egusphere-2024-863 response to Anonymous Referee #2

We thank anonymous referee #2 (R2) for the encouraging and thorough review of our manuscript with clear recommendations. We have considered our response to each comment carefully, especially to those that require clarification or are critical for which we provide more detailed responses with clear justifications. We are largely in agreement with their recommendations and have, therefore, proposed revisions that we feel will result in an improved manuscript. We have included all of the original referee comments in black. In some cases, where multiple comments can be addressed with one response we've boxed these comments. Our responses are in blue. Any proposed revisions are written with **bold underlined text**. We look forward to the response from the editor at their earliest convenience.

In this study, the authors present a framework to sequentially couple the ice dynamical part of the Open Global Glacier Model (OGGM) with the full energy balance model for snow and ice from the Joint-UK Land Environment Simulator (JULES). The authors apply this sequential (offline) coupling to 500 glaciers in the tropical Andes and make projections of glacier mass loss until 2100 for different RCP scenarios. They conclude that under RCP4.5 17% of the ice mass will still remain by 2100 which is more than what other glacier modelling studies have predicted e.g., compared to 2% as predicted by GloGEM (Huss and Hock 2015) and Marzeion et al. 2012.

The authors present a very unique and clever workflow which consists of running both models separately; JULES is in charge of computing the annual specific mass balance over discrete points in the study domain. Thus, this model comes up with different relationships of surface mass balance as a function of height per year $SMB(zi)t$ for each glacier. OGGM then extracts the annual specific mass balance at a particular location, elevation and time. The ice dynamical flowline module of OGGM then inverts for the ice thickness via mass conservation and the Shallow Ice approximation (SIA). For the glacier evolution OGGM solves the continuity equation with the updated SMB distribution given by JULES.

The authors only calibrate parameters within JULES full energy balance model. Ice dynamical parameters in OGGM are not calibrated. JULES SMB calibration consists of several steps where JULES parameters are modified to achieve the best fit to geodetic mass balance data for a benchmark of 30 glaciers; then parameters are extrapolated to other glaciers within small subregions.

Overall, the manuscript is well written and the methods and discussion section has a clear narrative and description of model experiments. It is clear that the authors have put a lot of effort in model calibration and evaluation of SMB. However, some parts of manuscript lack order and could be re-arranged slightly to enhance the impact of the results and discussion section. There are certain aspects of the methods that require clarification and the discussion section neglects to highlight certain limitations of not updating changes in glacier geometry in JULES simulations. Also, the authors do not asses the implications of not calibrating ice dynamical parameters in their simulations.

I will definitely recommend the publication of the manuscript after the authors clarify some of my questions below and make some changes to the manuscript. I also recommend below how the authors could evaluate their framework limitations regarding ice dynamics.

Major comments:

- The JULES model is unaware of changes in the glacier hypsometry through time i.e., JULES is not aware of glacier retreat simulated by OGGM. Glacier retreat might affect the $SMB(zi)t$ relationships that JULES outputs; if at a specific height zi and location that OGGM node becomes ice-free in a given year. Authors argue that changes in glacier hypsometry through time are accounted by lowering the ice surface and feeding into OGGM an SMB at a lower elevation. How this is decided between timespans and how authors deal with the transition between ice and ice-free areas is not clear. For example, given two points along the flowline, the $SMB(zi)t$ relationship might not hold if the second point (at a lower elevation) becomes an icefree area. Given that ice free physics are qualitatively different than ice covered areas the interpolation might no longer hold. Limitations regarding this issue are not explained by the authors and this should be clarified in the explanation of step 2 in section 2.4.3. and the discussion part.

We completely agree, this is indeed a limitation of the approach that has the potential to introduce discrepancies in surface mass balance for ice free nodes in OGGM and we did not outline this in the text. OGGM updates the glacier geometry on an annual basis. For ice free nodes, it considers ice flowing into the node upstream (when the glacier is growing) and the surface mass balance. So, these discrepancies have the potential to influence the evolution of the glacier extent over time. If we refer to the JULES model used in this study (always ice covered) as $\text{JULES}_{\text{ICE}}$ and the hypothetical JULES model that is aware of hypsometry changes as $\text{JULES}_{\text{HYP}}$, then we can define where discrepancies between the two models and resultant changes in the ice geometry are likely to be most pronounced. For a given annual timestep, this will be on ice free nodes in OGGM where:

1) One of the models simulates a positive annual mass balance while the other simulates a negative or neutral mass balance
2) Both $\text{JULES}_{\text{ICE}}$ and $\text{JULES}_{\text{HYP}}$ simulate a positive annual mass balance, but of different magnitude
3) Where the glacier is growing (i.e. ice is flowing in from upstream) and where the simulated annual surface mass balance of $\text{JULES}_{\text{ICE}}$ and $\text{JULES}_{\text{HYP}}$ are different

In the fourth scenario, where the glacier is not advancing and where both models simulate a neutral or negative mass balance, $\text{JULES}_{\text{ICE}}$ and $\text{JULES}_{\text{HYP}}$ will have no discernible effect on the simulations as both would result in the continuation of an ice-free surface in OGGM.

Given the above and the dominant negative mass balance of the glaciers over the simulation period, we would argue that discrepancies introduced by this limitation are likely to be small in this study. **Regardless, we agree that it is important to highlight this limitation in the text, particularly for readers who may be considering applying the approach to their own case studies. We will do so in the revised manuscript**.

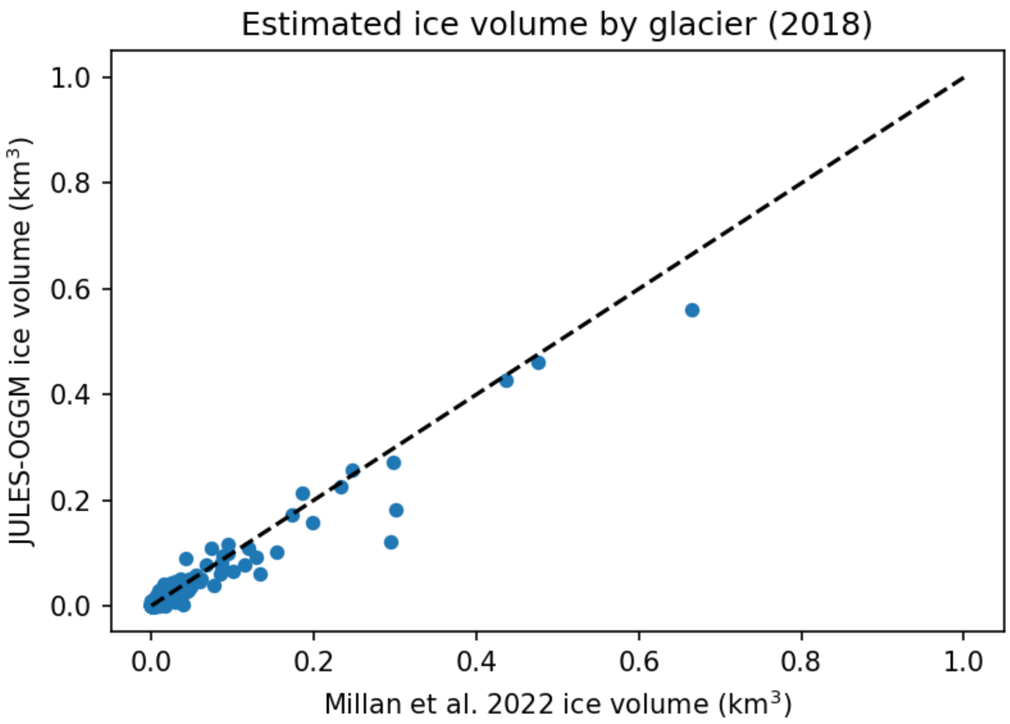
- The simulated glacier geometry that results from the interaction between mass balance and ice flow processes should be compared to ice thickness or volume observations. This is important as the initial glacier state can have a large impact on the simulated glacier evolution (Zekollari, et al. 2022). Authors do not validate the initial ice thickness distribution obtained with their JULES-OGGM framework before doing future simulations. They only evaluate the model by comparing simulated SMB to geodetic and in situ SMB observations.

There might not be in situ glacier thickness measurements for these glaciers, however, the authors could check how their simulated initial ice volumes or ice thickness distributions compare to those from Millan et al (2022) at least for glacier wide volume estimates. Millan et al (2022) is a satellite derived ice thickness product and uses, like OGGM, mass conservation and the SIA, these are not in situ ice thickness observations but if compared to model initial model simulations could provide an idea of the calibration error for the ice dynamic part of the workflow. In other words, errors derived by not calibrating ice dynamical parameters in OGGM or by not updating glacier geometry changes in JULES. By looking at Millan et al (2022) Figure 3b seems to be data available for C Vilcanota glaciers. I encourage the authors to make such a comparison as this could strength the findings of the manuscript.

We really like the idea of having an independent source of validation for the glacier geometry. It's worth noting that, for the initial ice mass at least (year 2000), we show that, cumulated over the VUB, our estimate sits between estimates used for GloGEM and MAR2012 as part of the GlacierMIP experiments (Figure C1 c&d). **We don't make this point in this text and so, will do so in the revised manuscript**. Having looked at the Millan et al. (2022) study, this looks like a promising basis to do a glacier-specific validation. Having read the manuscript in detail, we would highlight several important considerations of using these data:

1) Their ice thickness estimates would not permit the validation of the initial ice geometry specifically as suggested by the referee. This is because their calculations are derived from maps of ice velocity are estimated from the years 2017-2018. Our simulations are initialised for the year 2000.
2) They note that the "temporal mismatch" between the ice velocity product, digital elevation model and glacier outlines are important sources of uncertainty which can't easily be quantified. An important component of this is the glacier outlines. They use the same outlines as our initial glacier area (for the year 1998).
3) Errors in ice thickness for thicknesses below 100 m (this accounts for 95% of the glacierised area in the VUB according to this study) are assumed to be >50%.

While there are undoubtedly limitations of using these data, they are the current state-of-the-art and, therefore warrant being used in our analysis. We were keen to conduct this analysis, even before submitting our revisions, and so have done so considering this suggestion. In the figure below, the Millan et al. (2022) ice volume data are plotted against the JULES-OGGM simulation for 2018 for all glaciers. As we note above in limitation 1, we can't use these the validate the initial geometry specifically, but this comparison does implicitly validate this as well as the dynamic behaviour of the model over the 2000-2018 simulation period. Most simulations sit close to the 1:1 relationship (black dashed line). The total JULES-OGGM ice volume for glaciers where Millan data are available (not all glaciers in the VUB) is 7.19 km^3 whereas Millan et al. (2022) estimate it as 8.16 km^3. The lower estimated volume is perhaps not surprising, especially due to much older glacier outlines used to estimate these. **We will include this analysis and discussion of the limitations of the data and modelling approach in the discussion.**



Minor changes:

Abstract:

L26-27: "We conclude that this inhibits the robustness of extrapolating the JULES parameters across multiple glaciers". I will remove this from the abstract because this is true for any glacier model, parameters describing specific aspects of each glacier can't be extrapolated to other glaciers. See calibrations sections from Marzeion et. al. (2012) and Zekollari, et al. (2022).

Agreed, **we'll remove this in the revised manuscript.**

Authors should highlight the societal importance of their work in the Abstract and how important this region is for water availability.

Nice idea. **We'll add a sentence on this in the abstract of the revised manuscript.**

Introduction

L44. Replace The is … with "That is especially true in…"

A typo! **"The" should be replaced with "This". We will revise this.**

L49. "Sublimation" add citation and definition as this is the first time the authors mention the concept.

We feel that for the readership of this journal, the term "sublimation" is well-known and so feel that addition a citation and definition would provide additional, unnecessary wording to a manuscript which is already quite substantial.

L49-50: "sublimation can account for the majority of energy consumed for ablation" change to "can reduce the energy available for melting" (clearer and in line with Winker et al. 2009).

We agree, this is clearer. **We'll change this in the revised manuscript.**

Add at the end of the introduction the key takeaways from coupling these two models.

We're not entirely sure what this comment is suggesting exactly and so would request further clarification from the referee on this point.

Methods

My main suggestion here is to change the outline in the following way:

2.1 Study Area. This section should be moved and integrated into the Introduction, here authors could make the point that a lot of people depend on these water resources (see Millan, et al. 2022 figure 3b) and given the little knowledge surrounding this area, the authors work is highly important.

We appreciate the referee's desire for us to include some context with regards to the significance of the study basin for water resources in the introduction. We agree and will **add a brief description of this in the introduction where we first introduce the Vilcanota-Urubamba basin (line 91).**

We've considered the referee's request to put the study basin section in the introduction, but do not feel that this will improve the manuscript scientifically or in terms of its readability. In fact, with regards to the latter, we feel that it is much more desirable to have a separate study basin section that the reader can easily refer to when considering the results of the study. Therefore, on balance, we would prefer not to change this in the revised manuscript.

Sections 2.2 – 2.3.

Should be a new section called: 2. Input data and pre-processing. There authors should explain all the data input used in the model and the pre-processing stages needed to ingest the data into JULES-OGGM workflow. E.g., The authors used their own Glacier inventory. The processing of these outlines and why they choose those instead of the Randolph Glacier Inventory could be specified in a subsection e.g., 2.1. Glacier outlines. With this format authors could also expand into the analysis done to the climate input data.

2.3 Glacier Mass Balance data: Specify the advantages of Dussaillant et al (2019) vs Hugonnet, R and others (2021)? Most glacier modelling studies use Hugonnet, R and others (2021) to calibrate parameters in the glacier mass balance. E.g. Rounce et al. 2023.

2.4 JULES – OGGM glacier modelling workflow

Here I think authors could make a simple change to make the outline of the methods more organised:

3. Methods: glacier modelling workflow

3.1 JULES

3.2 OGGM

3.3 Sequential (offline) coupling of JULES-OGGM

This change will make it clear that the coupling is sequential and that both models are not fully coupled but one feeds input to the other.

We really appreciate the referee's thoughts on the structure of the methods section. We put a lot of effort into structing the manuscript in a way that would gradually introduce the reader to the various aspects of the data, tools and methods used – a difficult task given the number of novelties that we include! We appreciate that this approach has resulted in a methodology section discusses data, processing and models interchangeably within the same sections which may be confusing. We also appreciate that there is some repetition in having "JULES" and "OGGM" subsections in both section 2.4 and 2.5 and the above suggestions highlight this to us further. We like the referee's suggestion of keeping the data and models separate for example. While it is hard to commit to the exact changes suggested by the referee above, **we are more than happy to commit to reviewing our methodology section structure with the aim of streamlining it and making it easier to digest.**

L179. Replace "they" with Shanon et. al (2017).

Good idea, **we'll do this.**

L194. Replace "the flowline model" with "ice dynamical flowline model"

Agree, **we'll change this in the revised manuscript.**

L194. Replace ": in effect," with "– i.e.," … L199. Point the reader to the Figure 3a.

Agree, **we'll do this.**

L202-203. Climate data pre-processing details could be moved to the input data and pre-processing section.

As we state above, **we'll review our methodology section structuring and will include this in our review.**

L252. Add citation for the Randolph Glacier Inventory (RGI). Move that part of the workflow to the Input data and pre-processing section (e.g., add a Glacier outlines section).

As we state above, **we'll review our methodology section structuring and will include this in our review.**

Here the authors should mention the implications of not using the RGI, which limits the comparison of JULES-OGGM results with previous model estimates that use the RGI. e.g., Li, F., et al. (2023) found that in High Mountain Asia projected mass loss differences between inventories are higher than between adjacent emission scenarios, illustrating the vital importance of high-quality inventories.

Yes, we agree, the glacier inventory is very important which is why we chose to use one which we consider to be superior to the RGI. In terms of the implications of not using the RGI. The main one is, as you mention, the difficulty in comparing our simulations to other studies. We would like to point out that we do state this in the discussion (lines 526-527), but we appreciate that **we could emphasise this point further and refer to the Li paper cited. We will do this in the revised manuscript.**

This might not be as relevant for the Andes but having an idea of how different they are in total area coverage per glaciers should justify why authors do not use the RGI when comparing JULES-OGGM results to GloGEM and Marzeion et al. 2012 in Appendix C1.

Authors should add a calculation of how different the Area coverage per glacier is between the RGI and their inventory of choice.

We completely agree, we don't have the exact numbers off the top of our heads, but if you look at Figure C1 a, you can see that GloGEM and MAR2012 have an initial area of approximately 250 km^2 whereas our initial area is closer to 180 km^2: A significant difference! **We will add this into the revised manuscript.**

L257 When describing OGGM equilibrium assumption. Authors should specify the OGGM version used in their study. (e.g., v.1.4) as that assumption is not required by OGGM in the latest version (see model updates and documentation).

Yes, we specify the version number before this on line 208. **We'll make it clear that this assumption is tied to this version.**

L268 "while the default parameters for the ice flow component of OGGM were used." Add the implications of this in the discussion and limitations, point to that section.

Yes, we state this as a limitation in the discussion (lines 479-481), but we appreciate that we could have expanded on this more and point to ways forward in addressing this such as the "dynamic spinup" approach implemented in the latest version of OGGM that allows one to better constrain the creep parameter in OGGM. **We will expand on this in the revised manuscript.**

L271 "except for the temperature lapse rate" Add (See Appendix A).

We've already referred the reader to Appendix A by this point in the text so we don't see any need to include this.

L280. Add citation to the Monte Carlo framework used and add citations for other studies which have used the same strategy.

We understand the referee's desire to see a citation here, but we think this might be because we're using the term "framework". On reflection, this was the wrong word to use. We didn't implement a framework that's been previously developed and citable. Rather, we would have been better to say that we used the concept of Monte Carlo i.e. repeated random sampling, to establish the optimal parameter set. While we have tried to be transparent about what we did exactly, e.g. by stating that we took *"random perturbations"* of our parameters and assessed the simulations for goodness of fit and used the *"quasi-random Sobol sampling strategy (Bratley and Fox, 1988) to sample the parameter space efficiently"*. A key piece of information that we missed was that we sampled the parameters from a uniform distribution i.e. we made no assumption about "priors" in the parameter space. These pieces of information sum up our method (or framework as we wrongly refer it as). **We will reword this accordingly and add the extra level of detail about how we sampled the parameter space in the revised manuscript.**

L294. "Used across environmental modelling applications". Add citations.

Sure, **we will add several citations here.**

L298. Add safepython library citation, version used.

Good idea, **we'll add this.**

Results

L307-308. Fig 4a shows a perfect correlation, I wonder if multiple parameter combinations could achieve the same thing?

Almost certainly, yes! But to be clear, the point of this is not to identify equifinality (there are some nice recent examples of studies that aim to do this such as Schuster et al., 2023), it is to evaluate how the optimal efficiency of the model changes as you regionalise the parameterisation.

Model Evaluation

L347. The authors could enhance the discussion and provide an explanation from where the errors come from. This could be down to several limitations in their approach:

i) The JULES full energy balance model is not aware of changes in glaciers hypsometry through time. Thus, the SMB(z) relationships do not incorporate well ice dynamical feedback from OGGM. In other words, lowering the ice surface might not be a realistic representation of glacier retreat.

ii) the ice dynamical parameters in OGGM are not calibrated per glacier. Is it known that ice thickness (and ice volume) decreases as a function of the A factor; thus, perhaps the default parameter in OGGM might not be the right value for these glaciers? See Maussion et al. (2019).

iii) A comparison between the ice thickness obtained with JULES-OGGM vs Glacier thickness observations (or satellite derived thickness like Millan et al. 2022) should be made for a benchmark of glaciers to assess errors of using a default A and default sliding parameter (if sliding is also activated in OGGM).

Yes, this is also something that was picked up by referee #1. They also highlighted possible errors introduced due to deficiencies in the driving climate data which would be an additional point to consider here. As we stated in our response to referee #1, we have consciously remained ambiguous on the exact sources of deficiencies in the model and reasons for differences compared to GloGEM and MAR2012 models as the reasons are likely to be multifaceted. **But we appreciate that we could have expanded on some of these aspects in the discussion and will do so in the revised manuscript.**

L377-378. Authors could expand their conclusions and use these results to suggest better approaches to simulate albedo in JULES. And expand the very short conclusions.

We'd be happy to point to some recent advances in albedo modelling, especially using land surface models like JULES e.g. see the recent work of Hao et al. (2022). I hope the referee will appreciate, though, that we will probably refrain from saying that these are necessarily better. We'd need to experiment with this to find out first! **We'll add this to the discussion and conclusions.**

Discussion

The authors should consider validating their initial glacier state (see Major comments).

This is covered in our response in the major comments section.

L483-484. This is an important find and should be included in the abstract.

Agreed, **this will be included in the revised manuscript.**

L486. “wght_alb”. Give the full parameter name then refer to the abbreviation in (). Check this throughout the manuscript.

We have provided a description of all parameters with their abbreviations in Table 1 (page 12). But we appreciate that, given that this is quite early in the manuscript, it may help to remind the in the discussion. **We will do this in the revised manuscript.**

L490-493. Again, another good find that should be highlighted in the Abstract, Introduction and Conclusions.

We don’t agree that findings should go in the introduction for obvious reasons, but we can see the merit of **including this in the abstract and conclusions and will do so in the revised manuscript.**

Figures and Tables

Figure 1. It will be nice if in this Figure the authors could add the resolution of JULES grid.

We agree, this would be nice, but unfortunately, not practical. As we outline in the methodology (and show diagrammatically in Figure 3), each glacier has 10 JULES grid box nodes. The proportion of the glacier that each one of those nodes represents is variable (both in space and time – so not static) and in actual fact, the majority of the glacier surface is driven with simulations interpolated between these grid nodes. We hope that the referee can understand why this would make visualising the JULES “grid” extremely difficult. For this reason, we have chosen not to try and include the JULES grid on this figure.

Table 2. Add in the table caption each parameter name description and units.

The units are already in the table headings and the descriptions are in Table 1. Rathe than re-write the descriptions in the caption, **we’ll refer the reader to Table 1.**

Figure 4. Add error bars to scatter plots with confidence interval.

Figure 6. Add error bars to scatter plot with confidence interval.

We were also very keen to include error bars on these plots and did put considerable effort into assessing the feasibility of this. We’ll explain why we chose not to.

The Dussailant et al. (2019) data provide error estimates on a glacier-by-glacier basis based on the RGI outlines. As we mentioned, we used a different set of glacier outlines (Drenkhan et al. 2018), with a better representation of the real ice area and a different discretisation of the ice-covered areas into individual glaciers (the RGI has a total of X glaciers in the VUB, while we have 532). The glacier-by-glacier Dussailant et al. (2019) error estimates can therefore not be used directly to provide error estimates.

The differences in glacier extents also meant that we could not use the glacier-by-glacier mass balance stats from Dussailant et al. (2019). As we outline in the manuscript, we extracted the mass balance estimates from the 30 m gridded data, also provided by Dussailant et al. (2019). They also provide error estimates for each of these grid points in the form of standard deviations. These are useful for assessing errors at individual grid points, but cannot easily be aggregated to estimate errors over specific regions as they would require the assumption of conformity to a formal statistical distribution (e.g. gaussian) across all grid points and inter-grid-point independence. Both assumptions cannot be satisfied. The only way to calculate the error bounds robustly would be through deriving them directly form the raw analysis data (like the authors do in their paper). This would be considerable work and far beyond the scope of our study.

Figure 8. Maybe it is more intuitive for the reader if the authors visualize % of Area change or % of Mass change (e.g., Rounce et al. 2023)

Perhaps for some readers, but this is quite subjective and not fundamental to findings of our study. It's also easy for a reader to calculate this from the graph if they want to and we include % change in our take home statistics in the text. For these reasons, we prefer to keep this graph as is.

Figure 10. Is too small and very hard to read. I suggest perhaps dividing this in to two figures: Fig 10 for Mean elevation and Fig 11 for Energy Balance.

Yes, this was also picked up by reviewer #1. **We'll address this in the revised manuscript.**

Appendix C. Specify what are the implications of comparing this study to GloGEM and MAR2012 if this study uses a different glacier inventory, thus a different initial glacier area. Add initial glacier area coverage by each study.

Agree, we've addressed both of these points in our responses above.

Appendix Figure E. Same problem as Figure 10.

As per response regarding Figure 10 above.

Code availability

Specify the OGGM version used in this study and provide a zenodo doi for that version. See OGGM documentation on how to cite the model. https://docs.oggm.org/en/latest/citing-oggm.html Provide versions and citations for any other python code or tools used.

We agree, **we'll add this information to the revised manuscript.**

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