Dear Editors, dear reviewer,
We thank the reviewer for the insightful comments, which greatly contributed to improving 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)

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.

<u>Line 25</u>: 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.

<u>Line 26</u>: 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.

<u>Line 94</u>: *"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.

<u>L133 :</u> *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.

<u>Line 184:</u> *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.

<u>Line 192</u>: *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).

<u>Line 203</u>: *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.

<u>Line 472</u>: 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).

<u>L 516, Fig 10</u>: 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.

<u>L549, Fig 11</u>: 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.

<u>L558</u>: 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").

<u>L590:</u> 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.