We would first like to thank Dr. Tye for the time put forth in his careful review of our manuscript. As a third reviewer, we found some of his comments built on those from our first and second review but also caught important areas of improvement that speak to Dr. Tye's expertise. Below, we outline our direct responses to each of these comments and our improvements and changes. The final manuscript will be submitted separately to the journal editor for final editorial review.

The sans serif font in blue are Dr. Tye's comments The Serif font in black are our responses.

The manuscript by Matthew Morriss et al. provides an interesting case study of the Chaos Canyon Landslide (CCL) in Rocky Mountain National Park, including the character of the slope failure and its drivers. The manuscript is valuable for its application of a range of observational and modeling techniques to understand the event. The application of a wide range of techniques helps the authors to develop a comprehensive picture of this bedrock landslide, including the evolution of pre-failure creeping, and the volume of the slide mass. The authors explore the connection between the CCL and climate change, a topic of scientific and land-management interest. The authors generally do not overstate the significance of their results, which can only be speculatively connected to a climatic driver, but the manuscript would benefit from more critically assessing the potential mechanisms by which warming could have caused the CCL event. In addition to this point, I have several questions about the methods used and how the results are integrated, although I find no problems that jeopardize the validity of the authors' conclusions. In general, the manuscript presents a valuable case study and methodology that I believe will be of interest to the community and is appropriate for publication in ESurf after revision.

The weakest point of the manuscript is the connection between climate change and a CCL of the trigger. The occurrence of the CCL near the hottest time of the year is compelling in suggesting (speculatively) that temperature played a role in triggering the event. What is less clear is why the CCL occurred in the year that it did, which prevents clear establishment of a climate change-related mechanism. There is not a straightforward relationship between the behavior CCL and annual temperature, as 2022 was not an exceptionally warm year compared to the previous ~10 years, and the data do not resolve when pre-failure creep began. The authors explore both a reduction in interstitial ice and a meltwater-induced rise in groundwater as possible mechanisms for triggering the CCL. The hypothesis of significant interstitial ice reduction seems inconsistent with the authors' permafrost models, which indicate a maximum melting depth of <2 m in Summer 2022, an order of magnitude less than the depth of erosion (and thus minimum slip plane depth) indicated by the SfM analysis. Thus, the vast majority of the slide mass would still have been subject to freezing temperatures immediately before failure. The meltwater hypothesis is shown to be feasible through a simple factor of safety analysis, although the data and analyses presented don't establish how the magnitude and rate of meltwater produced in the area are likely to have fluctuated over the years, preventing assessment of any temporal trends that might explain the timing of CCL failure. It would be useful to see curves of annual precipitation and/or modeled meltwater production over time, if possible. Of course, pre-failure deformation of the CCL mass may have contributed to the timing of failure more than the specific conditions of 2022, but it is difficult to attribute this pre-failure activity to climatic forcing without better constraints on when it began. I don't see this issue as a fatal flaw for the manuscript, and the authors generally do a good job of stating that a climate forcing mechanism for the CCL is speculative. However, given the interest in this topic, I think the manuscript would be enhanced (and made more impactful) by a more in-depth discussion of potential climatic forcing mechanisms.

I have outlined some additional significant but less important points that would benefit from clarification below, in no particular order, with line edits following. In addition to these points, the manuscript would benefit from a close rereading to identify typographic errors, ensure that figures are consistent with their captions, revisit the order of figure calls, and ensure that all figure panels are referenced in the text. The authors should also consider making the field photos, analyzed satellite imagery, and SfM model available for the sake of reproducibility.

We would like to thank Dr. Tye for his thoughtful review of our manuscript. Many of his discussion points touch on similar issues that were raised by our second reviewer. I have outlined more detail responses below. Where necessary, I will refer to our responses to our second reviewer and how we modified our manuscript appropriately.

The weakest point of the manuscript is the connection between climate change and a CCL of the trigger. The occurrence of the CCL near the hottest time of the year is compelling in suggesting (speculatively) that temperature played a role in triggering the event. What is less clear is why the CCL occurred in the year that it did, which prevents clear establishment of a climate change-related mechanism. There is not a straightforward relationship between the behavior CCL and annual temperature, as 2022 was not an exceptionally warm year compared to the previous ~10 years, and the data do not resolve when pre-failure creep began.

This comment is very similar to the feedback received from Reviewer No. 2. 2022 was an unremarkable year for snowmelt as we showed in figure 6B. In some ways, it's remarkable that the collapse *did* occur in that year as opposed to past years. In the newly revised text, we now discuss the potential for multiple confounding factors that may have built through time: 1) decrease in permafrost that allows for more open pore spaces for water to infiltrate into the landslide; 2) weakening of the shear plane beneath the landslide, and 3) a continually less stable landscape position as the landslide deforms and translates into a steeper and steeper position above Chaos Canyon. These three factors allowed for an unremarkable amount of snowmelt in 2022 to raise the pore-fluid pressure to a point that helped push the landslide over into catastrophic failure. We cannot ascertain when the creeping movement of the landslide initiated, so we're unable to further comment on the relationship between on the longer lived motion of the landslide and climate change; however, we do draw upon other literature examples of mass wasting and connections with climate warming to posit that warming likely plays a role in the initiation and continued movement of this landslide. It is this movement that sets the stage for its ultimate collapse in 2022.

The hypothesis of significant interstitial ice reduction seems inconsistent with the authors' permafrost models, which indicate a maximum melting depth of <2 m in Summer 2022, an order of magnitude less than the depth of erosion (and thus minimum slip plane depth) indicated by the SfM analysis. Thus, the vast majority of the slide mass would still have been subject to freezing temperatures immediately before failure.

We appreciate the reviewers comment here regarding the presence of permafrost. It appears to have been unclear in our manuscript and we've now changed some of our text to specifically mention that this modeling does not necessarily mean there is permafrost across the entire landslide, but that there is the potential for intermittent permafrost. Factors such as snow accumulation on the upper portions of the landslide that would make permafrost formation more difficult to form and persist were not included due to lack of reliable data. This model served to indicate that there is the potential for internal and interstitial ice in the landslide deposit. We also showed an increasing temperature anomaly at the landslide site through time, which would lead to an increased instability of any potential permafrost that may be there. The ~1 m of depth for the thaw front on the date of collapse, provides support for the idea that by June 28th, melt water would have been able to penetrate the slide deposit and makes it way through the slide material by flowpaths no longer occupied by interstitial ice. Melt water would also introduce another pathway for melt of interstitial ice by more efficiently conducting warmer water into the slide (e.g. Vedie et al., 2011).

Additionally, geotechnical testing of soils that experience freeze thaw also indicate that even compacted soils that experience regular freeze thaw have a higher hydrologic permeability (Kim and Daniel, 1992; Qi et al., 2006; Vedie et al., 2011). So even as permafrost in places may serve as an obstacle to infiltration, the active freeze thaw processes provide more pore spaces in the shallow surface for snowmelt to infiltrate and travel along the landslide potentially following flow paths deeper into the slide.

In response to this review and the comments from Reviewer No. 2, we have modified the text of our manuscript.

References cited:

Kim, W.-H. and Daniel, D. E.: Effects of Freezing on Hydraulic Conductivity of Compacted Clay, Journal of Geotechnical Engineering, 118, 1083–1097, https://doi.org/10.1061/(ASCE)0733-9410(1992)118:7(1083), 1992.

Qi, J., Vermeer, P. A., and Cheng, G.: A review of the influence of freeze-thaw cycles on soil geotechnical properties: Freeze-thaw and Soil Properties, Permafrost Periglac. Process., 17, 245–252, https://doi.org/10.1002/ppp.559, 2006.

Vedie, E., Lagarde, J.-L., and Font, M.: Physical modelling of rainfall- and snowmelt-induced erosion of stony slope underlain by permafrost, Earth Surf. Process. Landforms, 36, 395–407, https://doi.org/10.1002/esp.2054, 2011.

The meltwater hypothesis is shown to be feasible through a simple factor of safety analysis, although the data and analyses presented don't establish how the magnitude and rate of meltwater produced in the area are likely to have fluctuated over the years, preventing assessment of any temporal trends that might explain the timing of CCL failure. It would be useful to see curves of annual precipitation and/or modeled meltwater production over time, if possible

We appreciate this comment and believe the text may already address it. The cumulative snowmelt curves are shown in Figure 6 Panel B going back nearly 30 years. I highlighted the individual last 5 years (including 2022). As was discussed in the response to Reviewer 2, 2022 was not an exceptional year from a melt volume perspective. It seems more likely that the previous years of movement aided in the development of a more distinct failure surface along with a weaker landscape position for the overall landslide deposit. Given the comment from the reviewer, Figure 6B appears sufficient. Moreover, the cumulative melt was calculated using the PDDS factor with a linear scaling, so the further temperature analysis displayed in Figure 11C is indicative of the temperature anomaly in preceding years.

Of course, pre-failure deformation of the CCL mass may have contributed to the timing of failure more than the specific conditions of 2022, but it is difficult to attribute this pre-failure activity to climatic forcing without better constraints on when it began. I don't see this issue as a fatal flaw for the manuscript, and the authors generally do a good job of stating that a climate forcing mechanism for the CCL is speculative. However, given the interest in this topic, I think the manuscript would be enhanced (and made more impactful) by a more in-depth discussion of potential climatic forcing mechanisms.

Given this comment from both Reviewer 2 and 3, we have bolstered our introduction and discussion sections with more text, and references, that catalogue the potential climatic mechanisms potentially at play in this landslide. We fully acknowledge, as the reviewer points out, that we cannot know when the failure began, which limits our insights into the slide at this time.

Other points

1. Composition of the CCL mass. The text is somewhat ambiguous as to whether the pre-failure mass was bedrock or regolith. Section 1.2 states, "The slide occurred along the contact between the Middle Proterozoic Silver Plume Granite and the early Proterozoic biotite schists." Does this mean that the slip plane is inferred to be the contact between these two units, or only that this is the geographic location where the slide occurred? The foliations mapped on Fig. A1 have dips similar to or less than the stated pre-failure surface slope of 40 degrees, consistent with CCL slip along a pre-existing foliation plane. Whether the pre-failure CCL material was bedrock or unconsolidated sediment would have implications for the failure mechanism—interstitial ice is probably less significant in igneous & metamorphic bedrock than sediment and foliation planes in bedrock might provide conduits for meltwater transport, so it is worth being more explicit about the composition of the slide mass. If the bedrock is important, consider adding the geology to Figure 2.

Thank you for this comment. It's possible that our description of the slide in the Introduction was not thorough enough or clear enough. We have revised the introductory text to make it clear that the landslide deposit which collapsed on June 28th, 2022 was a diamicton, or a deposit of poorly sorted material ranging in size from fine sediment too coarse boulders. Additionally, we have added The geology figure as Figure 2, highlighting the potential importance of the foliation. The text now reflects the potential contribution from a dipping foliation to accentuating the failure surface.

2. Intercomparability of the image correlation results. The image correlation techniques have an important role in the study, producing the only results that establish pre-failure movement of the slide material. Because of this, it would be valuable to see the image correlation results presented more systematically, including having similar figures for the different approaches taken with the Google Earth and PlanetScope imagery, such that the reader could evaluate the consistency of the results from the two imagery sources and distinct methods.

We appreciate the reviewers thoughts here; however, it's important to consider that these methods are collected from different satellites and from different points in time. We chose to keep this discussion of these two methods separate to reflect these differences and not confound the two separate methods for both collecting the data but also measuring deformation. We used the two methods in conjunction to best utilize their individual strengths. The Google Earth approach had very limited imagery options (just one pair of images to use) however the spatial resolution turned out very well for that image pair. The Planet imagery had many more options available meaning we could do temporal tracking, but the spatial resolution in each case was generally poorer than the Google Earth comparison. So each method gives something unique and complimented results from the other. We have added a figure that shows a direct comparison between the two methods to our supplement - per the reviewers recommendation.

3. Structured residuals in the SfM model. The difference model between the post-slide SfM model and the pre-slide topography (Figure 8) shows coherent differences outside the slide area. Areas downslope from the slide have negative differences and areas higher on the slope outside the slide have positive differences. I wonder if this reflects distortion in the SfM model, problems with registration to the DEM, or something else. This should be addressed in the text, along with any implications for the eroded and deposited volume estimates

The variations outside of the slide area are within the range of -5 to -15 meters downstream and +5 to +15 meters upstream (represented by light red and light blue colors in Figure 8). Your concern is valid as we've calculated an uncertainty of approximately 2.39 meters for this difference in the reference area (shown as a dashed polygon in Figure 8). We have three points of response to your comment:

1) The areas mentioned are distant from the mass movement, and the approach we used to center the terrestrial photogrammetry analysis might have introduced distortions away from the center of the landslide. This argument holds more weight when considering uncertainties upstream. Notably, the presence of a -5 to 5 meter margin around the landform provides reassuring evidence for higher accuracy within the landform itself.

2) The uncertainties arising from the terrestrial photogrammetry process result in an underestimation of volume differences, which has a somewhat conservative impact on our volume estimates.

3) The uncertainties could stem from the lack of Ground Control Points (GCPs) in the lower part of the area, downstream from the landform. a suitable GCP in this region couldn't be identified due to: (i) its proximity to the landslide, which lowered our confidence in selecting reference points in this area that were not affected by the movement, and (ii) its closeness to the scanning area, which led to occlusion of some potential bedrock surfaces by foreground relief.

Line edits

54-55 – reformat citation

I believe this is correctly formatted for the journal. I will make sure to check with the copy editors on this.

73 – redefine 'SfM' as this is its first use in the body of the paper

Fixed

125 – how was the environmental lapse rate calculated?

We have provided more details.

141 – how were the 305 photos collected from the 9 photo points (e.g., mosaic from each photo point location)?

These photos were individual frames collected with different shutter closures of the camera in use. There were a variety of number of photos taken from each site. They were then, as we describe in the text mosaiced using AgiSoft Metashape.

238-239 – rephrase to communicate greater confidence/reproducibility, e.g., "independent measurements of displacement of large boulders identified visually in the images are consistent with displacement magnitude inferred from image analysis"

Fixed to be consistent with the reviewers comment.

249 – change "1/velocity" to "inverse-velocity" or similar

Done

253 – rephrase "out of the ordinary" to "atypical" or similar

Done

263 – reintroduce what is being shown in this difference map and how it was obtained using a new topic sentence

Done

277-278 – revisit for syntax, degree symbol

Done

293 – add citations

Done

315 – I suggest eliminating the first clause as it is very different from where the paragraph is going

Done

346 – it appears that Fig. 12A only shows one mobility index, L/H

Fixed

350-351 – incomplete sentence

Fixed

352 – I think something is missing from the parenthetical note

Fixed

354 – reformulate to avoid use of contraction

Fixed

364 – citations needed or eliminate the reference to other scientists

Done

Figures

3 – panels C, D not called or discussed in text

We realize that not all of the panels were discussed in the text; however, we've opted to keep all four panels in the figure, which we have reorganized after our response to Reviewer 2 as these panels are consistent with InSAR result presentation common in the literature.

5 – what are the thin grey lines? How is velocity (panel B) calculated? It appears somewhat different from what I expect based on the slope of the displacement measurements in A. Also, are the points plotted at the date of the analyzed image each year?

The thin gray lines are the actual pixel displacement and velocity values. The velocity values were calculated using the central difference approximation. This is the most accurate way to perform a numerical derivative compared with forward differencing or backward differencing. The points are plotted on the same date as each image acquisition.

6 – replace "or" in first line of the caption with ", " if 3,668 m is the elevation of the top of the slide; revisit entire caption for spelling, capitalization.

Fixed

9 – the date of the slide is stated as June 29, in contrast with June 28 in the rest of the paper; I suggest adding something to state that the beginning of the hydrological year (0 on panel B x-axis) is not the same as the beginning of the calendar year

Fixed!

10 – colors did not come through for the version I received; caption states that the thick dashed line is limit of landslide material—is all the highlighted material in A the pre-collapse material or not?

The area shaded in gray represents the entire landslide deposit including material on its toe that did not fail on June 28th. The dashed line represents the approximate lower limit of material that failed on June 28th. We have added a supplementary figure in color and clarified the role of the dashed lines in the caption.

11 – define PDDS (both the acronym and how it is calculated); because snowmelt depends on both temperature and precipitation, it would be valuable to see annual precipitation plotted as well

PDDS is now defined in response to reviewer number 2 comments. I could not find an elegant way to display annual precipitation in a similar style to temperature; moreover, annual precipitation would confound both snowfall (accumulation) and rainfall (melting). It's not clear this would contribute to our overall analysis presented in Figure 11.

A1 – include geologic unit symbols

Figure A1 has now been moved to Figure 2 in the main text. More geologic symbols are included and properly defined in the caption.

I enjoyed reading the manuscript and think it will make a valuable contribution, and I encourage the authors to contact me with any questions or for clarification about the review.

Sincerely,

Alex Tye

alex.tye@utahtech.edu

We would like to thank the Reviewer for their comments that helped us make an even more robust manuscript and story regarding the Chaos Canyon landslide. We agree with the reviewer that this process will further aid us in understanding landslide processes in other alpine environments.

Best Wishes,
Matthew