Response to Reviewer 1

We would like to thank reviewer 1 for taking the time to read and provide feedback on this manuscript. Here, we respond to their comments point-by-point. Note that line numbers refer to the revised manuscript (unless otherwise noted) and quotations from the updated text are italicized and in quotations.

#	Comment	Line # / Section	Reply
1-1	The main short-coming of this study concerns it's overarching premise, which is stated as a regional analysis. The concept of regional analysis is not clearly defined in terms of scientific goals or questions being addressed at that scale. The eight landslide objects selected represent 1% of the total landslide inventory (perhaps this is a little higher when only lake/fjord terminating glaciers in southern Alaska are considered), and hence the authors need to consider whether the outcomes of the study have any relevance to the regional-scale understanding of glacier retreat and landslide instability phenomena in the lakes and Fjords of southern Alaska. In regional landslide studies, the analysis of multiple influencing factors are usually used to estimate susceptibility or plausibility of a landslide occurrence, particularly in terms of identifying patterns and key driving factors, in either a spatial and/or temporal context. The study here lacks this type of broad rationality, but rather focuses on eight landslide case-studies each with their own unique conclusions.	Overall	Thanks for this feedback. We agree that casting this work as a 'regional' study may have been somewhat misleading, and we appreciate the suggestion to present this work instead as a case-study (comment 1-2). In reality, this work falls somewhere between a regional analysis and a case study. We have too few sites for a regional study and the level of detail with a case study is typically higher than what we do here. Nonetheless, we are able to compare and contrast the evolution of eight sites, as well as open up some questions for further research. We investigate multiple influencing factors (precipitation, seismic energy, glacier retreat versus glacier thinning, and geology) and while we don't look at susceptibility or predict where future landslides may form, we are able to draw some general conclusions from the eight studied sites. Specifically, we find that landslides bordering water following deglaciation may be at particular risk for acceleration. Further work to see if this holds for more landslides over a wider area would be interesting, but is outside the scope of this work. Concretely, we've changed the title, removed references to a 'regional' work, and have instead framed the paper as a comparison of eight interesting landslides with sometimes similar and sometimes differing conclusions.
1-2	Revise the scope and the purpose of the study to reflect the case-study oriented approach using the eight landslides selected. However, bear in mind that the main outcome(s) lack scientific strength, e.g. it is already well established in the scientific literature that glacier volume reduction (e.g. thinning/retreat) can lead to slope movements, and on rare occasions, catastrophic failure.	Overall	Thanks for the suggestion. See the response to comment 1-1. We have reframed the paper and removed any claims of this being a 'regional overview'. However, we respectfully disagree with the reviewer that the results lack scientific strength - there have been only few studies comparing annual landslide evolution with glacier changes in Alaska. Additionally, the reviewer says that the link between glacier volume reduction and slope movement is well established, but the disagreement in the scientific community can be evidenced by the comments of reviewer 2 of this paper, who said "[Debuttressing] is not universally accepted as a causal mechanism of slope failure." McColl et al., 2010 is the most well-known example questioning whether debuttressing can cause slope instability. The authors note that ice is ductile under low strain rates and thus

			cannot be a rigid buttress for a deforming rock mass. Some papers have cited and built upon this idea (McColl et al, 2013; Storni et al, 2020; Lacroix et al, 2022), while others have found clear evidence for linkages between glacier downwasting and landslide activity (Kos et al, 2016; Glueer et al, 2020; Lacroix et al, 2022).
1-3	The potential strength of this study lays in the elucidation of the regional distribution of the landslide/glacier interactions, and the broader (spatial) understanding of dominant driving factor. This should include a regional consideration of rainfall and seismic activity (landslide-fault proximity). The current study already goes in that direction but is limited. To highlight a point stated in the conclusions, there is a potentially broader use of ITS-LIVE data on a world-wide scale, but to demonstrate this potential the authors should focus on a truly regional study across the fjords of southern Alaska, for which there is an extensive landslide and glacier inventory.	Overall	Thanks for the comment. A comprehensive regional comparison of landslide occurrence and the potential influencing factors (precipitation, seismicity) is beyond the scope of this paper, which focuses on eight sites specifically. Future work, which is mentioned in the conclusion, will focus on landslide detection over a larger area using ITS-LIVE, as well as comparison with various factors (elevation, aspect, spatial distribution, precipitation, etc.) to determine correlations with landslide occurrence. To make this clearer, we have added the following sentence to the conclusion (lines 570-571): "*Such a regional overview would allow for correlating landslide activity with various factors over a broader area, specifically elevation, aspect, precipitation amount, and proximity to faults, to name a few.*"
1-4	The manuscript is highly descriptive and long and could do with shortening/condensing, including moving non-essential information to a supplementary section.	Overall	Thanks for this suggestion. We have done a detailed read-through of the manuscript to eliminate superfluous information (e.g. repetition in the methods and limitations sections, ideas about tidal and wave influences, the changing ice to water interface, repetition in the results and discussion). We moved the detailed study site descriptions to the appendix as suggested in comment 1-14. We also moved Fig. 6 to the appendix, eliminated Fig. 7 by merging the precipitation information into Fig. 4 (and put the new supplemental plots in the appendix), and moved the updated Fig. 8 to the appendix.
1-5	The abstract is vague and requires more context with respect to the conclusions. As an example "..17 times higher compared to five years preceding the acceleration" (see line 10) – what is this referring to? The abstract needs to be more understandable.	Abstract	Thanks for this point. This was similar to feedback given by reviewer 2 (comment 2-2). We have rewritten the abstract and described the conclusions, and specifically the rates, in a more straightforward way (lines 8-15): "*We found that the majority of landslides underwent a pulse of accelerated motion during the studied time period. In four cases, landslide movement coincided with the rapid retreat of a lake- or marine-terminating glacier past the instability. At these sites and during these accelerations, the glacier retreat rates were up to 7 times higher than average, while the landslides reached velocities that were up to 9 times higher than their long-term average. Two sites showed no movement, though both landslides are known to be moving at velocities below the detection threshold of the methods employed here. At two other sites where the landslides are still in*

			contact with the ice, above-average precipitation and increased glacier thinning were found to coincide with accelerated motion, though conclusive causal links could not be drawn and the effect of short-term precipitation could not be ruled out."
1-6	The discussion of the Tungnakvislarjökull landslide should be moved to the section dealing with landslides in an Alpine setting.	50	We've moved this sentence to the portion of the paragraph about alpine glaciers.
1-7	discussions on landslides in Alpine setting section, the relationship of glacier unloading and landslide movement was established by Kos et al (2016), the statement pertaining to altered groundwater creating critical conditions is a speculative assertion in Glueer et al (2020).	50	We've rephrased the sentence and added the Kos et al. (2016) citation. We also adjusted the wording of the groundwater statement to clarify that it's speculative. The resulting sentences read as follows (lines 62-66): "*Studies of the Moosfluh landslide in Switzerland showed that landslide deformation can be related to debuttressing, with landslides reacting rapidly to glacier changes upon crossing a threshold of ice loss (Kos et al., 2016). Others found that the glacier controls the landslide velocity but has little effect on its stability (Storni et al., 2020), and (Glueer et al, 2020) suggested that altered groundwater conditions may lead to enhanced slope instability.*"
1-8	The potential landslide velocity is primarily controlled by litho-structural characteristics, buttressing ice is an external resisting factor with minor influence.	55	We are not totally sure to understand the reviewer's comment here. We believe the reviewer takes issue with the fact that the paragraph (lines 48-58 in the original manuscript) talks exclusively about debuttressing without referencing other factors (such as litho-structural characteristics) which are surely also relevant. To remedy this, we've rearranged the paragraph slightly to make the wording clearer. Additionally, we've added the following sentences to describe how debuttressing is still a debated topic (lines 45-48): *"In fact, there has been some debate about whether glacier debuttressing can cause slope failure due to the viscous nature of ice at low strain rates (McColl et al., 2010; McColl and Davies, 2013; Storni et al., 2020). Others suggest that debuttressing can increase shear stress and act in combination with other processes such as rainfall to promote slope movement (Le Roux et al., 2009)."* To highlight the importance of litho-structural characteristics, we've added the following to the same paragraph (lines 54-57): "*In addition, litho-structural characteristics (Kuhn et al., 2023; Stead and Wolter, 2015), rock mass properties (Wang et al., 2021; Gischig et al., 2016; Hugentobler et al., 2022), and changing lake water levels (Hendron and Patton, 1987; Wang et al., 2008) are among the mechanisms which drive landslide motion. All of these*

			processes—as well as combinations of them—may be relevant to the sites studied here."
1-9	and Kos et al (2016) suggested that landslides react rapidly to glacier changes upon crossing a threshold of ice loss”.	55	We are not sure to understand the reviewer's comment here, but we believe it's related to the fact that there were two citations within one sentence, and they were suggesting to make the latter into an in-text citation. We've reworked these sentences to reflect that change and they now read as follows (lines 62-66): "*Studies of the Moosfluh landslide in Switzerland showed that landslide deformation can be related to debuttressing, with landslides reacting rapidly to glacier changes upon crossing a threshold of ice loss (Kos et al., 2016). Others found that the glacier controls the landslide velocity but has little effect on its stability (Storni et al., 2020), and (Glueer et al, 2020) suggested that altered groundwater conditions may lead to enhanced slope instability.*"
1-10	Is it correct to write thinning or retreat? Retreat and thinning occurs simultaneously, are there glaciers that retreat without thinning or vice versa?	60	Thanks for this point. While glaciers typically thin and retreat simultaneously, they can also thin while advancing (in the case of a surging glacier, for example). We refer to thinning primarily as vertical change in the glacierized area, and retreat as horizontal change. However, this is something which we discuss later on in the discussion (Section 5.2) and see that it's confusing in the introduction. We've therefore changed the wording to remove "retreat or thinning" and instead say "ice loss."
1-11	I have doubts whether this study is a *detailed regional*	70	We've adjusted the wording to remove references to a regional study and reframed the paper to be a case study (please see our response to comments 1-1 and 1-2). Here, we rephrased the sentence as follows (lines 79-81): *“By doing so, we provide the first study comparing detailed glacier evolution---including both thinning and terminus retreat---with landslide movement in southern Alaska.”*
1-12	“…and discuss in the context of the possible physical mechanisms behind the slope instabilities (sect 5)”. What of the key factors geology and rock mass characteristics determining physical mechanisms? These are not featured nor discussed in the manuscript.	75	Rock mass characteristics are certainly relevant for slope stability. However, the remote-sensing-based approach that we use here does not allow for such a detailed consideration of the site-specific geology. Dealing with these factors is thus outside of the scope of this paper. I've added a sentence to the third paragraph of the introduction drawing attention to these relevant aspects (lines 54-57): "*In addition, litho-structural characteristics (Kuhn et al., 2023; Stead and Wolter, 2015), rock mass properties (Wang et al., 2021; Gischig et al., 2016; Hugentobler et al., 2022), and changing lake water levels (Hendron and Patton, 1987; Wang et al., 2008) are among the mechanisms which drive landslide motion. All of these*

			processes—as well as combinations of them—may be relevant to the sites studied here."
1-13	why are the criteria selected important or relevant? There could certainly be more criteria to consider geological susceptibility, permafrost thermal state etc. These are very important factors (spatially) to consider in a regional study.	95	Based on this feedback, as well as feedback from reviewer 2 (comment 2-4), we've restructured this section to have less reliance on strict selection criteria. We are interested in large landslides since they can have larger inundation zones (Iverson et al., 1998, Griswold & Iverson, 2008, Chae et al., 2017), ones which border glaciers so that we can study the effect of the changing glacier conditions, and ones which have showed recent signs of activity and thus may pose a higher risk of acceleration or collapse. We agree with the reviewer that geological susceptibility could be important too. However, this is very difficult to quantify and outside the scope of this work. Nonetheless, to address this point, we've added a sentence to the Section "Study Area" stating that all landslides are in sedimentary or metamorphic rock (lines 106-107): *"All study sites are large landslides in sedimentary or metamorphic rock."* We also pick up this point in the discussion (lines 444-445): *"[…] three out of four sites are in sedimentary lithologies (Yale, Tyndall, and Barry), which are particularly susceptible to water intrusion due to high porosity (Selley, 2005)."* Line 80 in the original manuscript draft stated that permafrost is unlikely, however we've now added an additional clause after the en dash to quantify this using data from Obu et al., 2018 (lines 89-91): "*The large precipitation amounts result in a thick winter snowpack which, combined with relatively mild temperatures, make extended permafrost coverage unlikely – less than a 1% probability of occurrence at our sites according to Obu et al. (2018) (Fig. 1).*"
1-14	these descriptions could be moved to a supplementary section and/or tabulated so they are more easily read.	Section 2.1-2.8	Thanks for the idea. In order to also shorten the manuscript as suggested (comment 1-4), we moved the descriptions to the Appendix and made a short, general description in the Study Area section.
1-15	legend for the geological base map should simply indicate the rock types (formation names are not particularly useful for those readers who don't know the local geology)	Figure 1a	Thanks for the suggestion. We changed the figure to have a more generalized form of the formation names, but did not change to rock type. From the 10 updated legend entries, over half contain information about the rock type (sedimentary & volcanic rocks, granitic rocks, etc.). One exception is "Chugach accretionary complex," a widespread unit in southern Alaska, and the lithology

			of this unit is described in the site description. Additionally, from the examples of other geological studies that we've seen, we noted that the formation name is typically given, not the rock type. We thus decided to stick with the formation name, but in a more generalized form as compared to the original manuscript.
1-16	Figure 1e is hard to read, perhaps make it into a separate larger figure. It would be useful to see the distribution of the landslide inventory more clearly	Figure 1e	Thanks for pointing this out. We've done some work on the color scheme to improve the readability of this figure, also based on feedback from reviewer 2 (comment 2-35). We've decided to keep panel e as a part of Figure 1, but we've added an enlarged version to the appendix so readers can see the landslide distribution more clearly.
1-17	Arrange the figure 4 sub components in the same order as figures 2 and 3.	Figure 4	Changed as suggested.
1-18	How would a precipitation (long-term) trend be an important factor for landslide activation/failure? What is the relationship? E.g. these questions need to be placed in the context of the enormous rainfall that parts of southern Alaska receive.	400	Thanks for this comment. Indeed, we acknowledge that analyzing annual precipitation may have some limitations (see "Meteorology" in the Methods, as well as "Landslide interactions with the glacier and environment" in the Discussion). There, we say that landslides can be caused by both short-term, intense precipitation and long-term precipitation causing saturation of the subsurface. In order to be more thorough, we've additionally analyzed ERA5-Land daily data. We've added a new plot to the appendix (Fig. G2) which looks at warm and wet periods. However, no link was found between the daily data and landslide movement, primarily due to uncertainty in the landslide activation time. Additionally, we've decided to analyze annual precipitation anomalies in Figure 4. This allows for comparison of a particular year against the long-term average (1980-2009) and thus puts a single year into a longer-term context.
1-19	This figure is difficult to read, they could be much larger in size, and maybe show only one Landsat image as a reference, with glacier/landslide outlines for each year where there are changes observed.	Figure 6	Per the suggestion of reviewer 2 (comment 2-40), we've decided to move this figure to the Appendix. We did increase the image size to the maximum possible. However, we chose not to use a single Landsat image because the multiple images show the evolution of various features (e.g. the development of a scarp or the appearance of a crack), and it is precisely this evolution which we aim to show. As the figure is now in the appendix, we decided to leave it as-is.
1-20	What is the significance of showing cumulative monthly precipitation plots in terms of landslide activation/failure when the dark blue lines correspond to other years (light blue lines) where no activation is recorded? Why isn't rainfall a significant conditioning/triggering factor in southern Alaska?	Figure 7	We agree with the reviewer that this figure was difficult to interpret. Because of the new analyses performed in reply to reviewer's comment 1-18, we decided to remove this figure from the revised manuscript.
1-21	an untested hypothesis is merely speculation. Is there precedence in the	415	We agree that this statement was speculative. After reworking this section, removing Fig. 7, and including

	literature for this assertion? Either demonstrates the plausibility of a snow loaded slope triggering movement or leave it out.		the precipitation time series in Fig. 4, we removed all references to snow load being a relevant mechanism for slope triggering.
1-22	triggering of a landslide would be associated temporally with the earthquake occurrence, but this relationship unfortunately cannot be shown. Lack of evidence in this case doesn't mean that seismic activity is not important, it may actually be more important than glacial retreat for landslide activation/ongoing failure – this should be expanded in the discussion.	Section 4.4.2	Thanks for this comment. We had tried to address this in Section 5.2, where we conclude the discussion of seismic activity by saying the following (lines 485-486): *"While we do not find a direct link between seismic activity and slope accelerations in our data, we recognize that seismic events can contribute to the development of instabilities through a preconditioning of the related slopes."* To make this clearer, we've added the following sentence to Section 4.3.2 to direct the reader to the corresponding discussion section (lines 362-363): *"While we acknowledge that seismic shaking can cause rock damage which impacts landslide stability (see Sect. 5.2), the evidence here shows no direct link between specific seismic events and landslide acceleration."*
1-23	How reliable is it to compare ice thinning thresholds between Alpine landslide/glaciers and landslide/Fjord glaciers? How relevant is 100m of thinning at Moosfluh to the cases in southern Alaska.	495	Thanks for this good point. We added an additional sentence to Section 5.2 to account for the distinction between alpine and maritime glaciers, as well as land- versus water-terminating ones (lines 458-459): *"However, these numbers cannot be directly compared to the Moosfluh case, which is an alpine glacier in very different climatic conditions, but may imply region-specific thinning thresholds."* We've also added a conceptual figure (Fig. 6) which deals with the land- versus water-terminating cases and discuss the differences in Section 5.1.
1-24	On this point, please always refer to the primary literature where phenomena/relationships are first reported and then the later studies that find confirmation – the Glueer et al article falls in the latter.	495	The citation for Glueer was left as-is since the authors defined the threshold at 100m, but we've added a Kos et al. (2016) citation to the statement about landslide activation following glacier thinning to a critical thickness. The sentences now read (lines 453-456): *"Kos et al. (2016) proposed that landslide activation may begin after the glacier thins to a critical thickness. For the Moosfluh landslide in Switzerland, for example, Storni et al. (2020) found that slope displacements were larger where ice thickness was below 50 m and smaller where the ice was ca. 100 m thick, while Glueer et al. (2019) found that the whole landslide accelerated after the ice thinned below 100 m."*
1-25	The several factors that the authors did not consider are central to the discussion, and therefore need to be included in a regional study.	560	The factors that the reviewer is referring to are: site-specific structural geology, slope hydrology, and snow height. As mentioned in the text (line 564 in the original manuscript), the structural geology is not something

			that can be determined from remote sensing data and is therefore outside of the scope of this paper. Additionally, slope hydrology is closely related to the fracturing of the subsurface and thus also cannot be inferred from remote sensing. To address the impact of snow accumulation on slope stability, we have added a plot looking at annual snow totals (derived from meteorological data) to the Appendix (Fig. G3), as well as the following sentence in the text (lines 492-495): *"Annual snowpack releases large amounts of water into the slope, which may impact stability. Detailed analyses on snow hydrology are outside the scope of this work, but an analysis of total solid precipitation over the time period 1980–2022 did not show a correlation with landslide activation (Fig. G3 in App. G)."*

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