We thank the Reviewer for their comments on the revised manuscript. Our responses to their comments are in this document where the Reviewer 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**. The manuscript and appendix have then been edited for clarity and concision.

Anonymous referee #2 Submitted on 17 Sep 2025
The authors have revised the manuscript substantially and included additional paragraphs and information to address the reviewers' earlier suggestions. I appreciate the effort that has gone into clarifying the methodology and extending the appendix. However, I must admit that I am still struggling to follow several aspects of the climate-glacier modelling approach. I understand the authors' intention to keep the main text concise and therefore refer frequently to earlier publications (Rowan et al., 2015, 2021) or the appendix, but in its current form, Section 2 (Methods/Modelling) is not yet fully self-contained. For the reader to easily grasp the workflow, the methods should be presented in a clearer, more consistent, and complete manner. My main comments are outlined below:

1. Methodological structure and redundancy: Several parts of the methodology are redundant and would benefit from re-structuring. At present, the climate–glacier modelling design is described across three different sections (2, 2.5, and Appendix B.2, pluss cross-links to previous publications), which makes it difficult to follow. I would recommend beginning with a concise overview of the modelling framework (synthesising Sections 2, 2.5, and Appendix B.2) and introducing the two main components (surface energy/mass balance and ice dynamics) before moving to the downscaling of RCMs. While Appendix A is well organised, Appendix B.1 currently combines model description, model evaluation, and energy flux analysis in a way that feels disjointed. One option would be to create clear subsections (e.g., 1. COSIPY model description, 2. Energy flux analysis), or alternatively, shift the main findings from the SEB/SMB modelling into the Results section and retain only the model description in the appendix.
We thank the Reviewer for their detailed comments on the revised version of the manuscript. While we worked to ensure that the climate-glacier model experiments were described in sufficient detail (there were 200 lines in the methods Section 2 of the main text in the R1 version of the manuscript) we agree that the structure of Methods Section 2 could be improved, rather than separating this across the main text and appendices. Section 2.5 *Glacier Model Experimental Design* was intended to describe the experiments after the models and their parameterisation were described in detail, but this ordering is not essential and **this section has been moved to the start of Section 2 immediately after the introductory text that describes the overall modelling concept and is now Section 2.1, with subsequent sections renumbered.**

All the content in Appendix B has been moved to the main text: Appendix B1 which described the COSIPY surface energy balance modelling methods and results has been in part moved to the methods and combined into Section 2.4 COSIPY surface energy balance modelling. The results from Appendix B2 have been moved into the main text results section; Table B1 is now Table 2, and Figure B1 has been moved into the main text and is now Figure 6. Appendix B2 which described the glacier modelling in further detail was combined into the results. Figure B2 has been moved into the main text and is now Figure 3. We retain Appendix A which describes the climate model downscaling rather than move this to the main text, as this contains only extended methods and would overly expand the main methods section. The Methods Section 2 now stands at 268 lines and we consider that this text is sufficiently detailed to describe the experiments undertaken in this study with an appropriate level of reliance on previously published descriptions of all the models and data used.

2. COSIPY model setup and calibration: As far as I understand, this is the authors' first application of COSIPY to Khumbu Glacier to simulate spatio-temporal variations in energy fluxes and mass balance. Therefore, the calibration and setup should be described in sufficient detail. It remains unclear whether COSIPY was calibrated against glaciological or geodetic observations, or whether the authors relied entirely on literature values (as Table B1 seems to suggest). The manuscript mentions that albedo values *were perturbed by 5 %, but I could not find any consistent description of a calibration strategy or sensitivity analysis of key parameters in COSIPY. This should be clarified.*

The Reviewer is correct that this is the first, to our knowledge, use of COSIPY for modelling Khumbu Glacier, and we have added further information to support the model application. We made a series of reference simulations forced by AWS data for the period 2013–2015 CE that were compared with observed mass balance for Khumbu Glacier that are now included in Section 3 in the new sections **3.1 COSIPY parameter perturbations** and **3.2 Evaluation of COSIPY surface energy and mass balance results** that present a summary and figures illustrating the calibration of the COSIPY model for Khumbu Glacier, including a new figure showing the COSIPY reference result (Fig. 4). As noted above, Appendix B1 has been combined into this section, which contained evaluation of the COSIPY radiative fluxes at five points on the glacier (Fig. 6).

3. SMB forcing and spin-up simulations: Although the authors responded to the comments of both reviewers during the first round concerning the modell forcing between the present day and future 5-year time slices, I'm still wondering which SMB serves when as input for the transient simulation of Khumbu Glacier. How do you generate your SMB fields for the 5000-year spin-up simulation (ELA set to 5,325 m – where does this value come from? – and fixed accumulation and ablation gradients)? Which meteorological data do you consider for the spin-up phase and LIA period? How do you modify the SMB during the LIA – the only information is a 50 m ELA increase over 500 years, so 1 m per decade? I understand that the authors use the SMB from COSIPY for the two time slices (2015-2020 and 2095-2100), but which forcing is then applied for the transient simulation from 2020 to 2095? The manuscript states that "no change in forcing [is] applied between [the two] time steps" (Line 735). Could the authors clarify precisely when the switch from present-day forcing (2015–2020) to future forcing (2095–2100) occurs?

We use the LIA simulation from Rowan et al. (2021) as the starting point for the simulations presented in this study. This LIA simulation results from an elevation-dependent mass balance function using values from Benn and Lehmkuhl (2000) where change in MAAT (ELA) is used to force the model to equilibrium, which is extensively evaluated against observational data in our 2021 paper. To avoid excessive duplication of previous work, this was briefly described in the current manuscript as noted by the Reviewer. The focus of the current manuscript is on using COSIPY calculations of mass balance to simulate the present day and future glacier and as such the spin up is intended only to provide an ice volume as a starting point. The following text has been added to Section 2.1 to clarify this point:

We used a step forcing rather than interpolating mass balance over time, whereby the future mass balance is imposed and the glacier adjusts to this from the start of the century in question. We arrived at the present-day simulation from the LIA simulation by forcing the LIA glacier with the 2015–2020 CE mass balance for 200 years. We use the output from the present-day simulation with the 2095–2100 CE mass balance to force the model to 2100 CE for a period of 80 years. We then use the result from this simulation as the starting point for the 2200 CE simulation forced by the 2195–2200 CE mass balance for 100 years, and the same approach for the 2300 CE using the 2295–2300 CE mass balance.

And the limitations of this approach are expanded in the 3rd paragraph of the Discussion section 4.1:

An experiment was conducted using mid-century (2045–2050 CE) mass balance forcings to investigate any effect on glacier-climate imbalance. This experiment produced near-identical results in 2100 CE to the experiments with no mid-century forcing, because the response time of the simulated glaciers was longer than the 40-year period between the present-day and future time slices, and so the mid-century surface mass balance forcing was not considered necessary in our experiments. We used a step forcing for mass balance rather than interpolation between mass balance calculations in the glacier model. The glacier continued to evolve through each time period rather than equilibrate with the forcing, but this approach did result in a stepped response in terms of ice volume change over time where the forcing was changed (Fig. 10d).

4. SMB-elevation feedback: I recognise the technical challenges of coupling iSOSIA and COSIPY. Nevertheless, neglecting glacier surface elevation changes in COSIPY may underestimate mass loss by omitting the SMB-elevation feedback. A discussion of this limitation would strengthen the manuscript.
We agree, and the following comment has been added to the Discussion section 4.1:
We note that this stepped approach could be improved by interpolating the mass balance over time and coupling the COSIPY and iSOSIA models such that mass balance was calculated dynamically for the evolving ice surface, but this was beyond the scope of our model application.

5. RCP 8.5 trajectories: The mass change trajectories under RCP 8.5 show a re-acceleration of glacier mass loss after 2100, which appears unexpected and is likely linked to the transition from RCM to GCM forcing. In its current form, I am not convinced that robust conclusions can be drawn from these simulations. A discussion of the plausibility of the RCP 8.5 projections is needed. The authors might consider alternative strategies to smooth the forcing transition, for example by homogenising the RCM and GCM datasets (e.g. using the final decade of RCM simulations) to provide a more consistent post-2100 forcing.
We agree, and this also relates to the use of the mass balance forcing in the glacier model, as described above. We note that the original text in section 3.3 of the results stated that the projections beyond 2100 CE were more uncertain, and the manuscript has more generally been updated to reflect this point. The Discussion section 4.1 has been updated to address this point:
The reacceleration in glacier mass loss after 2100 CE is in part due to this stepped forcing approach and the uncertainties associated with GCM projections, which increase with time after 2100 CE particularly under RCP8.5. For example, forecasts of global warming for 2281–2300 CE relative to 1986–2005 CE under RCP8.5 range from 3.0°C to 12.6°C (Collins et al., 2013). The transition in the glacier model between the downscaled RCM and the GCM forcing could be improved by homogenising the climate model results across 2090–2100 CE, however, as noted above, the computational expense of forcing COSIPY with downscaled RCM outputs to create inputs to the glacier model required the use of a time-slice approach that was limited to five-year periods, and integration of the RCM results with GCM results was beyond the scope of this study.

6. Comparison with observations: When comparing modelling results with observations, the manuscript often remains vague about which specific datasets are used. For instance, "The distributed surface mass balances calculated using COSIPY are most similar to observed values…" (Line 888) does not specify which observations are meant. I suggest including either a dedicated paragraph or a concise table summarising the in situ and remote-sensing datasets employed for model comparison and evaluation. Some of this information is included in Figure B2 captions, but it would be more helpful in the main text.
This text is now contained in Section 3.1 where Figures 3 and 5 compare the glacier model results with observations. The new Section **3.2 Evaluation of COSIPY surface energy and mass balance results** addresses the evaluation of the mass balance calculations, see response to Reviewer comment 2 above.

Minor comments:
Line 31: "is only" => 34 % is still considerable; better: "is reduced to"
Updated as suggested.

Line 288: "sublimination" => sublimation.
Done.

Line 419: "2,100 m a.s.l.." and thereafter => "2,100 m a.s.l."
The double period here and elsewhere gives one for the abbreviation of level and one to end the sentence.

Line 452: "m a.s.l" => "m a.s.l."
Done.

Lines 1429–1431: "parameterisation of avalanching [...] improved the agreement between simulated and observed accumulation rates" => please provide information on observed accumulation rates and a reference if available
Citation added. There are few observations of avalanche contribution to accumulation in the Himalaya at present, so we rely on Benn and Lehmkuhl (2000) here.

Line 2360: "–0.00554 °C m –1" and thereafter => strictly speaking, a negative lapse rate implies an increase in temperature with height; please revise.
Noted, and corrected in the manuscript.

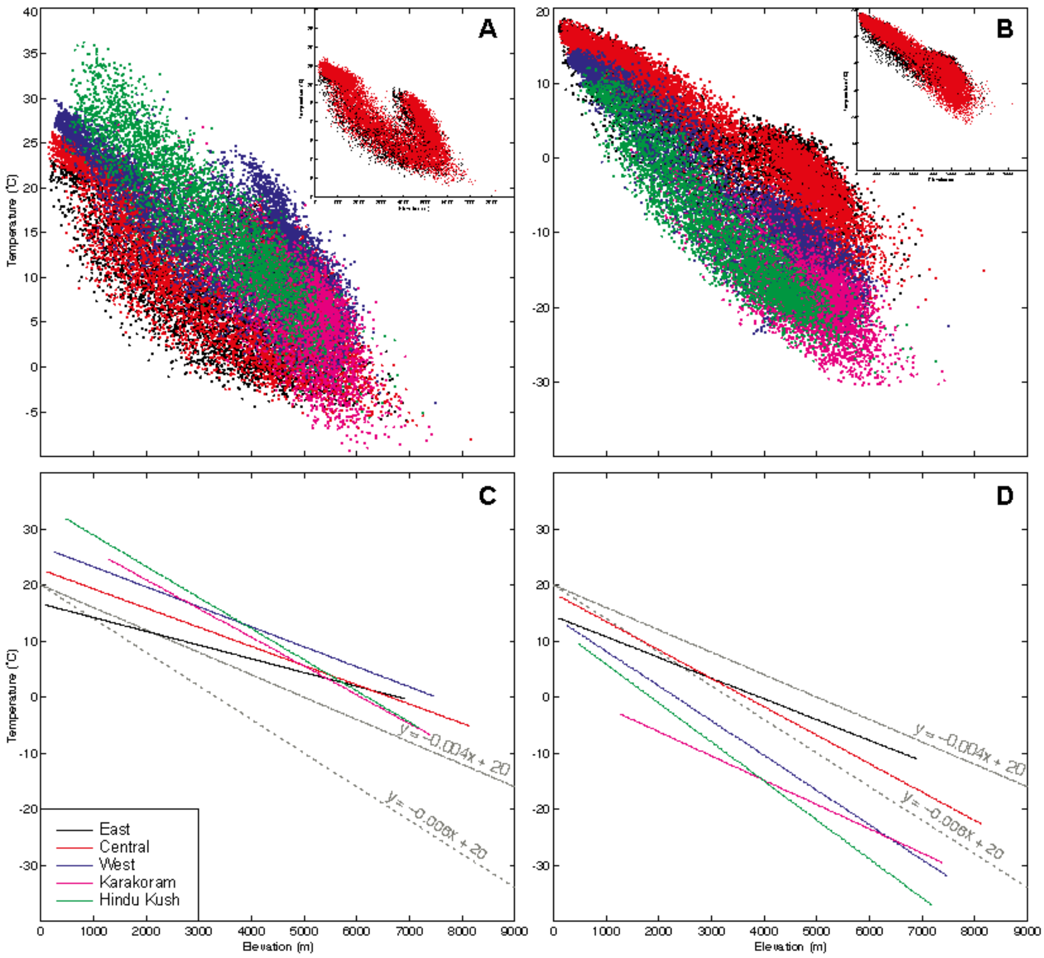
Line 2361: "produced glacier-wide mass balance and spatial calculations that were closest to those observed" => please provide information on the observed mass balance (glaciological or geodetic?)
Citation added to King et al. (2020) and text updated to state that these are geodetic observations.

Lines 2366 and 2374: "precipitation lapse rates" => the term "lapse rate" usually refers to temperature; "precipitation gradient" would be more appropriate
Corrected throughout the text.

Lines 2486–2487: Incomplete sentence: "Reconstruction of Khumbu Glacier using terrestrial cosmogenic nuclide dating of boulders on the surface of ice-marginal moraine crests shows that since the late Holocene…" Sentence removed as this point is now made in the following paragraph of Section 2.1.

Line 2497: "lapse rate -0.004 °C per m" => should be positive (see above).
Noted, and corrected in the manuscript.

Line 2500: "0.003°C per m-1" => use consistent units, e.g. "°C per m" or "°C m-1." Please also explain the reasoning for limiting the upper range of atmospheric lapse rates to 0.6 °C per 100 m, which is quite low.
These values were calculated from regional land surface temperature data, as presented in the plot below (not added to the manuscript), and the sentence has been updated to read:
We tested a range of atmospheric lapse rates from 0.003°C per m^{-1} to 0.006°C per m^{-1} maintaining the same ELA, **based on the range of monthly values calculated from regression of NASA MODIS land surface temperature data for the Central Himalaya,** which resulted in a difference in ice volume of 0.4 x 10^9 m^3 and no change in glacier length.



Line 2509: "not consider" => "not considered"
Done.

Lines 2512–2519: Sentence is overly long; please split into shorter sentences.
Done.

Table B1: "glaciological constants" => "model parameters"
Done. Note that this is now Table 2.