Section A5 has been added to the Appendix to discuss in more detail the variability in calculated surface energy balance across the glacier shown in Fig. 5, and the new Section A6 presents COSIPY parameter perturbations and sensitivity testing results. We decided to present these as text and in greater detail than a summary table as these results may be useful to future studies.

- Please rephrase "climate-glacier model" as a workflow or approach throughout the manuscript (e.g., lines 108, 120, 153), as all glacier models need forcing and coupled climate-glacier modelling is not done in this study. Please also clarify what is meant by "the glacier model" (i.e. not COSIPY) throughout (by using "iSOSIA" instead? e.g., lines 207, 315, 420, 432, 504, 628, 1396, 1409).
The generic name for the modelling approach has been revised throughout to be "a glacier modelling approach" or "the glacier model", with more detail given on forcing and specific model components as needed.

Other Corrections
Line 22: „mesoscale meteorology is not represented in current glacier models" – please clarify the methodological novelty, as there are studies forcing glacier surface energy and mass balance models with atmospheric data on scales of ~(10-100) m
This sentence has been changed to be more specific and state that the novelty of the study is in investigating the effect of precipitation distribution and change on glacier change. This text now reads: "the impact of changes in precipitation amount and distribution on future glacier change remains uncertain because **this variable** is **often** not represented in glacier model **projections."**

Line 27: replace "from downscaling of Regional Climate Model results" with "from statistical downscaling of existing regional climate projections"
Done.

Line 73-75: please correct, Wirbel and colleagues modelled debris transport in a 3D velocity field
The citation to Wirbel et al. (2018) has been moved to the end of the sentence to clarify that 3D dynamics were used in their study.

Lines 121, 129: please clarify how "appropriate scale" and "robust representations" were quantified or remove
Text removed.

Line 135: this is the first mention of COSIPY and iSOSIA, please move references about these models here
Citations added here.

Line 156: remove "forced by"
Done.

Line 178: remove "climate"
Done.

Throughout the manuscript, please add "(not shown)" where the result is not visible (e.g., line 197 lapse rate testing, line 211 volume difference without avalanching, line 1397 closest agreement with geodetic observations, line 1419 radiative flux comparison) or include it as a supplement
"(results not shown)" added as suggested. We note that the comparison with geodetic measurements are shown in Fig. 3D and 3E, although the citation here is to Rowan et al. (2021) where the King et al. (2020) results are reproduced. A reference to Figure 3 has been added and the caption to this figure updated to clarify the original citation for the geodetic data.

Line 200: "We examined the uncertainty in accumulation resulting from the application of a calculation to move snowfall from slopes susceptible to avalanching" please indicate that further information will be provided in Section 2.5
Text here updated to read (note that section numbering is updated):
"We examined the uncertainty in accumulation resulting from the application of a calculation to move snowfall from slopes susceptible to avalanching**, which is described in detail in Section 2.6**."

Line 239: clarify the correction was done in this study and not Maraun et al. (2016)
Text here updated to read:
"This correction was applied to the raw RCM outputs **in this study** to produce a third downscaled dataset…"

Section 2.4: please remove the repetitive text in this section (lines 271, 272, 279, 309, 305, 312-314) as well as the reference to Weidemann et al., 2018.
Text removed.

Line 435: Figure 6?
Yes, this has been corrected.

Line 443: "snow cover that shielded the subsurface from surface temperature" does not make sense, please rephrase
Text here updated to read:
"due to persistence of the initial snow cover that shielded the subsurface from **relatively warm air temperatures** until the subsurface adapted to local conditions."

Line 445: where can the refreezing result be seen?
In Fig. 6, where Q_{melt} is at or below zero each winter at each site, with differing durations of freezing, as shown in the figure. The figure reference has been added here to indicate this. The new figure A6 in the Appendix shows the results for refreezing as calculated in COSIPY.

Line 452: where does Figure 6 evaluate model performance in simulating radiative fluxes?
In general, the manuscript would benefit from more explicit figure referencing (e.g., lines 500-502 and Figure 5).
Apologies, this is not shown in Fig. 5 as stated, and this figure reference has been removed. Figure referencing throughout the manuscript has been revised to more carefully support the text, including adding a reference to Fig. 6 for the mass balance evaluation (lines 500–502 in the original manuscript).

Line 510: there is no reference extent in Figure 8, to show underestimation when excluding debris
Fig. 8 has been updated to show the mass balance and ice thickness when no sub-debris melt is imposed (the clean-ice glacier condition) for comparison.

Lines 517, 519: 1984 to 2015 or 2018?
This was a typo in one place, corrected to 2018 here.

Line 540: please clarify if this study found greater warming in winter, or Sanjay et al. (2017)

This is a result from our study, and the reference to Sanjay et al. (2017) has been removed here.

Line 1421: how does Matthews et al. (2020) support this humidity gradient?
Text here updated to read:
"The relative humidity gradient was calculated as –0.002 % m^{-1} **using data from** the Ev-K2-CNR and the GlacioClim **AWS** networks, **and evaluated by comparison with measurements made by the National Geographic network of 5 AWS ranging in elevation from 3,810–8,430 m a.s.l.** (Matthews et al., 2020) to capture trends at higher elevations."