We thank the two Reviewers for their comments on this manuscript. Our responses to their comments are combined 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 suggested changes to the manuscript are shown in **bold black text**. The entire manuscript including the appendices have then been edited for clarity and concision. The major change that we have made is that Appendix A has been expanded to better describe the analysis and use of the RCMs and AWS data. New Appendix B has been added to describe the analysis and use of COSIPY and iSOSIA.

*RC1: **'Comment on egusphere-2025-1211', Emily Potter, 16 May 2025***

*This manuscript investigates the change to the Khumbu glacier in the Nepalese Himalaya under future climate change, extended to 2100 using downscaled regional climate models from CORDEX, and to 2300 using delta change forcing. The study incorporates statistical climate downscaling, energy and mass balance modelling using COSIPY, and glacier modelling using iSOSIA.*

*The manuscript is well written and presents an important updated projection for the future of the Khumbu glacier. Of particular interest is the result that precipitation changes may considerably offset glacier mass balance loss caused by warming, under a moderate climate emissions scenario. In addition, the comparison of the future results to mass loss already committed by current warming provides a useful benchmark.*

*Due to the ambitious use of three different modelling/statistical techniques described in a concisely written paper, I have a few questions regarding some choices and methods, detailed below. I appreciate the authors' effort to keep the manuscript brief and easily readable, so would recommend any substantial additions to methods be added to the appendices rather than the main text, but this is of course at the discretion of the authors.*

We thank the Reviewer for their positive assessment of our manuscript and constructive comments. As suggested here, the appendices have been substantially expanded to include the requested information.

*Comments*

*Line 117: 'RCMs were assessed…' Was this something the authors did? If so, were these RCMs compared against other CORDEX models? Based on fig. A3, the raw RCMs seem poor at representing the annual cycle in precipitation. While the quantile mapping has largely rectified this, it may be worth commenting in section 4.1 on the reliability of future precipitation trends from the model if the monsoon is poorly represented in the present day. Also relevant at line 394 inferring these CORDEX models give a good performance.*

Yes, we assessed the RCMs as described in the manuscript to determine which were the most suitable to use here. The following text has been added to add further detail on the RCM assessment in Section 2.1:

**"The three remaining RCMs were discounted due to being intermediate to those selected for our experiments (i.e. close to the future precipitation scenario represented by CCCma) or particularly poor at reproducing seasonal temperature and precipitation cycles. For example, despite the annual precipitation sums from the CSIRO RCM being closest to observed values and having the potential to be the 'driest' scenario examined, analysis of precipitation seasonality indicated that the monsoon signal was completely absent with a strong dominance of winter precipitation in the results of this RCM."**

and in Section 4.1:

**"Whilst the three selected RCMs performed relatively well in representing annual precipitation cycles from the six available CORDEX RCMs, we note that this representation was still fairly poor, although substantially improved by quantile mapping. The poor representation of monsoon dynamics in the present-day RCMs highlights an additional uncertainty associated with future precipitation scenarios and that these results should be treated as a set of possible scenarios."**

Line 394 in the original manuscript has been edited to read:

**"A multi-model mean approach using all the CORDEX South Asia RCMs, which is widely used elsewhere, was not considered sufficient to represent present-day and future climate conditions**

**in the Khumbu Valley as this approach gives equal weighting to models irrespective of performance in reproducing climate (Pierce et al., 2009), and does not enable intercomparison of results from these future climate conditions.”**

*Lines 123-126: Could you add a reference or plot to show that these CMIP models span the range of possible future scenarios for precipitation? How do these CMIP models compare in terms of temperature increases? This would be particularly useful compared to the 10 GCMs used by Rounce et al., (2023), to determine whether higher temperature projections are contributing to the difference in mass loss. The average and range of equivalent climatic changes for the GCMs used in Rounce et al., (2023) could perhaps be added to Table 1, to cover both points.*

This is now shown in Fig. A4 and the values for projected precipitation in our results have been added to Table 1. The climate forcing used in Rounce et al. (2023) is based on ERA5 data to the present day then projected using GCM outputs that ‘are adjusted using additive factors for air temperature and multiplicative factors for precipitation to remove any bias between the GCMs and ERA5 data over the calibration period (2000-2019)’ (*ibid*.), which is not directly comparable with our climate forcings that are calculated from RCMs that take their boundary conditions from a set of the GCMs. However, the use of the IPCC RCPs in their study and ours enables model comparisons as the equivalent climate change is the same.

*Section 2.2: Quantile mapping can cause biases in future model-projected trends (e.g. fig. 2 Cannon et al., 2015). Does the method used here preserve the trends from the RCMs? In addition, assuming the RCMs have the common problem of too many wet days, does the method used act to correct frequency of precipitation?*

The trends from the RCMs are preserved through quantile mapping, and to summarise this the following text has been added to this section:

**“The temperature change between the present day and the future time slices was preserved and there was no evidence of any imposed strengthening in the monsoon resulting from this downscaling. An increase in the frequency of days per year outside of the monsoon season with high precipitation amounts (defined here as over 15 mm of daily precipitation) accounts in large part for the higher annual precipitation amounts relative to present day found in four out of the six RCMs. However, the total future annual precipitation increase is on average 8.8% greater in the downscaled climates relative to the raw RCMs, suggesting this positive trend was inflated following downscaling. The downscaled climates reduced the frequency of precipitation, although, as in present day observations, monsoon precipitation occurs frequently and can be characterised as predominantly drizzle into the future.”**

*Line 131 and throughout: the time periods used get a little confusing. I think that 14 years of AWS data (2006-2019) is used to downscale 5 years of COREX model data (2015-2020). Could this be clarified in the methods, as it wasn’t fully clear until the discussion.*

Apologies, the use of the AWS data to downscale the CORDEX model results has been clarified throughout the text.

*Section 2.3: Are all meteorological inputs to COSIPY taken from the nearest RCM point? Except for precipitation and temperature, was any correction applied to other variables (e.g. to atmospheric pressure to correct for the elevation difference)?*

Yes, relative humidity, direct radiation, and atmospheric pressure were all corrected and this is now described in detail in the Appendix A in the section ‘**Meteorological distribution across the domain’.** Figure B1 shows the points used for meteorological inputs to COSIPY and the model results used.

*Section 2.4: What forcing was used for the glacier modelling between the present day and future 5-year time slices?*

No change in forcing was applied between the two mass balance time slices and therefore these are applied as step changes. As described in the Discussion section of the original manuscript, we found that including a mid-century mass balance forcing had negligible impact on the results:

*"An experiment was conducted using mid-century (2045–2050 CE) mass balance forcings to investigate the effect on glacier-climate imbalance. However this experiment produced 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 mass balance forcing was not considered necessary."*

This has been clarified in Section 2.4:
*"The distributed mass balances calculated using COSIPY (forced by the downscaled RCMs) were used as input to the glacier model* **with no change in forcing applied between these time steps**"

*Figure 3 doesn't appear to be referred to in the text.*
Thanks for spotting this; Figure 3 is now referenced in Section 2.3.

*Line 295: I'm not sure where the observations of present-day state, or the recent (<50 years) change are in figure 4. If this was meant to be the reference to figure 3, could the authors clarify in the figure caption where the 50 years comes from?*
This was poorly phrased, and has been updated to clarify that the spin-up simulation of the present-day glacier shown in Fig. 4 was compared with geological and remotely sensed observations following Rowan et al. (2021) where the 50 years refers to change over the period of satellite observations. The text has been updated to read:
**"The simulated glacier geometry and dynamics (Fig. 4) were compared with remotely sensed observations of velocity, surface elevation change and debris cover extent for the present-day glacier and moraine positions indicating the extent during the Little Ice Age maximum (Rowan et al., 2021)."**
Detailed information about the glacier model evaluation has been added in Appendix B and referred to from the main text.

*Line 297: As I understand it, the model performs better with the mass-balance calculated within the glacier model, rather than by COSIPY. Could the authors clarify which method is used for future projections?*
This was not clear as stated and has been misunderstood by the Reviewer. We use the mass balances from COSIPY throughout, but combining these with the sub-debris melt scheme in the glacier model improves the fit to observations of mass balance. This has been rephrased to:
**"The distributed mass balances calculated using COSIPY were more similar to observations when integrated with the glacier model to include the effects of melt reduction beneath supraglacial debris."**

*Line 357-360, and fig. 7 and 8: Could the authors clarify the difference in results (up to 2100) shown in fig. 7 b compared to fig. 8? The discussion of difference between RCP scenarios seems to hold true for fig. 7, but not 8.*
The model results shown in Fig. 7b and Fig. 8 are the same but while those in Fig. 7b are shown for the complete time period (2015–2300 CE) those in Fig. 8 are only for the end-of-century period, and the difference between RCPs is greatest after 2100 CE. We have combined Fig. 8a into Fig. 7 to enable easier comparison.

*Line 456 and figure 8: The mass change between this study and Rounce et al., (2023) appears to relate to different starting points, e.g. this study projects a loss of ~35% compared to a 2010 glacier mass, whereas Rounce et al., (2023), project a loss of ~75% compared to the 2000 glacier mass. The comparison might be more suitable if both graphs were shown in relation to the same starting value, i.e. by shifting the curves for Rounce et al., (2003) up to start at the projected 2010 value. I would also suggest changing the colour of the line Rounce et al., (2003) RCP2.6 (or removing entirely), as it is similar to this study's RCP4.5.*
Figure 8 has been combined into Figure 7 and is now Fig. 7d. The starting point for the Rounce et al. (2023) experiments have been redrawn so that this is the same as in our study. Figure 8b has been

removed as the only new information in this figure was the result from our temperature-only forcing simulation which is now shown in Fig. 7d.

*Section 4.2 Are there other studies of future mass loss in the nearby region that could be included in the comparison discussion here? E.g. Khadka et al., (2020)*
While there are other studies of future glacier change in this catchment, we have cited only those that use a similar approach of forcing a glacier model with climate model outputs and a surface energy balance calculation as these are most directly comparable with our results.

*Table1: typo in 2300 heading (should be "change from 2200 CE…"?).*
Yes, this has been fixed.

*Line 161: "represent moderate and extreme warming…" change to moderate and extreme emissions scenarios, rather than warming.*
Done.

*Figure 5 and 6: please define h_0 in the figure caption.*
The following text has been added to both figure captions:
**"where $h_0$ is a constant in Equation (1) representing the characteristic debris thickness at which the reduction in ablation due to insulation by supraglacial debris is 50% of the value for an equivalent clean-ice surface (Anderson and Anderson, 2016; Rowan et al., 2021)"**

*References*
*Cannon, Alex J., Stephen R. Sobie, and Trevor Q. Murdock. "Bias correction of GCM precipitation by quantile mapping: how well do methods preserve changes in quantiles and extremes?." Journal of Climate 28.17 (2015): 6938-6959.*
*Khadka, Mira, Rijan Bhakta Kayastha, and Rakesh Kayastha. "Future projection of cryospheric and hydrologic regimes in Koshi River basin, Central Himalaya, using coupled glacier dynamics and glacio-hydrological models." Journal of Glaciology 66.259 (2020): 831-845.*

*RC2**: 'Comment on egusphere-2025-1211', Anonymous Referee #2, 20 May 2025***
*This is a review of the manuscript (no. egusphere-2025-1211) by Anya Schlich-Davies et al., entitled "Increasing precipitation due to climate change could partially offset the impact of warming air temperatures on glacier loss in the monsoon-influenced Himalaya until 2100 CE", submitted for publication in The Cryosphere.*
*The authors investigate the mass balance and long-term evolution of Khumbu Glacier in the monsoon-influenced Himalaya from the Late Holocene through to 2300. Their approach combines multiple Regional Climate Models (CORDEX South Asia dataset) with two climate change scenarios (RCP4.5 and RCP8.5), a physics-based glacier surface energy and mass balance model (COSIPY), and a second-order ice flow model (iSOSIA). This integrated modelling framework is used to evaluate the effects of supraglacial debris, rising air temperatures, and changing precipitation patterns on future glacier mass loss. A notable strength of this study is its use of regional climate model data alongside the inclusion of key physical processes such as sublimation, avalanche-driven snow redistribution, and the evolution of the debris layer.*
*One of the central findings of the study is that under RCP4.5, projected mass loss of Khumbu Glacier due to global warming could be mitigated by up to 50% as a result of increasing precipitation trends projected by the considered climate models. The authors offer a valuable assessment of committed glacier mass loss from historical warming and outline potential future trajectories of this iconic glacier under different emission scenarios. These insights are not only relevant to the scientific community but are also critical for informing policy, particularly in the context of communicating the urgency and importance of emission reduction strategies to the public.*
*The modelling approach presented in the manuscript is ambitious and holds significant promise for improving the robustness and reliability of future projections for (debris-covered) glaciers in high mountain environments. However, in my view, the manuscript requires substantial and careful revision.*

*In particular, key aspects of the methodology lack sufficient detail and clarity, which limits the transparency and reproducibility of the results. I outline these concerns in more detail below.*
We thank the Reviewer for their detailed comments and recognition of the value of this work in improving projections of glacier change in the Himalaya by considering mesoscale meteorology and glaciological processes including the evolution of supraglacial debris layers. We have revised the manuscript and added to the appendices to address these comments as described below.

***Aim of the Study and Rationale for the Modelling Approach***
*The manuscript would benefit from the clear formulation of a central research question - e.g., How will projected changes in precipitation and debris thickness affect the mass balance and long-term evolution of Khumbu Glacier? - or the articulation of a testable hypothesis. While the overall aim becomes apparent throughout the manuscript, it is not stated explicitly, either in the abstract or the introduction. Including a concise statement of objectives would help guide the reader and clarify the study's focus from the outset.*
The following sentence has been added to the summary of aims paragraph at the end of the Introduction section:
**"In this study, we use a climate-glacier model forced by mesoscale meteorological variables to test the hypothesis that changes in precipitation in response to climate change will reduce the impact of warming on glacier mass loss."**

*In the introduction, I would expect a dedicated paragraph summarising previous modelling efforts for (debris-covered) glaciers in the region, along with a discussion of their limitations. This would provide context and a stronger motivation for the implementation of the enhanced modelling framework proposed in this study. The choice to extend the modelling period beyond 2100 is appreciated and commendable, as it allows for exploration of long-term glacier response to historical and projected climate forcing. However, the rationale behind selecting the specific modelling period (Late Holocene to 2300 CE) as well as the choice of models (COSIPY and iSOSIA) should be better justified and discussed.*
The Introduction has been substantially updated to summarise previous debris-covered glacier modelling efforts in general and in relation to the Himalaya and their limitations.

The following text has been added to justify the time scale of our simulations:
**"The simulations start from the late Holocene because this is the period when Khumbu Glacier was last in dynamic equilibrium with the local climate as evidenced by the large ice-marginal moraines dated to 1.3 ± 0.1 ka surrounding the present-day glacier (Hornsey et al., 2022) when the glacier surface was free of debris (Rowan et al., 2015). The focus of our experiments is to simulate glacier evolution to the end of the 21st Century, but the centennial dynamic response time of a large debris-covered glacier such as Khumbu Glacier means that the glacier continues to evolve beyond this time scale and we continued our simulations through the subsequent two centuries to explore the longer-term future glacier evolution."**

To justify the choice of models and data used for their evaluation, a new section has been added to Appendix A called **'Regional Climate Model analysis and selection'**.

*The authors assert that their modelling approach allows for a more robust representation of mesoscale meteorological phenomena compared to previous studies. However, they neither specify which phenomena are meant nor explain why these would be better captured by the selected Regional Climate Models (RCMs), which operate at a horizontal resolution of 50 km - still insufficient to resolve the complex orographic and microclimatic variability of the Everest region.*
This information has been added to Appendix A.

*Recognition is due for the integration of important physical processes such as avalanche-driven snow redistribution, sublimation, and debris layer evolution. Nevertheless, it is somewhat disappointing that the individual effects of these processes on the surface energy and mass balance, as well as on glacier evolution, are not discussed in more detail. Instead, they are only briefly mentioned in a single sentence*

*(Lines 478-479): "Our results show that avalanching and sublimation are important controls on recent and future glacier evolution for Khumbu Glacier." This deserves a more in-depth treatment.*
The effects of avalanching has been stated more clearly in the text and detailed information has been added to Appendix B. The impact of sublimation is now discussed in Section 4.3 where the following text has been added:
**"Sublimation simulated in our study occurred at all elevations with the highest rate of ice loss due to sublimation (–0.12 m w.e. $a^{-1}$) in the upper reaches of the Khumbu Glacier catchment near to South Col (about 7,495 m a.s.l.) where sublimation dominates ablation with only minor seasonality. Whilst this amount of ice loss by sublimation is not negligible, it is almost half that found in the point-based calculations after adjusting for the different time periods represented by our studies (Matthews et al., 2020), which is likely due to the assumed uniformity of wind speed across the model domain in COSIPY."**

***Terminology***
*I recommend that the authors carefully review and revise the terminology used throughout the manuscript to ensure consistency and scientific accuracy. A few examples of inconsistent or suboptimal phrasing include:*
*Line 6: "warming air temperatures" => replace with increasing or rising air temperatures.*
Done.

*Lines 34 & 83: "highest glacier on Earth" => this probably depends on the exact definition (mean/max/min elevation), stating just the elevation range as you did is sufficient. Instead of the superlative, I would rather emphasise the wealth of observational data available for Khumbu Glacier as the main rationale for its selection.*
We agree and this has been rephrased throughout.

*Line 69: "climate warming" => global warming or climate change*
Done.

*Lines 30 & 44: "moderate warming scenario RCP4.5" => given that RCP4.5 projects a global mean warming of approximately 2.5-3.0 °C, it is more appropriate to describe it as an "intermediate" scenario, in line with IPCC terminology.*
Changed so that **"Intermediate emissions scenario"** is used to described RCP4.5 throughout.

*Line 45: "extreme warming scenario RCP8.5" => see comment before and consider rather "high" or "pessimistic" emission scenario.*
"extreme warming scenario" replaced with **"high emissions scenario"** throughout.

***Data and Methods: Observations and Remote Sensing Datasets***
*A concise overview of the glaciological and meteorological observations, as well as remote sensing datasets used in the study, would be helpful to better understand the evaluation of the climate data and modelling results right from the beginning. I suggest including a section within the methodology, potentially supported by a summary Table (the location of key measurements could be indicated in Fig. 1c).*
To justify the choice of models and data used for their evaluation, a new section has been added to Appendix A called **'Regional Climate Model analysis and selection'**.

***Data and Methods: Bias Correction and Statistical Downscaling of RCM Outputs***
*While the general workflow for bias correction and statistical downscaling is understandable, several important aspects remain unclear and merit further elaboration:*
1. *Lines 122-124: "Three of the six available CORDEX South Asia RCMs (NOAA, CCCma, IPSL) were selected as discrete scenarios that span the range of possible future precipitation conditions (Table 1); either wet, moderate, or dry climate in 2080–2100 CE". What were the selection criteria for these three models? Please clarify how "wet," "moderate", and "dry" future climates*

*are defined. Ideally, provide quantitative precipitation trends for each case to substantiate this classification.*

The assessment of the precipitation scenarios from the RCMs was qualitative rather than quantitative, and this has been rephrased in the text. Figure A4 shows the precipitation change results for each of the six RCMs so that these can be visually evaluated by the reader.

2. *Figure A3: If I interpret this figure correctly, all three selected RCMs significantly overestimate mean annual precipitation (by up to 500%; i.e., 3000 mm vs. observed 600 mm/year) and the fraction of non-monsoonal precipitation. If so, this casts doubt on the reliability of the RCM projections. Does quantile mapping preserve the original projected precipitation trends? Please provide a brief discussion of the resulting uncertainties and potential implications for glacier modelling.*

The Reviewer has correctly interpreted the figure, but please note that the downscaled precipitation shown in Fig. A4b is a much better fit to observations than the RCM precipitation, and the trends have been preserved, as discussed in Appendix A.

3. *Lines 408-412: The sentence describing the use of AWS data and time-slice selection is unclear. How exactly did you use 14 years of AWS data to downscale and bias-correct five years of RCM outputs? This requires clearer phrasing and more detailed explanation.*

This is now discussed in Appendix A.

4. *Spatial input fields: How were spatial fields for air temperature and precipitation generated for the SEB/SMB simulations? Did you simply apply gradients (if yes, how were they calculated) in combination with the resampled 100-m DEM?*

This is now discussed in Appendix A.

5. *RCM variables for COSIPY: Please include a Table listing all RCM-derived variables (with units) used in the SEB/SMB simulations, indicating which variables were bias-corrected or downscaled (beyond temperature and precipitation, if applicable).*

Table B1 has been added to Appendix B to present this information.

6. *Table 1: It is unclear why the projected temperature increase from 2100 to 2200 is considerably lower than in the previous and following centuries for both scenarios. This warrants clarification. Also, please correct what appears to be a typo in the last column header. It should refer to changes relative to 2200, not 2300.*

Appendix A has been substantially expanded with new text and figures to address each of these points. For example, to address Point 1, Figure A4 has been added to illustrate 21$^{st}$ Century precipitation trends for each of the six RCMs. The typo has been fixed.

***Data and Methods: Glacier Modelling (Sections 2.3, 2.4 and 3)***

*Please note that Section 3 exists twice (Line 243 and 273).*

This has been fixed.

*Although I have read the glacier modelling sections multiple times, I still found the workflow difficult to follow without referring back to Rowan et al. (2015). For clarity and completeness, I recommend addressing the following points:*

1. *Glacier evolution model summary: While the glacier dynamics model has been previously described in detail by Rowan et al. (2015), the current manuscript would benefit from a concise summary of its key principles and the calibration approach. For example, was the model calibrated against observed surface velocities, ice thickness distributions, or terminus positions? How was model performance evaluated?*

This is now discussed in Appendix B.

2. *Methodological advancements over prior work: The authors should clearly identify the specific methodological advancements of this study relative to their previous publications (e.g. Rowan et*

*al., 2015, 2021). Is the integration of COSIPY the main novel component? Does the use of RCM-forced surface energy balance modelling represent a substantial improvement in boundary conditions for the dynamic model?*

The following sentence has been added to the Introduction:

**"This approach represents an advance in the use of such models to understand the evolution of Himalayan glaciers, as for the first time mesoscale meteorological forcing is used with a model that represents the processes of sublimination and snow avalanching, which are important controls on the mass balance of high-elevation glaciers."**

3. *Clarification of "late Holocene" reference: The "late Holocene" is mentioned as starting point for the simulations at different sections in the manuscript. Please provide a consistent and specific date (e.g. "1.3 ± 0.1 ka") early on in the text. Figure 1c should also show the location of the ice-marginal moraines used to constrain the glacier extent and thickness during the spin-up period. Additionally, it remains unclear which climate data were used for the 5000-year equilibrium simulation (Line 244) and how the transition to the Little Ice Age (LIA) was handled. How is the LIA defined in this context for Khumbu Glacier?*

This has been updated throughout the text to clarify that late Holocene refers to about 1 ka, which is the age of the largest lateral and terminal moraine surrounding the debris-covered tongue of Khumbu Glacier (Hornsey et al., 2022). The caption to Fig. 1 has been updated to indicate this extent:

**"…the glacier outline from the RGI database (black line) that is equivalent to the late Holocene (~1 ka) glacier extent identified from ice-marginal moraines,…"**

The text in Section 2.5 has been updated to clarify the forcing for the spin-up simulation, which is identical to that in Rowan et al. (2015, 2021), and further detail has been added to Appendix B:

**"The late Holocene (~1 ka) glacier was reconstructed using a 5000-year long equilibrium simulation starting from an ice-free domain and used as the starting point for three transient simulations through the 'Little Ice Age' maximum forced by a step change in mean annual air temperature (MAAT) equivalent to 1.5°C colder than the present day (Appendix B). The simulation was then forced to present-day conditions using the three surface mass balances (one for each RCM) calculated using COSIPY."**

4. *Choice and calibration of COSIPY: Please include a brief explanation why you chose COSIPY for your SMB simulations? The great advantage of COSIPY is that it considers physical processes such as sublimation and refreezing that are relevant in monsoon-influenced settings, but the drawback is that it is computatiolly costly and highly sensitive to climate input data and parameter calibration (see e.g. Temme et al., 2023). I could not to find any information on model calibration or any kind of sensitivity test. Did you calibrate the model against glaciological or geodetic mass balance observations?*

The justification for the use of COSIPY is given in Section 2.3:

***"COSIPY was chosen as it is currently considered a leading open-source method for estimating glacier mass balance and has previously been applied to glaciers in High Mountain Asia. COSIPY includes a calculation of sublimation, which is an important ablation process for high-elevation glaciers (Bonekamp et al., 2021; Brun et al., 2023; Huintjes et al., 2015)."***

Sensitivity testing of glacier-wide mass balance and point-based calculations of mass balance, energy flux and melt components was carried out using a range of atmospheric and glaciological parameters. The glaciological parameters are now inTable B1, with discussion added in Appendix B. Sensitivity testing of the lapse rates used for meteorological distribution of precipitation, temperature and the seasonality/time of day of temperature lapse rates, with discussion added to Appendix A. The evaluation of the simulated mass balance was carried out using *in situ* mass balance measurements (e.g. Benn and Lehmkul, 2000, Fig 1D) and remote-sensing observations of mass balance (King et al., 2017; 2020). This evaluation using these sources is discussed in section 3.1. Following integration with iSOSIA, velocity, ice thickness and debris thickness were evaluated using remote sensing observations (e.g. Luckman et al., 2007; Quincey et al., 2009; Heid and Kääb, 2012; Altena and Kääb, 2020). Whilst Khumbu Glacier is relatively well studied for the region, there is a paucity of mass balance measurements above the ELA and particularly of *in situ* measurements.

*Which values (default?) did you use for the various parameters (e.g. roughness length, albedo) in COSIPY? Please include a table with the model parameters and their values (or ranges if calibrated).*

These values have been added to Table A1.

*Also add uncertainties to your SMB simulations (Fig. 3 and Fig. 7B). Have you tested to run COSIPY with 3-hourly or 6-hourly time steps to reduce computational costs?*

In Fig. 3 the intention is that the uncertainty in the SMB calculation is represented by the range of RCMs used as shown here. Fig. 7b shows simulated glacier volume change and not SMB, and the uncertainties associated with such simulations arise from many sources making these difficult to quantify in a meaningful way. Instead we have discussed the potential sources of model uncertainty in detail throughout the manuscript. We did not attempt to reduce the temporal resolution, but carried out some testing to assess the impact of reducing the spatial resolution, finding that reducing from 30 m to 100 m had minimal impact on the results whilst greatly reducing computational expense. This point has been added to Section 2.3.

*Moreover, it would be valuable to visualise and discuss key physical fluxes simulated by COSIPY - e.g., trends in energy components (net radiation, turbulent fluxes), the role of sublimation, meltwater retention/refreezing - to support and contextualise your conclusions.*

Figure B1 has been added to show the melt components as 5-day average of energy fluxes across study period for five sites on the glacier.

5. *Coupling of COSIPY and iSOSIA: Based on Figure 2A and the manuscript text, it appears that COSIPY and iSOSIA were run in a one-way rather than fully coupled manner. Please clarify. How often (e.g. every season, year, decade) did you update the SMB in the ice dynamics model to compute avalanching, ice flow etc.? Which SMB did you use between the two time slices (2015-2020 and 2095-2100) to inform the ice dynamics model? Was glacier geometry (e.g. surface elevation) updated in COSIPY using information from iSOSIA (if yes, at which time steps?) to account for SMB-elevation feedbacks?*

The Reviewer is correct that as described in the text the models were used in series rather than coupled, we believe that this is adequately described and illustrated in the manuscript and the Reviewer has noted this. The mass balance forcing between time steps has been described in response to the comment from Reviewer 1. We have added the following to Section 2.4 to make this clear:
*"Surface processes within the glacier model then modified the distribution of accumulation and ablation* **but this was not updated in COSIPY**".

6. *Precipitation and glacier volume evolution after 2100: The glacier volume trajectories in Fig. 7B show abrupt changes, particularly under RCP8.5, that appear to correspond to significant changes in precipitation (trends). This raises questions about the plausibility of the long-term forcing. Please include a figure (e.g. in the appendix) showing the evolution of multi-annual or decadal mean temperature and precipitation for the full simulation period (2000–2300) for each RCM/RCP combination. Discuss how reliable these projections are.*

Figure A4 is new and shows the RCM precipitation trends until 2100 CE. As discussed in the original manuscript, projections of precipitation beyond this point are not possible.

***Presentation and Discussion of Findings (Sections 3, 4 and 5)***

1. *I found it difficult to interpret the 30 different 2.5 D visualisation of glacier mass balance, ice thickness and debris thickness in Figs. 4, 5 and 7. I would suggest to show the results for the best-performing RCM (in Fig. 4; move the remaining tiles to the appendix), increase the the size of individual tiles and the legends, use different colour schemes for different variables and add a reference outline (e.g. "present day") in all simulation panels for easier comparison.*
*In addition, a summarising table comparing the performance of different model setups and their agreement with observational data would be very helpful. If feasible, an animation (e.g. a GIF showing the glacier's temporal evolution in terms of mass balance, geometry, and debris cover)*

*could be a valuable supplement and make the results more accessible to both scientific and non-scientific audiences.*

We prefer to keep the multi-panel figures as they are in the original manuscript as this gives the most accessible comparison between the results from different simulations, and note that the figures in the original manuscript are at a low resolution and that the final versions will be a much higher resolution that allows zooming in to each panel. Additionally we prefer to keep the colour scale the same throughout, as this has been chosen specifically to emphasise difference between the highest and lowest values, which is not as easy to view with a gradational scale. As Fig. 4 presents the results of a sensitivity experiment, a reference outline could be misinterpreted; the intention is to show difference between simulations rather than a degree of fit to an outline and as the view is identical in each panel these can be readily compared.

As noted in the original manuscript, the performance of the different glacier model parameterisations were explored as the focus of in Rowan et al. (2021; JGR-ES) and rather than duplicate the detailed results of that paper here, which would greatly increase the length of the current manuscript, we refer instead to this earlier paper. A modified version of the key results figure from Rowan et al. (2021) has been added to the new Appendix B as Fig. B2 to summarise the most relevant results.

2. *One of the central findings is the potential mitigation effect of an projected increase on the long-term glacier mass balance. If this remains a key focus of the manuscript, I would expect a deeper and more critical discussion of (i) how robust these projected precipitation trends are across the different RCMs and (ii) how these findings compare with other studies and what implications they have for our broader understanding of the future evolution of glaciers in this region.*

This is now discussed in more detail in Section 4.1 and the following text has been added:

**"The downscaled future climates were compared with those from other studies using CORDEX results, and showed similar annual and seasonal regional temperature trends strongly linked to the choice of RCP, and similar positive precipitation trends with poor agreement between RCMs (Kaini et al., 2019; Sanjay et al., 2017). The relationship between precipitation and the two future emissions scenarios was less clear than that for air temperatures, because the monsoon-influenced Himalaya shows particularly poor RCM consensus and high levels of uncertainty in future precipitation trends with warming relative to other regions in High Mountain Asia (Sanjay et al., 2017)."**

3. *The importance of sublimation and avalanching for the modelling of the future evolution of Khumbu Glacier is emphasised (Line 478-479). However, the current discussion is too brief. I would encourage the authors to support their claims with quantitative evidence or a figure.*

The effects of avalanching have been stated more clearly in the text and detailed information has been added to Appendix B. See also response to Reviewer 1 above about sublimation.

4. *The comparison with the global glacier modelling results of Rounce et al. (2023) in Fig. 8 is a valuable addition. However, please use a consistent reference date (e.g. 2010) for both your model simulations and the global study. I would also suggest to state mass losses instead of volume losses for easier comparison with other studies (e.g. Kraaijenbrink et al., 2017; Rounce et al., 2023). The 39% difference in projected mass loss for Khumbu Glacier under RCP4.5 between this study and Rounce et al. (2023) is substantial and merits a more thorough discussion. What are the key drivers of this discrepancy - differences in SMB model physics, debris representation, regional precipitation trends, or calibration strategies?*

Done, see response to Reviewer 1 above.

***Additional references that might be of interest for the introduction and discussion***

*Arndt & Schneider (2023): https://doi.org/10.1017/jog.2023.46 => Modelling glacier mass balance and investigating sensitivity to atmospheric forcing in High Mountain Asia*

*Collier et al. (2013): https://doi.org/10.5194/tc-7-779-2013 => Interactive modelling of glacier-atmosphere interactions*

*Collier et al. (2015): https://doi.org/10.5194/tc-9-1617-2015 => Modelling the impact of debris cover on ablation and glacier-atmosphere feedbacks*

*Compagno et al. (2022): https://doi.org/10.5194/tc-16-1697-2022 => Modelling supraglacial debris-cover evolution*
*Temme et al. (2023): https://doi.org/10.5194/tc-17-2343-2023 => Strategies for SMB model calibration (including COSIPY)*
*Zekollari et al. (2017): https://doi.org/10.5194/tc-11-805-2017 => Complete different setting, but insightful simulations and discussion on the potential mitigation impact of increasing precipitation under different warming scenarios (see e.g. Fig. 11)*

We thank the Reviewer for these references and have included Compagno et al. (2022) but not all of the others as we felt that these duplicate what is already cited in the text.