Response to Reviewers.

We thank the reviewer for their review and providing useful comments on the manuscript. We have now produced a revised version that we think addresses the comments and raised issues. We address each comments below with our response to the reviewer is in blue. Where we considered appropriate to add, we present in *blue italics fonts* the text that is added to the revised manuscript.

Reviewer #1.

The paper is generally well written, follows a logical structure, and is well illustrated. […] Thank you again for giving me the opportunity to read through this manuscript. I genuinely enjoyed reading it. With some extra work this study should become an excellent piece of work.

We are happy to see that the reviewer supports the publication of our work. We thank the reviewer for constructive and useful comments. We hope we have successfully addressed the points that the reviewer raised.

Furthermore, I requested a clarification in the text regarding why the presented Martian database was limited to a seemingly arbitrary runout distance of 5 km and suggest expanding it to a complete database which would certainly increase the significance of this work. However, I am aware that this would require a significant time investment and might be out of the scope of this work. Thus, a clarification of the 5 km cut-off value within the methods will be sufficient.

We added a clarification for the cut-off value of the length of martian landslides. Our intent is to have a population of martian landslides with comparable size to the Icelandic landslides. This is because we want to remove any possible size effect to the morphometric results and morphological structures. In doing so, that is keeping one parameter locked, it will be easier to identify other parameters (e.g., elevation drop, gravity) that may contribute to differences, if any, between the two populations.

The discussion and conclusion should be expanded based on the descriptions presented in this work. It would be great to add a paragraph discussing why Icelandic landslides commonly form longitudinal ridges compared to other places on Earth and what we can infer from that for conditions on Mars and vice versa? This was the central theme of the introduction, and it is reasonable that the authors outline their thoughts on this in an additional paragraph. Adding this paragraph will round up the manuscript by reconnecting with the subject outlined in the introduction. Furthermore, it will increase the significance of this work. The conclusion paragraph should then be updated.

We expanded our discussion (as also requested by Reviewer 2). We left the conclusion paragraph as it is as we think it succinctly summarise our work.

I added numerous comments with improvement suggestions to the attached pdf documents.

We have made many of the suggested changes. Here we provide the response to some of the reviewer's comments:

- We changed the title, substituting 'Landforms' with 'Landslide Deposits'. We also modified 'landforms' in the abstract, as suggested.
- I am wondering if there are as well "non- long runout" landslides that have developed longitudinal ridges? If not then I personally don't find it necessary to add "long runout" every time.

We are not aware of any case of landslides that is not long runout exhibiting longitudinal ridges. However, we think that it is important to use the 'long runout' as it highlights the link between the development of the ridges and this type of landslides.

- Did you identify all longitudinal ridges with the available DEMs or as well from differences in vegetation and/or shadows in RGB imagery? Add one sentence about that to clarify your approach.

We specified that we used high-resolution satellite and aerial imagery in the first sentence of this section. In the first sentence of the introduction, we specified that longitudinal ridges *'are features visible at the surface of the deposit and extend parallel to the direction of movement'*. We do not think it is necessary to add another sentence as we did not use vegetation or shadows.

- this is a misleading description of paleosols. At least I am assuming you are referring to the red interbeds/redbeds between the lava layers. those are old soils that may be of basaltic origin but not necessarily. I would simply rewrite to: "..... of Tertiary basalt lava flows alternated with softer, red interbeds (paleosols)....". as well, in case you are writing in british english then palaeosols is the correct spelling

We used the description given in the literature (see citations). However, we removed 'vescicular basaltic'. Unless the editor has different opinion, we are ok with the spelling' paleosols'.

- I find this difficult to understand. what to you mean by "inclined surface"? the mountain slope itself? please clarify.

We have added the following part in order to clarify (now lines 236-238): '*Such connecting surfaces have slopes of about 8□-15□. They correspond to sedimentary units, whose origin cannot be established from remote sensing only. A river gorge near the Dalvík landslide exposes outcrops showing the sedimentary origin of the inclined surface over which the landslide deposited (Figure 5c).*'

- What I find very interesting is that the H/L ratio of the Icelandic and Martian means is basically identical (0.293 vs. 0.297) [plus many other comments on this section]

As also raised by Reviewer 2, this section has some issues in explanation and reasoning. We have modified the entire section 5, which is now divided in Section 5 and 5.1. As part of this modification, we added this sentence: '*It is interesting that these two populations with equivalent L (that is, with assumed equivalent volume) show similar mobility (Icelandic average H/L ratio = 0.303; martian average H/L ratio = 0.292), thus supporting the absence of distinctive trends of mobility in small-scale terrestrial and martian long runout landslides.*'

- This comparison is problematic. Instead of focusing on the total numbers of the respective populations in the databases… / Explain how different conditions on Mars and and Earth can lead to this overall shift. /

As also raised by Reviewer 2, this section has some issues in explanation and reasoning. We have modified the entire section 5, which is now divided in Section 5, 5.1, and 5.3 (we did not modify section 5.2). We addressed the raised issues in the newly written section 5.1

- it can be confusing for some readers if you talk of long runout landslides and then suddenly call them small-scale.

In order to better express the concept of small-scale long runout landslides, we added/modified several sentences throughout the manuscript.

Now lines 43-46: '*Therefore, the definition of a long runout landslide is not based on the final horizontal length of the deposit. In fact, the final horizontal length of long runout landslides can range across two orders of magnitude, from less than a kilometre to tens of kilometre (Legros, 2002, and references within). However, it appears that a volume threshold (106 m3) exists, below which landslides do not develop a long runout (e.g., Heim, 1932; Legros, 2002).*'

Now lines 134-136: '*In total, we selected 112 landslides. As martian landslides can be up to 70 km long (Lucchitta, 1979), those selected for this work will be also referred to as 'small-scale', as done in the literature (Guimpier et al., 2021).*'

- do you mean conditions? - could you specify what mechanism(s) you mean? are this slide specific dynamics or climatic changes/conditions that led to more frequent long runout landslides on Mars and to a lack of recent long runout landslide activity?

These are exactly the things that remain unresolved in the study of long runout landslides. We did not make any changes.

- figure 1, 4 and 6 could be combined into max 2 or even a single figure

We did not make any changes.

- We modified axes' caption and added scale bar in Figure 6
- (Figure 1) marking the panels with a and b makes it much easier for the reader / add the view direction used in of the right panel to the left panel

We added letters to the panels. We added a drone symbol and an arrowhead to show the view direction in panel b

- (Figure 13) please add that in the caption for the y and x axis. For more intuitive reading of this I would recommend to add more numbers to the y axis. Furthermore, I find it easier to read if you just write out the numbers instead of using the exponential format / mark the Davík landslide and the Martian landslides from fig 8 in the lower two lots too / a Figure numbering a-c would be useful

We made the recommended changes.

Comments from the Supplementary Materials:

- In a and b it would be helpful for the reader to add contours or some other way to gain perspective of elevation

  The purpose of the figure is to show the partially buried landslides. Although we appreciate that providing perspective of elevation would be a plus, we do not think it is fundamental for the purpose of the figure. Therefore, we did not add contour lines.
- turn c and d so the black arrows point in the same direction as on the left panels

  The two panels are orientated so that the North is towards the top of the images. The two panels are derived from the same landslide so the different direction of the flow (represented by the black arrows) is consistent with the spreading of the deposit. We did not make changes to the figure.

Response to Reviewer #2.

Overall, this is a very useful addition to the study of long runout landslides, and in particular, the improved understanding of these features on Mars. It is mostly very well written and clear.

We are happy to see that the reviewer consider our work useful to the study of long runout landslides.

Some statements regarding the suitability of the landslides studied on Iceland as appropriate analogues for Mars seem to lack support, and discussion of important aspects appears to be absent. This includes consideration of the different gravity and atmospheric pressure on Mars, and also the inevitable ambiguity of the role of surface snow or ice in the formation of the Icelandic landslides whatever their age. […] I had some problem understanding the logical flow of the argument in Section 5, and I think some editing of this section to help "hesitant" readers.

We thank the reviewer for constructive and useful comments. We hope we have successfully addressed the points that the reviewer raised.

Line 33. Saying "such as" suggests that there is a longer list of specific distinctive morphologies, and it seems odd not to provide a full definition here that includes a complete list (perhaps in grouped or truncated form if the list is long).

We added additional morphologies.

Line 44. Change “are a necessary” to “is a necessary” to agree with “the presence” which is singular.
Done.

Line 49. “in tectonic context” – modify to “in a tectonic context” or “in tectonic contexts”.
Done.

Line 55. The term “slope stratigraphy” here is a little hard to interpret. I think the authors are referring to the original stratigraphic layering which is often preserved within the landslide mass, and observable in the slope at the point where the landslide started. Perhaps clarify this point?
We changed into ‘original stratigraphic layering’.

Line 61. The words “these landforms” refers to glaciers, but I’m not sure what “and associated morphologies” means here.
We changed into ‘landslides with longitudinal ridges’.

Lines 71-72. The phrase “that either longitudinal ridges are indeed common in long runout landslides on Earth” appears to me to have an additional required element missing to work as an explanation of the observation. If they are indeed common, why have they not been found commonly elsewhere, or is there a preservation bias on Iceland (for example, being young in age, or at lower elevation, or some other factor)?
We agree and we acknowledge that we ill-expressed the idea of a preservation bias. We have modified the sentence.

Line 80. From a consideration of the different gravity on Mars, would a good analogue be expected to be the same length as on Earth? Does the fact that the authors consider similar sized landslides as comparable between Earth and Mars suggest that they reject any role of atmospheric pressure in providing a gaseous lubricant during landsliding, as this is very different between the two planets?
This is a good point. We did not make any suggestion about mechanisms as it remains an open question in the long runout landslide community and we do not think that the comparative work done in this study can attempt that. However, we have discussed some of the issues raised and included some of the suggestions from the reviewer in the new section 5.3.

Lines 81-83. The authors say “long runout landslides share similar morphometric values and diagnostic structures, such as longitudinal ridges…” Again, why simply provide one example of a similar structure when it would be possible to be precise and provide a more complete list of values and features?
We modified the sentence in ‘similar morphometric values and diagnostic longitudinal ridges’ to be specific as we are describing what we have done in this work.

Lines 83-84. Is similarity of morphology sufficient to conclude that Earth examples are “good analogues of martian landforms”, unless the authors simply mean analogues in terms of morphology? That is, can we conclude that similar morphology implies similar formation mechanism? Geomorphologists coined the term “equifinality” specifically to highlight the problem with this interpretation which is often found not to be the case on Earth. I feel that some discussion of the theoretical or philosophical implications of this statement is required to justify it here.
This is a good point. We added a paragraph on the issue of equifinality in the new section 5.3.

Lines 101, 106 and elsewhere. Avoid double parentheses.
Done

Lines 127-128. My understanding of the term “paraglacial” is that it refers to processes following local deglaciation. That is, the ice front may be a few meters away only. The concept of “paraglacial

period" is therefore potentially confusing to readers unaware of this. Note that an exponential decrease in mass-wasting might be consistent with much of this movement having occurred while there is still glaciation in the area (depending on the relative exponential lifetime of mass-wasting and rate of glacial retreat).
We have changed 'paraglacial period' to '*paraglacial adjustment*' in accordance with the previous sentence, which refers to the processes.

Line 147. Quotation of calibrated radiocarbon ages should be presented as a 2 sigma age range (as for the earlier ages); the present authors are probably quoting Mercier et al. (2017) when they provide estimates with +/- uncertainties, but these are not typically meaningful for calibrated ages. Please can the authors carefully check the original data, and recalibrate the raw data if necessary, to derive a useful age range here. Note that lab codes for these age estimates should also be provided (possibly in supplementary data, but with a citation here).
We have reported the ages as they are provided in the papers that we cite. This paragraph simply summarises existing results about ages of the landslides considered in this study, showing that they occurred in a cold period in Iceland. Therefore, we do not think it is necessary for the scope of the paragraph (and given that we do not conduct any dating in this work) to check the original data, recalibrate the raw data and provide lab codes. We cite previous literature, in which details are provided. We added two sentences to invite readers to refer to the cited papers to find the details: *'here we report the ages as provided in the literature; for further details about dating techniques used, age-depth models, and uncertainties, the reader should refer to the cited literature'*

Line 149. Presumably the authors mean 1 km northwest (not northeast) of Dalvik; 1 km NE is in the sea.
Done.

Line 153. For a landslide, the "accumulation zone" is at the bottom (presumably). Is this what is intended here, or does this mean the upper part (somehow analogous to a glacier accumulation zone)? Possibly clarify the wording here, as it is potentially confusing.
We changed into 'deposit'.

Line 177. Change "is " to "are" in "the statistics… is summarised…"
Done.

Line 183. In what ways does this "inclined surface" differ from the higher elevation slope? Why consider this part as distinct from the higher slope? Maybe there are morphological differences such as slope angle or roughness or concavity? Please add if this information is available, or simply refer to this as the lower slope.
We have added the following part in order to clarify (now lines 236-238): '*Such connecting surfaces have slopes of about 8□-15□. They correspond to sedimentary units, whose origin cannot be established from remote sensing only. A river gorge near the Dalvík landslide exposes outcrops showing the sedimentary origin of the inclined surface over which the landslide deposited (Figure 5c).*'

Line 211. See Line 177 comment above. Change "is " to "are".
Done.

Lines 221-224. After reading and rereading four times, I still cannot understand these sentences. Are the ridges visible in CTX and HiRISE images or not? OK, in simple images, yes, but there aren't DEMs available to determine their elevation characteristics? Is this what is being said here? It is very hard to follow.

We modified the paragraph that now reads:
'*Longitudinal ridges are visible in both CTX and HiRISE images across the entire length of the debris aprons. Similarly to what we observed at the Dalvík landslide, longitudinal ridges split, generating two ridges from a parent ridge (Figure 11). Unfortunately, the resolution of the CTX-derived DEMs that we have generated (20 m/px) is not able to resolve the topography of longitudinal ridges; and no HiRISE image pairs are available to generate higher resolution DEM of the landslides included in this work. Therefore, a topographic comparison between longitudinal ridges of the martian and the Dalvík landslide could not be conducted.'*

Line 232. Add "do" before "they share".
Done.

Lines 238-246. It seems very strange to me to have this discussion of the differences in size of landslides without mentioning the differences in gravity between Earth and Mars, and without considering potential differences in the atmospheric involvement. Why expect a scaling factor of 1?

As for other similar issues raised by the reviewer, we have discussed this and included some of the suggestions from the reviewer in the new section 5.3.

Line 249. "The former suggests…" Surely this should be "The latter suggests" as the removal hypothesis comes second in the previous sentence? But why the sudden jump to clear expectation of an exact match in shape and size between Earth and Mars? If in one population removal of some landslides biases the estimates, then surely the comparison will be confused by relatively recent glaciation in Iceland, that was extensive around the LGM and probably retreated in the late glacial. Most or all of the landslides studied in Iceland are likely to be post-LGM (as found by the small dating sample), so earlier landslides in the Iceland population were certainly removed. Are there some assumptions about removal that are not discussed here? I can't understand how it is possible to propose that a comparison can be drawn between these two populations as they are so morphologically similar, but then infer some geologic process based on the observed differences.
As also raised by Reviewer 2, this section has some issues in explanation and reasoning. We have modified the entire section 5, which is now divided in Section 5, 5.1, and 5.3 (we did not modify section 5.2). We addressed the raised issues in the newly written section 5.3.

Lines 253-255. "Therefore, we suggest that the different population numbers reflect the removal of the geomorphological records of martian long runout landslides." Again, I cannot easily follow the logic here. Why do you expect the populations to be identical? I thought the point of the paper was to suggest that they might be similar by comparing the statistics. If they are not the same, are they simply not the same? Or am I missing something fundamental here? I'm happy to accept that both populations represent snapshots of landslide creation, weathering, erosion and/or burial, with some fraction of landslides still visible. We know and can infer something about the timescales on Earth for this population, but on Mars, so far no chronological considerations were presented (though these do appear below). So I find this section hard to understand. In comparing the statistics of the two datasets, don't you have to normalize for the available vertical drop? This (the vertical drop) clearly can't be greater than the height of the mountain hosting the landslide. What about the frequency of large magnitude earthquakes? This is likely greater in Iceland than Mars, and an important trigger for landslides, but not considered here.
Same as above comment.

Line 260. More recent than what? Than the last 20Ma? Doesn't that time period extend to the present? How many recent deposits (and what is "recent" in Mars terms?) would one expect when

the available time period is 3.5Gyr? 1 landslide per million years in the target area would equal 3,500 landslides. Again, perhaps I'm missing something here.
Same as above comment.

Line 267. Replace "significant" with "significance".
Done.

Lines 278-9. The H/L ratio vs elevation drop (Fig. 13c – note the letters need adding to the figure) shows what appears to be a very significant lower boundary in both datasets. This boundary is parallel in both datasets. This suggest to me there might be something significant about this, so I'm a little surprised that attention isn't drawn to this feature.
This is a good observation. We added the sentence '*The plot, also, seems showing that there are two distinct lower boundaries for the martian and Icelandic landslides.*' However, we do not want to speculate about the origin of such distinct lower boundaries, as this should need an analysis of more landslides and possibly from other planetary bodies before we could suggest something, for instance that is due to gravity.

Line 295. Should "decrease" here be "increase"?
We added a sentence that explains the scaling relationship mentioned in the text: '*the distance between ridges always ranges between 2 and 3 times the thickness of the deposit*'. We realised that the lack of that explanation would lead a reader to confusion. With this addition, it should be clearer and that 'decrease' is correct.

Line 326. I wonder whether "structure behaviours" would be better as "characteristics of structures" or Structure characteristics"?
We prefer to leave it unchanged as we think 'behaviours' convey a sense of dynamics.

Figure 1. Is the longitudinal profile an average of the slope profile or one particular line? Consider adding letters to distinguish the different panels (perhaps an editorial decision).
We modified the figure and caption to clarify where the topographic profile has been obtained.

There are no references to Figs. 2 and 3 until after Fig. 4 is mentioned, so change numbers or add figure citations. Note Fig. 13 is mentioned in the text before Fig. 7 and later figures.

Figure 4 is mentioned before Figure 2 and 3 because it shows the locations of the GCPs which are mentioned in the methods section and not because it is discussed. Therefore, we do not think that figure numbers have to be changed.

The reference to Figure 13 is wrong. We have changed now to '*Supplementary Figure 2a and 2b*'.

Figure 3. Top right panel (add letters to these?) has incomplete white line.
The incomplete white line is because we are not able to trace the entire deposit as it looks like it has been buried by another deposit. We added the letters to the panels.

Figure 7. Add north arrow, and consider showing location on Fig. 6?
We added the north arrow and we added a small version of the entire landslide to show the location of this area. We did not add the location in figure 6 as there are already 3 boxes and the additional would have overlap to one of them, and we were concern with the clarity.

Figure 8 caption. I think the word "which" (line 2) should be "with".
Done