Dear Editors, dear reviewer,
We thank the reviewer for the insightful comments that greatly helped to rive our manuscript. Please find below the reviewer's comments in *black italics* and our answers in blue.

Sincerely,
Coline AriaGno (on behalf of all co-authors)

Summary: Ariano et al. explores how landslides influence landscapes evolution using numerical modelling in combination with topographic analyses. Specifically, they focus on three catchments in the western Alps that exhibit different morphologies (fluvial vs. glacial), to assess how landslide activity and erosion vary spatially and over time depending on the pre-existing landscape. Indeed, the different catchments represent a gradient in glacial imprint and deglaciation timing, allowing these regions to be used as a natural laboratory. The main objective is thus to predict and explore landslide activity and its role in transient landscape evolution during interglacial periods.
In general, this is a well-illustrated paper and well-written, with appropriate references. The scope of the study is well thought out, and I believe the results will be of interest to the wider geomorphological community. However, I do feel that the discussion could use some work (made easier to follow), to clearly communicate the implications of this work to the scientific community.
Particularly, I found that the main hypothesis of the paper is not defined consistently throughout the paper:
1: Line 143-145: "Our working hypothesis is that the different morphological signatures observed for Alpine catchments are evidencing both landslide activity and deglaciation timing."; So, the scope is to test that landscapes today are a result of both glacial erosion and landslide activity (as well as other processes). Very clear and feasible, and I believe this is indeed shown by the results.
2: Line 559: "Our landscape evolution model […] has been designed to explore the hypothesis that landsliding represent a dominant geomorphological agent during postglacial periods."; Sort of similar to the first instance; landslides are important. Clear.
3: Line 665: "our initial hypothesis about the capacity of landslides to erase this glacial topographic inheritance over the last post-glacial period". So, here you state that the hypothesis is to test whether landslides can erase the glacial imprint over an interglacial cycle. This is very different from other two instances, and in my opinion much more difficult to test.
4: Line 704-706: "Here we discuss our initial hypothesis, that all the studied catchments had the same glacial topographic imprint, and show that the three catchments have a distinct erosion dynamics explained by diachronous landslide activity following different glacial retreat times". Again, this is different, and I am not sure I agree that you can be sure the three catchments all started out with an equal glacial imprint, give their differences e.g., in glacial duration (you do comment on this in the end).
In any case, my point would be that the hypothesis (or hypotheses) to be tested should be clear throughout the manuscript, and more care should be put in developing the rationale behind the 3. and 4. points listed here. These arguments are not trivial, and you are sort of cutting some corners by referring back to a hypothesis (that was stated differently initially).
Thank you for these insightful comments, also raised by the second reviewer. We have made some changes and clarified the hypotheses in the introduction and throughout the article (Lines 149). Specifically, we have retained the main hypotheses exposed in the introduction and recalled at the beginning of the discussion, but have rephrased the points 3 and 4 above for clarity.

Can you really infer from this study that the initial glacial landscape of the 'fluvial' catchment has been erased by landslide activity, whereas the others are still ongoing? If the upper catchments will not transition into fluvial catchments over timescales of 100 kyr, how has the lower catchment managed to do the transition already? You come to this in the end of section 5.2, but I really think the argumentation for this point should be outlined much more clearly throughout. And it should be clearly stated where you land in terms of your initial hypothesis. You could also present these ideas with more caution: "If the three catchments had the same glacial topographic imprint initially…".

As you and the other reviewer suggested, we clarified our hypothesis (lines 149): Considering that the three catchments have a similar initial glacial inheritance, the fluvial (Pisse) catchment has already been strongly influenced by hillslopes processes, as testified by V-shaped valley in its lower section (fig 2). However, the 100-kyr simulation is not sufficient to induce similar changes (transition from U- to V-shaped valley) in the glacial (Pilatte) and intermediate (Etage) catchments, which means that a mechanistic approach, such as Hylands' model, cannot alone explain the fluvial topography of the fluvial (Pisse) catchment. That is why we argued in section 5.2 that the glacial shaping (erosion power) or/and the shorter glaciation duration in the fluvial (Pisse) catchment have an significant impact on the topographic evolution of the catchment during our simulations.

Thus, we do not infer that the initial glacial landscape of the 'fluvial' catchment has been erased by landslides activity. Instead, we argue that this actual topography is possibly a combination of a less intense glacial inheritance and longer/more active hillslope processes. We have modified the manuscript accordingly in section 5.2.

*Another smaller comment I have relates to the time scale of the model simulations. The **100-kyr duration** of the models seems a bit odd, i.e., to simulate landscape evolution over such a duration without considering glacial changes. It also seems a bit unnecessary given the overall scope, focused on interglacial timescales. Perhaps this could be justified (or the rationale behind could be explained), for instance by including in the discussion some reflections of how results would differ/be limited given a different choice. Right now, it is simply stated as a fact in the manuscript (line 615-616).*

We agree with the reviewer that 100 kyr is not entirely realistic as a timescale for investigating the post-glacial period. However, this was raised during our modeling study as is an interesting and necessary duration which enables 1) to obtain clear temporal trends of erosion rates, smoothing out landslide variability at the onset of our simulations, and 2) to quantify the duration theoretically required to erase the glacial imprints and to reach stable hillslopes. We now mention this in the discussion section on line 638.

*In addition, I think the discussion lacks some perspective related to the fact that the used **(present-day) topography** has already been influenced by these processes since glacial retreat (i.e., the DEM has already been affected by these processes to some extent – and given your conclusions potentially to a large extent!). Could this suggest that landslide erosion rates would have been even bigger between glacial retreat and now? Specifically, this study predicts a pulse in erosion rate by landslides over a few thousand years. Would this pulse already be done in the real world? Or has it simply been even bigger prior to today? Could such a trend be extrapolated back in time based on the presented results? Reflections on these questions could be added in the discussion.*

Indeed, the initial topography we use in our models has already been influenced by post-glacial hillslope processes. Considering the results we have, landslide activity since the deglaciation should have been at least equal to the one we observe at the beginning of our simulation. It is

however difficult to extrapolate further as the deglaciation time and the onset of post-glacial hillslope activity is asynchronous between our catchments but also within each catchment: lower areas having been deglaciated earlier than higher areas. Moreover, we do not consider the role of permafrost and its retreat, which is likely to have a large in controlling the intensity and timing of post-glacial landslide activity. It would, in turn, be interesting to investigate a possible delay between glacial retreat and landslide activity as modulated by the rate and timing of permafrost degradation.
We have added a few sentences in the discussion to mention this point and to add some perspectives on the influence of the chosen DEMs (modern topographies) used in our model (section 5.1.2, Line 645).

Finally, I have listed several comments and suggestions below that will hopefully be useful when making the final adjustments of the manuscript. In addition, I suggest going through the manuscript to do a final check of language (e.g., lines 152, 344-345, 444, 503, etc.) and consistency in reference style (e.g., line 51, 72, 351, 580 also using 'e.g.,' instead of 'e.g.', etc.).
We have checked the manuscript and language mistakes as suggested, and thank the reviewer for all the useful suggestions.
Lines 28-32: the mentioning of 'the glacial buzzsaw' might need a little more elaboration. It reads as if the glacial buzzsaw is usually attributed to a decrease in unstable slopes as well as a lowering of maximum topography. But is the mechanism in the glacial buzzsaw not that glaciers increase the steepness of their headwall slopes (i.e., increase in unstable slopes), such that hillslope processes are more active, and therefore by extension reducing the maximum elevation of a catchment? E.g., to quote one of the defining papers: Mitchell and Montgomery, 2006: "The summit altitudes are set by a combination of higher rates of glacial and paraglacial erosion above the ELA and enhanced hillslope processes due to the creation of steep topography."
It is true that this sentence is a bit ambiguous. To not overload the abstract, we decided to cut this mention of the glacial buzzsaw and glacial processes, as it is well developed and explained in the discussion of the manuscript (section 5.3.2).

Line 76: an uncovered landscape or uncovered landscapes. Corrected.

Lines 77-78: consider if this sentence should also be past tense. Changed as suggested.

Lines 84-85: "leading to a postglacial increase on both the frequency and intensity of hillslope events through time". I understand that the frequency and intensity go up as the regions deglaciates, but is the point not that it then decrease through time hereafter?
This sentence was not completely clear and the end part could be misleading. This has been rephrased for clarity.

Lines 98-100: maybe start with a 'while' Done.

Lines 134-138: I would mention the stochastic nature of the model here already. Done.

Line 152: 'Three'. Removed as suggested.

Line 181: I suggest consistency using 'three' versus '3'. I would suggest 'three'. Done.

Lines 205-211: you could consider also citing the new paper by Maxime Bernard here (see below). Reference added.

Line 222: why V+ ?. This was an error and has been corrected to “V-shaped”.

Line 263-264: maybe this is the tradition when concerned with the used model. But I find it odd to refer to hillslope height, when talking about elevation/height change between two cells. Would maximum stable slope not be more appropriate?
Here, we have decided to retain the terminology used in the original framework of the Hylands model, which is widely referenced in the manuscript (Campforts et al., 2020).

Line 278: perhaps ‘the erosion scar generates a failure plan’ can be formulated more precisely. Is the erosion scar not generated by the failure and not vice versa?
Modification done.

Lines 282-284: this was not completely clear to me – i.e., whether all DEM cells in the entire catchment above a certain plane would be considered unstable for one specific landslide event?
Starting from the trigger cell, the model identifies a failure plan upstream (following the Culmann angle) and all the cells above this plan are eroded. The roughness and the topographical irregularities of the terrain naturally limit the extent of the landslide.

Line 286: is it necessary to introduce Ff here, when not elaborated further? One could simply state that ‘in this setup, all sediments are instantaneously evacuated.’
As suggested, we have been more synthetic in this part and we also have moved the details of the simulation (section 5.1.2), with a different *Ff value*, in the supplementary.

Lines 292-324: I would suggest simply to incorporate this section in the Model calibration section 3.3. I see no reason for dividing this into two distinct sections. For instance, the first part of section 3.3.1 gives info/context relevant to the text in 3.2 and right now you refer to back and forth between the sections several times. In addition, I found lines 314-324 difficult to follow. If parameters give rise to few landslides, how do you then generate a large amount of landslides? Maybe it is the ‘Then’ that leads to confusion. Should it be ‘Either we compile multiple simulations … or we reduced the return time…’. But still, it could be clarified how you calibrate t_{LS} while scaling this parameter.
The subsection 3.2 is not included in subsection 3.3 because it describes the procedure we used for the overall modelling, while subsection 3.3 details the specific calibration of the model for our simulations. We agree that these sections are closely related, which is why they are included in the same section 3.
We have modified the ambiguous part of this paragraph and made also some changes in subsection 3.1.1 for clarity in the presented parameters.

*Line 344-345: *did* not display any clear rollover.* Done.

Line 346: I guess this is not a matter of visualization but representation. Capitalize ‘we’ or perhaps add. ‘However, we’
As the second reviewer suggested too, we added a connector at the beginning of the sentence.

Line 350-353: -2.3 is larger than -2.5 ;-). Done as suggested.

Lines 397-401: I would expect that the listed three combinations are part of a whole envelope of realistic parameter combinations, where the listed are just some examples. I would highlight that instead of listing specific values explicitly. Also, it would be nice to see an example of the

spatial/temporal patterns in landslide activity for this selected catchment, perhaps for a few 'end-member simulations', showing the variability possible within reasonable values of parameter values (friction angle, cohesion, return time; e.g., supplementary figure, particularly if they are not very different – but that would be a point in itself).
We indeed chose the intermediate combination, among acceptable range of values for each parameter, to further develop our study and simulations. We modified the main text to be less specific about these examples of possible parameter combinations.
Moreover, as you rightly suggested, we ran new simulations with 'end-member parameters'; i.e. one with low parameter values (C= 20 kPa, t_{LS}= 50 kyr) and another with high parameters values (C= 100 kPa, t_{LS} =250 kyr) and added the associated figures in the supplementary.

Lines 425-427: "landsliding results in homogeneous slopes which only slightly exceeds the internal angle of friction (i.e., 0.7, represented by white color in Fig. 6).". Seems like there are plenty of red colors still? Or do you mean only the regions associated with landslide activity? Again, would be nice with additional panels showing initial and/or changes in slope compared to initial values. Perhaps also comment on the high-slope regions that do not experience landslides.
Indeed, this sentence refers to the regions associated with landslide activity. The model does not erase all the steep slopes as it is stochastic. Please note that the color scale was different between the initial and final slope maps (Figs 1 & 6). We thougth that it was more relevant to show the initial slope in the presentation of the study area (Fig. 1) but that it would be redundant to show it again in Figure 6.

Lines 452-460. For consistency, I would suggest referencing all figures when referring generally to all catchments, e.g., not only fig. 8 but also the corresponding supp. figs.
Done as suggested.

Line 483: it seems a bit arbitrary with the selected elevation range of 2400-2800 m, to capture the minimum for both catchments. Why not simply specify a different elevation for each catchment? The large range makes it difficult to see in the left panels that there are fewer red dots in that interval (as it is so wide) – particularly for the glacial catchment.
The selected elevation range is linked to the fewer predicted landslides in the Etages (intermediate) catchment. Further, we relate this elevation range to glacial morphology. This elevation range refers to an area influenced by glaciation, which is indeed quite large due to multiple glacial cycles and ELA oscillations between glacial and interglacial periods. We think this is important to keep the same elevation range between the catchments since they experienced similar climate forcing. As it is not easy to show the quantity of landslides in the left panels, we have highlighted the density in the histogram in the right panels.

Lines 487: be careful using the word 'observations' in connection with model predictions.
Thanks for this suggestion, this has been corrected.

Line 493-495: it is unclear how this is evident from Fig. S5. Should be S4, I assume. Also reference to Fig. 7G-H in the next sentence is unclear.
Same comment was raised by reviewer 2, we made the changes for clarity.

Line 569: I would suggest specifying rock uplift and sediment transport already in title and throughout. Done as suggested.

Lines 584-586: the language of this bit is unclear to me.

We rephrased the sentence for clarity: "However, despite these limitations, we believe that our modeling approach stays appropriate to assess the hillslope stability over 100-kyr timescales, which is largely dependent on climatically-shaped alpine topography and bedrock mechanical strength."

Line 593: return time of 150 kyr? Yes, this model parameterization is explained in the section 3.3.3.

Lines 588-596: I am not sure I understand the rationale behind the need for a model with an average erosion rate of 2-3 mm/yr to compare with the catchment-averaged erosion rate of 1 mm/yr. Do you want to imply that the ***hillslope erosion rate needs to be higher that the average because other parts of the catchment have lower values?*** *Or is it because you are interested in the predicted longer-term erosion rate to be closer to 1 mm/yr? This is not clear from the text, and then why specifically 2-3 mm/yr was chosen? This would also rely on the assumptions you make about what has happened in the catchments since deglaciation until now (since you use present-day topography that have experienced many landslides already), which is what is reflected in the cosmo-derived rate.*
This point has also been raised by reviewer 2, we rephrased the sentence for clarity and to argue for the supplementary test with higher erosion rates: "Considering effective sediment connectivity in the catchment (in our study area, main fluvial valleys are sediment bypass areas without significant incision but potential transient storage) and only landsliding to derive our catchment erosion rate, 1 mm/yr is likely to be an end member minimum value for our simulations."

Lines 672-674: I don't follow the argument here; can you be certain that the 'glacial' vs. 'fluvial' catchments are due to reshaping through hillslope processes? Given the much lower hillslope activity in the fluvial catchment, could this catchment simply have experiences less glacial modification in the first place? The intended message of this section in general is a bit difficult to follow (lines 668-682), could it be spelled out more clearly?
Indeed, the fluvial catchment has potentially experienced less glacial erosion and we come to this point in detail during the discussion (5.2.2), once all the arguments from the results have been gathered. However, the V-shaped valley we observed (Fig. 2) implies hillslope and fluvial activity in this catchment, which can be the result of postglacial reshaping of the topography. We slightly modified the paragraph and hope that it will be clearer for the reviewer.

Line 736: starting a new paragraph with 'this observation' is somewhat unclear. Please specify what 'this observation' refers to. Done we rephrased as suggested.

Section 5.3.1: this section is somewhat short and could potentially be included elsewhere. In addition, there is some discrepancy here related to other parts on the manuscript – arguing that U-Shaped valleys takes multiple glacial cycles to form, while other parts of the manuscript seem to suggest that the 'fluvial' catchment has transitioned from glacial to fluvial during one deglaciation. This will likely be sorted out/become clear if the hypothesis of the paper will be clarified.
Section 5.3 reflects on the topographic evolution of mountains at the light of this study' outcomes. The short section 5.3.1 places the post-glacial period back in the Quaternary, i.e. with several successive glacial /interglacial periods. We are not certain that this reflection can be placed earlier, and section 5.3 would no longer be relevant if we moved section 5.3.1.
Concerning the fluvial (Pisse) catchment, we argued that its lower part seems to have achieved a V-shaped valley, characteristic of a fluvial erosive catchment. However, as explain in section

5.2.2, the glaciation was probably less intense in this area and the U–shaped valley not well marked.

Lines 772-797: as mentioned, I believe enhanced hillslope processes are already a recognized component of what has been presented as 'the glacial buzzsaw', which could be recognized in this section. This does not make the current study irrelevant in this context.
We agree and thank the reviewer for raising this point. The concept of glacial buzzsaw applies during glacial period, when, as correctly pointed by the reviewer, the mountainous reliefs created by glacier indirectly induce hillslope processes on steep slopes. Here we are referring rather to the interglacial period, when glaciers are no longer the main agent of erosion.
Moreover, the mechanisms associated with the buzzsaw are still not very clear in the literature. For example, Brozovic et al. (1997) and Egholm et al. (2009) illustrate the hypsometric distribution of elevation and slope, but landslides are not included in their studies. Only Mitchell and Montgomery (2011) addressed explicitly landslides as a potential mechanism of the buzzsaw. We think this is therefore an area that remains to be explored.

Comments on Figures:
Figure 3: please specify t_n, t_m, etc. in the caption. Also, for clarity there should be arrows from 'Trimline zone' to yellow circles in both sides. Done as suggested.

Figure 4, caption. 3. 10^4 m^2 (the period .) should be fixed, here and throughout the paper.
Corrected.

Figure, 6. I would suggest also to show panels with the change in slope (final slope versus initial slope). It is difficult to assess the changes not having the initial slopes at hand.
The initial catchment slopes are presented in figure 1, and not to overload figure 6 we preferred to show only final slopes.

Figure 7: *I would suggest adding thin horizontal lines at slope 0.7 to mark the internal angle of friction.* Done as suggested.

Figure 8. *unclear why panel B is termed 'steepest slope'.* Modified to 'all slopes'.

Figure 9: *y-axis label could simply be 'triggering point elevation (m)'. I think right panel C could be interpreted as bimodal, although I agree it is not as clear.*
The y-axis label appears less ambiguous like that according to us. Concerning panel C, yes it could be but there is only one elevation interval which is lower than others, so the apparent bimodal distribution can result from our binning. We prefer to be careful and not to over interpret this result.

Cited References:
Bernard, M., van der Beek, P. A., Pedersen, V. K., & Colleps, C. (2025). Production and preservation of elevated low-relief surfaces in mountainous landscapes by Pliocene-Quaternary glaciations. AGU Advances, 6.

Reviewer 2: Alex Densmore

This is a good manuscript that uses an appropriate numerical landscape evolution model (HyLands) and a set of intentionally-simplified numerical experiments to look at patterns of landsliding in mountain catchments after deglaciation, and by extension to look at the evolution and resetting of glacially-eroded catchment topography after deglaciation. The authors have made a number of simplifying assumptions but these are generally well-explained and justified. A few of the points raised in the discussion, such as the importance of landslides as an erosional agent, are at least in part a product of these assumptions, and I've flagged some places where I think this needs to be more clearly acknowledged.
There are a number of statements that are ambiguous or hard to understand, along with some more minor typos and grammatical errors, and I've noted these in the PDF. I won't repeat these here, but one more general point is that the authors refer repeatedly to a set of hypotheses that differ from place to place in the manuscript. I'd strongly suggest that their hypothesis is stated once in the introduction, and then referred back to consistently throughout - as written, though, it's not entirely clear what they're trying to demonstrate.
The suggested edits and clarifications should be fairly straightforward for the authors to address, and so on the whole I'd class this as minor revision. I look forward to seeing this published.
Alex Densmore
We thanks Alex Densmore for the manuscript's evaluation and insightful comments that allowed to better present our study and results. Following your comments and your corrections in the manuscript's pdf, we made all the suggested changes in the main text. Here, we respond in more details to some of your comments.

Line 25: *Does this mean that the model is calibrated to reproduce an observed area-volume scaling law from the area and an observed spatially-averaged denudation rate? Or have I misunderstood this?*
Since we did not have a specific area-volume scaling law for the study area, we cannot say that the calibrated model is reproducing it but we calibrated our model to predict an area-volume scaling which is in agreement with global compilations. In addition, yes the calibration method used knowledge from spatially-averaged denudation rate specific to the study area. We cannot expand this further in the abstract, but have detailed this in our Methods section.

Line 26: *Two questions on this - first, by 'less intense' do the authors mean fewer landslides, or fewer landslides in repeated places, or...?*
Second, line 26 says that the model is calibrated against the massif-averaged denudation rate, so how can lower erosion rates in the fluvial case be sustained? Aren't they meant to be the same in both cases?
We thank the reviewer for these points. 'less intense' means fewer landslides, the text has been changed accordingly. For the second point, we cannot expand the abstract to explain further the difference in erosion rate for this catchment, but all details are provided in the Results and Discussion sections.

Line 94: *"I fully agree with this sentence, but it doesn't really emerge as a research gap or problem from the text above. Instead, this paragraph seems to suggest that we know that landslides will occur after deglaciation and permafrost retreat (77-79), that we expect high rates of landsliding after deglaciation (85-86), and that there's a feedback with fluvial incision (88-92). So, if we understand all of those things, what's the knowledge gap that the authors are*

trying to fill? Or, to put it another way, haven't we already quantified the spatio-temporal impact of landslides on postglacial landscapes?
Note - I agree with the authors that this is important to do! But I think the paper will be strengthened if they can use this part of the introduction to point out what we DON'T understand, and therefore what gap they will fill. This only needs to be an additional 1-2 sentences on top of what is here...”
We thank the reviewer for raising this point, and have tried to point out the remaining gap at the end of this paragraph.

L133 : *There's nothing wrong with the text in this paragraph, but it's also not particularly closely linked with the focus of the manuscript - the authors could cut this down to keep the focus on what they are trying to do. For example, the sentence on soil-covered models and rainfall variability is fine, but not really relevant here.*
This paragraph has been written to provide an overview of the differences between existing numerical models of surface process and mass wasting processes. We agree with the reviewer that it could be presented in a more concise way, and have removed the last sentences to keep only main points. By presenting other types of models which also deal with surface processes, this is a way of justifying our choice to use the Hylands model.

Line 184: *Is it possible to show this on Fig 1?*
The red stars on the map illustrate the data we have on the deglaciation timing for the study area (Delunel, 20210, cited on Figure 1 caption). The proposed timing around 15 ka for the Pisse catchment is an extrapolation timing based on the available constraints upstream. We now refer to Figure 1 explicitly for this deglaciation timing and have rephrase the sentence.

Line 192: *This is a very effective figure. The annotation text is quite small and could be made larger, especially in panels B and C - the Veneon river is hard to see. It would also be good to clarify what the stars represent - are these meant to show the locations of the glacier termini at those times? 'Estimated deglaciation timing' is a little ambiguous.*
We thank the reviewer for this comment. The figure has been changed for annotations being bigger (panels B and C) and we have clarified in the caption what the stars do represent (exposure of glacially-polished bedrocks and erratics).

Line 203: *It might be more straightforward to reference this conceptual figure first (as Fig 2), and then to describe the observed profiles as Fig 3, rather than going back and forth between them.*
We have tried swapping the figures 2 and 3, but that doesn’t avoid the back and forth between the figures and it doesn’t make the reading any smoother, so we prefer leaving the initial numbering.

L209: *It would be worth spelling this out the first time it is used. Also, the ELA isn't shown on any of the figures, so it's not clear how it actually relates to the observed topography in these catchments.*
We have spelt ELA there for clarity. We agree that it would be interesting to show the ELA on figures. However, the actual ELA is really high in catchments (~3100m) compared to the long-term ELA (~1800 m at the LGM). In turn, the actual ELA would be out of the figure for the fluvial catchment and not really related to the actual topography for the two upper catchments. Instead, as a reference between the three catchments, we have illustrated (dash grey line in Figure 2.) the elevation at which the model simulates a low frequency of landslides, linked to

the turning point in the topography (Liebl et al, 2021). This well-marked shoulder visible on the actual topography is probably related to long-term effects of glaciations, with oscillating ELAs between 1800 and 3100 m.
We added these details in the text (Section 2.2 – Line 220).

L273: Just to clarify - this is defined on a cell-by-cell basis, right? And if so, is t in equation 2 the model run time, or the time since the previous failure at that cell?
As written, this is still deterministic, because P_t will increase with time and asymptotically approach 1. Can the authors add a sentence or two to explain how they introduce a stochastic component, as mentioned on line 237?
Yes, the model is defined on a cell-by-cell basis. In equation 2, t is neither the model run time nor the time since the previous failure at that cell. Following Campforts et al., 2022, *t* (yr) is a time interval equal to *dt* which remains constant during the model runs. We added this in the main revised text to clarify this point.

L289: This is OK, and the authors are clear about what they've done. Given the regional erosion rates of c. 1 mm/yr, we'd expect c. 100 m of base level change at the outlets of these catchments, which is therefore neglected. I'd expect the authors to come back and examine the impact of this assumption in the discussion.
Yes this is a good point there, and we are coming to this point (fluvial incision) in the discussion (section 5.1.1, Line 610).

L425: I wonder with this how much the final topography and slope histograms differ from a much simpler case where the topography is simply trimmed above a threshold slope. In other words, over 100 kyr, does the stochastic aspect of the model matter compared to the imposition of a simple threshold slope? Maybe the authors will come on to this point in the discussion...
Indeed, after a characteristic time, maybe longer than 100 kyr, the final topography is similar to a simple threshold slope model, close to the critical slope. However, compared to a simple slope threshold model, the stochastic aspect of our model enables to describe both individual landslides, their location in the catchment and their timing during the postglacial phase.
We now mention this point in the discussion (section 5.2, lines763).

L445: I don't think the translation to degrees is necessary - for comparison with the figures it's fine to leave this in m/m
We prefer to leave the translation to degree as it is, as does not make the text any heavier.

Line 472: perhaps 'around and just above'? I'm guessing this is because the landslide algorithm chooses a failure plane that bisects the topographic slope and the friction angle, leading to a lot of model hillslopes that are somewhat (but not much) steeper than 0.7 m/m. In fact, it would be impossible (?) to generate failure on a plane sloping at 0.7 m/m, so because there's no deposition or base level change, the net result is going to be that slopes less than or equal to 0.7 m/m won't change. It would be worth mentioning somewhere that this is hard-wired into the model.
We thank the reviewer for this point. We have changed as suggested the text in caption, and have addressed this point further in the discussion (section 5.3.2).

L 516, Fig 10: The annotation text is very small, especially on the insets - can this be increased?

Also, since the text refers to simulation times in kyr, it would make sense to have the x-axes in the same units, so that the reader doesn't have to convert things.
We made the changes in the figure 10 and we also modified the x-axes in figures 11 and 12 to be consistent.

L549, Fig 11: The text refers to rates in mm/yr and times in kyr, but the axis labels here are in different units - these should be made consistent.
We have changed the x-axis label and we made this point consistent for all figures similar to this one in the manuscript and in the supplementary.

L558: It's up to the authors of course, but I wouldn't choose to start the discussion with a section on model limitations. I think it would be better to focus on what they can say with their model setup, and then turn to what can't be said or how it could be improved.
True, it would be a way to organize the manuscript. However, we chose to not finish the discussion with the limitations. It appears also relevant for us that readers have the limitations in mind for the discussion section, but we rephrased the title of the section to be more general ("Modeling approach").

L590: I don't quite follow the authors' argument here - they mentioned previously that there is fluvial incision happening in these catchments, and there's little space for much sediment accumulation, so the main missing element seems to be fluvial incision. If that's happening at a rate of c. 1 mm/yr in response to base level fall, then there's not much scope for higher hillslope erosion rates. Or am I misunderstanding what they are saying?
We have rephrased that sentence, our argument for the fluvial systems is that most fluvial valleys in our catchments are not evidencing incision, except at the catchment outlet with small gorges, and can be considered as sediment bypass or buffering areas, reducing in fact the hillslope erosion rates with sediment transient storage. That is why the adopted 1 mm/yr is more likely an end member minimum value for hillslope erosion rate in our catchments.

Line 598: Isn't the intention to simulate a single glacial-interglacial cycle (lines 27, 413)?
This is correct, but we also investigated longer timescales with our long 100-kyr simulation. We have rephrased the sentence : " Ignoring sediment transport over either the postglacial period or a long timescale (100 kyr), which in reality should include multiple glacial-interglacial oscillations, is a strong model limitation... ".

L637: It is a little awkward to have a fairly extended consideration of results that aren't shown in the main text - this paragraph requires a reader to download the SI and flip back and forth between that and the main text. If the authors want to include this comparison, I'd suggest moving that figure into the : main text to avoid this.
We agree and have moved the details of this extra simulation in the supplementary and, in parallel, reduced the description of the Ff parameter in the methodology part (3.1.2). We cannot for clarity provide all simulation results in the main text, so we prefer to simplify the message by moving the supplementary simulations in its document.

L646: That's true, but I'm not sure how relevant those parameters are for bedrock landslides, to be honest. All told, I don't think this is the biggest issue with the experiments

We understand the reviewer's comment and concern, but we have decided to leave the sentence in our main text because ongoing research is being conducted on these parameters and we think it shows the potential complexity associated with mass wasting.

L680: "our modeling results suggest that the glacial and intermediate glacial-fluvial catchments have not yet completed their post-glacial transition after 100 kyr simulation "
That's true - although the lack of fluvial incision/aggradation also means that there is nowhere else in the catchment where landslides can happen, other than those initially steep locations. If fluvial incision were allowed to occur, then you'd expect some landsliding on the lower hillslopes adjacent to the trunk stream - and then the bimodal pattern would be less pronounced... so this could also be an artifact of their simplified model configuration.
The addition of fluvial incision in the model would probably have created steep reliefs in the bottom of the valley, such as gorge, prone to landslides. It could create a trimodal distribution of landslides (river gorges, valley walls and crests) but should not erase the initial bimodality. However, this fluvial incision would not necessarily reduce landslide activity on the upper part of the catchment as glacial period already shaped steep slope above. Moreover, the U-shaped valley created during glacial period implies low slope areas where fluvial incision will occur, which disconnects this new relief from the valley walls.
In the field, we can observe some gorges between the main trunk and the tributaries, but they are of limited size (< 10m). Also, the development of these gorges is related to knickpoint propagation, which would be interesting to study but seems a bit out of the scope of this paper. (due to the limited timescale associated to post-glacial dynamics).
We have made some changes concerning fluvial incision in the section 5.1.1.

L687: But this isn't accounted for in the model, so it's not clear why it's relevant here. There's also no way for the model to reflect permafrost loss (following sentences).
Here we used a real initial topography (present day) which is possibly influenced by the permafrost. In turn, nunataks and crestlines in the glacial catchments are still maintained by the role of permafrost. That is partly why the landslide model, based on local slope compared to rock strength without the role of permafrost, suggests frequent landslides at high elevation. Without permafrost, these particularly steep reliefs could have already been eroded by landslides, as the model suggest at the beginning of the simulation. We added some details in the main text to clarify this point (section 5.2.1, line 714).

L710: I agree with this statement, but given that the authors have ONLY modelled erosion by landsliding, and given that they've calibrated their model so that landslide erosion matches the regional spatially-averaged incision rate, I don't think they can claim this as a finding - instead it's inevitable, given the way they've set up their experiments.
We agree with the reviewer that for the intermediate catchment (Etage), it is not a finding that modelled erosion rate matches observed erosion rate given that we calibrated the model like that. However, it is the comparison between the three catchments which is a finding, as only one catchment was used for the calibration. The distinct evolution of the erosion rate in the three catchment depends on their respective topographies and properties, not on the model setup. We slightly rephrased these sentences for clarity.

L732 : That's fine - but with a 1 Myr simulation, the lack of base level change (or rock uplift) and the lack of any fluvial activity really start to become important, and so I'm not sure that this is a very useful or meaningful experiment.
This short experiment is of course not realistic as many processes (e.g., tectonic uplift, isostasy, sediment transport) are not taken in account. However, we think it allows discussing the characteristic time needed to reach an "interglacial-state" topography with no more landslide prediction in the absence of other external forcing.

Line 749: Are there other reasons to suspect this - catchment elevations or hypsometry, for example?
We have specified in the main text that this catchment is in a more external position in the massif, and has overall lower elevations.

L786: I agree with the concentration of particular slope values, which as mentioned above is probably at least partly due to the failure algorithm. But I don't see evidence of these slopes occurring at specific elevations in Fig 7. In fact, if anything, what Fig 7 seems to show is the expansion of those slopes of c. 0.7-1.0 m/m to an increasingly wide range of elevations over time, as the hillslopes are forced to become increasingly planar (which is also seen in the final slope maps in Fig 6). So I'm not sure that this is really all that similar to a 'buzzsaw' producing hypsometric maxima within a narrow elevation range. Or have I misunderstood what the authors are arguing for?
We agree with the reviewer that the range of elevations with the particular slope of 0.7-1.0 m/m may be larger than the one created by a "buzzsaw" mechanism. However, the range of elevation is still limited compared to the full catchment-elevation range and some specific elevation clusters concentration appears. These clusters reflect the landslides activity bimodality and thus are located around the shouldering.
We have modified the main text to discuss the difference between our proposed landslide and literature glacial buzzsaws and to also question the buzzsaw mechanism.

Line 795: How does landsliding produce low-relief areas? That's not a model outcome from the figures, certainly not Fig 6.
We have rephrased the sentence by: "...and producing lower slopes and lower-relief areas at or above the ELA (Fig. 6)."

L815: I'm still not sure what the authors mean by 'intensity' - this should be clarified when they first use the term
The landslides "intensity" is related to the volume of the landslide. We added this precision in the introduction (L90), and have changed in this sentence by "magnitude" for clarity.