Review Report for

Hydrothermal processes influence the along-strike morphologic variability of fault scarp profiles: applying supervised machine learning to Holocene scarps in the Stillwater Seismic Gap, Dixie Valley, Nevada, USA

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Round 1 Review

Comments by reviewer 1 ("anonymous")

Completed: 2025-10-15 06:54 PM
Recommendation: Major revisions
Reviewer Comments
For author and editor
The present paper is a case study of using geomorphologic variability on a fault that crosses a hydrothermally active area and seeks to demonstrate that low geomorphologic variability can be used to
Overall Finding. I recommend this paper be accepted pending major revisions.
Geomorphica offers some suggested question to be answered by reviewers. Here are some of those recommended questions and my answers:
Is the paper of value and interest to a significant proportion of the potential readers of Geomorphica? Yes. It shows how morphological variability may show the effect of hot springs and fumaroles on the morphology of geologically recent fault scarps.
Is the manuscript clear and easy to follow? Generally yes. Some figures need major revision, as described below. The methods section is hard to follow.
Is the manuscript's title adequate and accurate? Yes.
Is the abstract adequate? Yes.
Are the methods appropriate and described in sufficient detail to be transparent and reproducible? As to sufficient detail, no. The method is from another paper, and this paper is a case study application of that method. In order to understand the method, one must read the base paper's methods section. The authors believe the base paper's method does not use curvature of the profiles, and imply that this has some advantage over other methods. But a cursory glance at the method in the base paper quickly shows that it uses profile curvature foundationally.
Are the conclusions adequate and supported by the data? Uncertain. I express concerns below, and call for more explanation.
Is the paper unnecessarily long? No.
Comments by section.
Introduction The abstract and introduction clearly state this is a case study (lines 10, 93, and 368). Elsewhere it clearly references Brigham and Crider (2022) (B&C) multiple times. It could be made more clear that this paper presents a case study specifically of Brigham and Crider's method. Perhaps mentioning Brigham and Crider in line 93 as being the basis of the case study could make this stand out better. It could be as simple as "We present a case study based on Brigham & Crider (2022) from ...."
Geologic Setting Figure 1 caption, the word "are" is missing after the word "scarps" in the last sentence.
Methods Line 186 states that the method "does not rely on predefined morphometric indices (such as slope or curvature) ...". However, B&C specifically relies on curvature to identify the crest and toe of the scarp (B&C Line 229) and to find certain other key points before creating the matrix. Also, slope and curvature are not really morphometric indices, but may be the bases for scale-dependent terrain

indices. My quibble could be resolved just by saying "such as those based on slope or curvature". A similar revision should be made where this claim is repeated on Line 447.

Line 188. I recommend the sentence ending there read closer to "profile as the input for the classification step of the analysis." This change would avoid confusion on how much of the analysis is based on classification of the profile shape. Lines 190-193 Mentioning the high dimensionality of the whole profile draws attention to the question of what are the dimensions, and why is a collection of profiles (N, x, y, three dimensions, where N is profile number) a high-dimensional dataset? Also, if I understand the base paper's methods section, PCA and SVD are applied to each unit (i.e., contiguous collection of normalized profiles), and not to each profile individually, as stated at line 192/3. It may be too much to present in a case study why the dimensionality of the profiles is so high that it needs to be conditioned by PCA in order to be suitable for a Support Vector Machine. Perhaps the method's use of PCA and SVM can be mentioned without drawing attention to this high dimensionality. If not, then a sentence or two explaining what it is in an 3D dataset that has high dimensionality could be added.

Figure 2. This figure is helpful and looks nice. Consider adding light-grey or grey contours to Figure 2 panel-1. This will help readers understand how the criteria for determining the scarp limits relates to the terrain.

Figure 2. The legend shows items for Active, Recently active, and Inactive fumaroles. But only the fumarole temperature is shown on the panel-3 map. The actual fumarole point icons were left off. Please add these to the map.

Results Figure 3 (top of pdf page 10), I found very difficult to understand. After I realized it was a modification of Brigham and Crider (2022, B&C) Figure 7, then it all made sense. Most readers will not have the benefit of finding B&C Figure 7. The only part of Figure 3 which really can be considered "results" is the characteristic shapes of the profiles classes, so Figure 3a is helpful in that regard. However, to avoid confusion, I recommend bringing B&C Figure 7 forward intact into the methods section (as it is in B&C) without modification. Figure 3a would be fine as a distinct figure by itself if it really is part of the actual results. It would fit better between lines 268 and 269.

Figure 5. I wish to praise Figure 5. It is excellent in that it illustrates graphically what the method section was describing. It helped me a lot in understanding the method.

Discussion Paragraph at Lines 345-354. The discussion (and the introduction) seem to assume that the hydrothermal and and fumarolic processes took place after the Gap Event earthquake and were responsible for all of the reduction in the morphologic variability. Note, especially, Line 348 says "rapid erosion has homogenized or erased the original scarp form." But the images, Figure 4c i and ii appear to show color changes and morphology patterns consistent with extinct geothermal pools that formed relative to a different gravitational potential. That is to say, they are tilted and can't hold water now. Further, the geologic map of Figure 2 shows the fault location through unit C had to be inferred. Finally, when I look at the site of unit C in maps.google.com in 3D view, the terrain of unit C appears to be a continuation of a scarp so large it is not covered completely by the unit boundary shown in Figure 2. The left (start) boundary cannot really be defined by the change of slope and curvature criteria B&C use in their methodology. Please address more clearly how unit C was located given the apparent absence of a clear slope and curvature indicator as that used by B&C.

Lines 375-376. Though it is true that tracking morphologic variability does not require assumptions about geomorphic processes, it should be explained that it also cannot distinguish among different types of erosive geomorphic processes, and this limits the utility of this type of analysis for identifying hydraulic boundaries. Or, if my assertion here is mistaken, please address the concern.

Lines 428-430. This sentence should specify that the method requires only high-resolution topographic data.

Line 450. The sentence claims the morphologic variability method is adaptable to new landform types. This should be clarified or removed. Specifically, the method explained in detail in B&C intrinsically depends on automatically detecting the top and bottom of the scarp. The discussion needs to clarify what other landform types entail such a strict and automatable condition. In other words, what other landform types would submit to this type of analysis? This is requested because the method description in B&C is clearly for features along a corridor with a well defined start point and end point along the profile.

After Line 468 and before Line 469, one or two paragraphs should be added which discusses the sensitivity of the process to unit selection. The entire DEM, shown in Figure 2, spans a length of about 1100 meters, and the fault scarp is visible for about 600 meters. But the "control" units, A and B, are on the order of 100 meters in length each. A or B could have been located anywhere. Further, "unit D" could easily have been fit in somewhere. And the results for unit C are influenced by the results of A and B (but no D). How sensitive are these results to which part of the scarp are selected for analysis? Are there guidelines for selecting the locations of units? Would the results have been the same if the entire 600 meters of scarp were run as a single unit? These uncertainties cast doubt on the statistical integrity of the method. Please add one or two paragraphs here to address these general concerns (though not necessarily every question here).

Global Issues Review An important claim in this paper is at Line 118-9: "Our findings suggest that analyzing fault scarp morphologic variability provides a new tool for identifying [lithologic changes due to geothermal system modification] remotely."

This general sentiment is repeated elsewhere, including Lines 507-510, the concluding sentence of the paper.

As it is, the paper does not provide enough information to support this claim without more detailed qualification. For example, had the authors included the range front alluvial channel that crosses the fault (just west of unit B, see Line 210), would it not also have had a low morphological variability? And if it did, wouldn't that be a counterexample to finding such changes with the B&C approach? To clarify, the point is that hydrothermal processes may not be the only cause for a low morphological variability, and this should be stated somehow so as to pull back from the broader claim.

Comments by reviewer 2 ("anonymous")

Completed: 2025-10-20 08:34 AM
Recommendation: Minor revisions

Summary This manuscript uses the Stillwater Seismic Gap in Dixie Valley as a natural laboratory to quantify form and variability in fault scarp morphology as a function of offset landform lithology and geochemistry. The authors use a high-resolution UAV dataset to identify scarps in the Dixie Valley and, by taking scarp-perpendicular profiles, they classify the scarps into six classes with a machine-learning workflow with expert assistance, and compute a morphologic variability metric. The authors find systematic patterns in scarp form and morphologic variability that appear to depend on the host material. The paper is well written and puts forth an interesting dataset and methodology applied to it and I think will be appealing to the geomorphology and earthquake science communities alike. With that being said, I have some concerns about the assumption that the scarps examined are single-event scarps as I detail below. I think these can be addressed by the authors either through additional literature review or through examination of the morphology of the larger scarps they document. Because of this, I recommend minor revisions and subsequent publication. Main comments I am not sure the authors can confidently assert that the scarp formed during a single event, given the 2-2.5ka uncertainty in the paleoseismic data. The Fairview Peak-Dixie Valley earthquake sequence itself is evidence that multiple events can happen within a small area close in time. Those events would not have been separated paleoseismically. In addition, given the height of the largest scarps (7 m), I don't think it can be ruled out that they are composite. I think both of these points deserve more discussion/clarification either in the intro or the discussion. A potential way to explore this besides literature review is to look at the morphology of the tallest scarps and see if there are any indicators of multiple events present. 1 Do the authors have a sense from their analysis of whether it is possible to separate the presence of hydrothermal alteration in scarp formation versus scarp degradation? It's not clear whether the morphologic variability was largely the product of scarp formation in the different materials, with these materials eroding similarly, versus scarps having looked relatively similar coseismically but eroding very differently because of the material properties. The authors seem to conservatively point towards differences in erosion in section 5.1 as the favored case, but I'd like to see additional discussion in this section on how they expect the lithology to control scarp formation and then degradation. I don't see this as necessary to justify publication, but it could represent an important contribution of this work, as the influence of hydrothermal processes on coseismic scarp morphology affects probabilistic displacement hazard analysis, while their influence on scarp degradation shapes how paleoseismic data are factored in earthquake hazard models. Minor comments First paragraph of the intro: I think another form of first order variability in scarp height/morphology that ought to be mentioned here is coseismic slip variability - which will result in (even drastic) changes in scarp height even if the material is relatively consistent along strike. Line 133 extra space between 7 and m. Lines 222-224 - I recommend referencing Brigham and Crider here, since I had to go to the paper to see the detailed normalization approach. Could point readers to eq. 2 in Brigham and Crider (2022) for the details. Hydrothermal alteration is very outside my expertise so I hope a second reviewer can assess the descriptions associated with this in the manuscript! Figure 2 - is the geologic map from the literature or created by the authors based on field/drone observations? 2 I struggled to understand panel b in figure 3 without additional context. I looked at the Brigham and

Crider reference to see if that could help and found that version of the method reference figure much easier to follow. I suggest the authors adopt a similar approach, showing a sample scarp with the profiles along for panel b. Are the profiles detrended to remove the influence of pre-EQ topography? I could see some of the classes reflecting variability in pre-EQ topography and not being related to scarp formation or degradation processes. Line 323 - extra space between variability and Fault

Recommendation by Associate Editor 2 ("anonymous")

Completed: 2025-11-18 11:12 PM
Recommendation: Minor revisions
Reviewer Comments
For author and editor
The study uses high-resolution DEMs to extract cross-sectional profiles along a normal fault scarp, identified different classes of morphology, established quantitative metric for variability in the morphology, and then used this expert-defined dataset to train a automated model and extracted from profiles. The purpose of the paper was to demonstrate that hydrothermal alteration of rocks and sediments (regardless of alteration occurred pre- or post-faulting) can be detected using this automated tool. The implication of this study is that interpretations of fault scarps based on the assumption of uniform erosion post erosion should be challenged.
As AE2, I read the manuscript, checked out the data repository, and the two reviews. I agree with both reviewers that the manuscript is well written, concise, and the figures are well constructed. I also agree with reviewers that the scope of the manuscript is of value and high interest to the geomorphology community.
Reviewer 1 suggested that the manuscript could be rejected if other reviewers and editors also think so. No aspect of the manuscript meets the reason for rejection according to the Geomorphica's Review Guideline. Reviewer 1 ultimately recommended "major revision" mainly because they think the conclusions in the manuscript are not fully supported by the results. Based on the reading of Reviewer 1's comments, I think they want major revisions because: a) The manuscript makes unsubstantiated conclusions from the data/evidence presented but can still be fixed upon revision; b) Lack of error analysis, flawed error analysis, or lack of information about assumptions (reasons for major revision in the Review Guideline). I don't agree with this assessment. The authors did not make "unsubstantiated conclusions" and the authors explained the assumptions made in their analyses well. Particularly, Reviewer 1 judged that the main methods presented in Brigham and Crider (2022) used curvature a metric for morphological classes and that the authors claimed their method is better than previous studies using curvature to analyze fault scarps. I think Reviewer 1 is confused here. Brigham and Crider (2022) used *local* curvature in the DEM to identify the toe of a slope and the morphologic classes (the fundamental metric for analyses) were not determined based on the curvature of the *whole profile*. Perhaps this could be clarified in the revisions, but it does not warrant a major revision. The authors provided the DEM dataset in an open-source repository and the assumptions made by the authors can be tested. Reviewer 2 recommended a minor revision. I agree with this judgement.
To sum, Reviewer 1 was mostly concerned about the minimal description of the methods and thought the authors overclaimed that their method was better than past analyses of fault scarps. Reviewer 2 was mostly concerned that the variation of the profiles could be due to pre-fault hydrothermal alterations of the rock and sediment units and authors did not sufficiently demonstrate that the slope variations are only due to post-fault alterations. These are valid points and I suggest to the authors to take comments seriously and address all the concerns made by the reviewers. In other words, please make revisions suggested by the reviewers, instead of simply providing explanations without making changes to the manuscript.

Decision by Editor-in-Chief (Alice Lefebvre)

2025-11-25
Moderate Revisions

We would like you to undertake a moderate revision of your manuscript. As this is a moderate revision decision, your revised manuscript and the answer to reviewers will be first examined by the associate editor, who will decide if we need to invite one or both reviewers to look at the revised version. Therefore, please make sure to thoroughly and precisely answer all reviewers comments. The anonymous associate editor made some very detailed comments and suggestions on the reviews and how you can answer them. Make sure to follow their suggestions.

Authors' responses to review comments

2026-01-27

Manuscript: Hydrothermal processes influence the along-strike morphologic variability of fault scarp profiles: applying supervised machine learning to Holocene scarps in the Stillwater Seismic Gap, Dixie Valley, Nevada, USA

Journal: Geomorphica

We thank the reviewers and the Associate Editor for their constructive comments, which have helped us improve the clarity and rigor of our manuscript. Below we provide point-by-point responses to each comment. Reviewer comments are shown in **bold**, and our responses follow in regular text. Line numbers refer to the revised manuscript.

Response to Associate Editor

We thank the Associate Editor for their careful reading of the manuscript and their supportive assessment. We appreciate the clarification regarding the recommendation of minor revision rather than major revision, and the helpful interpretation of Reviewer 1's concerns.

Reviewer 1 judged that the main methods presented in Brigham and Crider (2022) used curvature as a metric for morphological classes and that the authors claimed their method is better than previous studies using curvature to analyze fault scarps. I think Reviewer 1 is confused here. Brigham and Crider (2022) used *local* curvature in the DEM to identify the toe of a slope and the morphologic classes (the fundamental metric for analyses) were not determined based on the curvature of the *whole profile*. Perhaps this could be clarified in the revisions...

We agree and have clarified this distinction in the Methods section (Lines 157-170). We now explicitly state that curvature is used in a preprocessing step to identify the scarp crest and toe, while the classification of profile form uses the full normalized profile shape rather than curvature values.

Reviewer 2 was mostly concerned that the variation of the profiles could be due to pre-fault hydrothermal alterations of the rock and sediment units and authors did not sufficiently demonstrate that the slope variations are only due to post-fault alterations. These are valid points and I suggest to the authors to take comments seriously and address all the concerns made by the reviewers. In other words, please make revisions suggested by the reviewers, instead of simply providing explanations without making changes to the manuscript.

We have substantially expanded our Discussion section to address the chronology of hydrothermal activity relative to the Gap Event rupture (Lines 326-341), drawing on radiocarbon dating from Lutz et al. (2002, 2003). We now clearly distinguish between pre-rupture sinter formation and ongoing fumarolic alteration, and discuss how these different chronologies affect scarp morphology.

Response to Reviewer 1

Overall Finding. I recommend this paper be accepted pending major revisions.

We thank the reviewer for their thorough evaluation and constructive feedback.

Introduction

The abstract and introduction clearly state this is a case study (lines 10, 93, and 368). Elsewhere it clearly references Brigham and Crider (2022) (B&C) multiple times. It could be made more clear that this paper presents a case study specifically of Brigham and Crider's method. Perhaps mentioning Brigham and Crider in line 93 as being the basis of the case study could make this stand out better. It could be as simple as "We present a case study based on Brigham & Crider (2022) from …."

We agree and have revised Line 81 to read: "We present a case study based on the morphologic variability method of Brigham & Crider (2022) from Dixie Valley, Nevada…"

Geologic Setting

Figure 1 caption, the word "are" is missing after the word "scarps" in the last sentence.

Corrected. The caption now reads (Line 763): "Hydrothermal features and scarps are from Lutz (2002) and Callahan et al. (2019)."

Methods

Line 186 states that the method "does not rely on predefined morphometric indices (such as slope or curvature) …". However, B&C specifically relies on curvature to identify the crest and toe of the scarp (B&C Line 229) and to find certain other key points before creating the matrix. Also, slope and curvature are not really morphometric indices, but may be the bases for scale-dependent terrain indices. My quibble could be resolved just by saying "such as those based on slope or curvature". A similar revision should be made where this claim is repeated on Line 447.

We thank the reviewer for this important clarification. The reviewer is correct that curvature is used in a preprocessing step to identify the scarp crest and toe (Brigham and Crider, 2022). However, the classification of scarp form itself is not based on curvature values but rather uses the full shape of the normalized profile. We have revised the text to clarify this distinction.

Lines 164-169 now read: "Our method does not rely on predefined morphometric indices (such as those based on slope or curvature of the whole profile) for classification, but rather uses the full shape of the scarp cross-sectional profile as the input for the classification step of the analysis. Each scarp profile is automatically cropped to the scarp toe and crest (curvature is used in a preprocessing step to identify the scarp crest and toe) and normalized by the scarp height…"

Line 188. I recommend the sentence ending there read closer to "profile as the input for the classification step of the analysis." This change would avoid confusion on how much of the analysis is based on classification of the profile shape.

Adopted. The revised sentence (Lines 166-167) now reads: "…but rather uses the full shape of the scarp cross-sectional profile as the input for the classification step of the analysis."

Lines 190-193 Mentioning the high dimensionality of the whole profile draws attention to the question of what are the dimensions… Perhaps the method's use of PCA and SVM can be mentioned without drawing attention to this high dimensionality.

We agree that the reference to "high dimensionality" may cause unnecessary confusion in a case study context. We have revised this section (Lines 171-177) to describe the method more clearly without emphasizing dimensionality:

"Each normalized scarp profile consists of many coordinate pairs. Principal component analysis (PCA, implemented via singular value decomposition) is applied to the collection of profiles to identify dominant morphologic traits and reduce the data to a few principal components suitable for classification. Using the principal component scores of the profiles as input features, we train a support vector machine (SVM) classifier. The SVM is a supervised learning algorithm well-suited for smaller datasets; it finds the optimal boundaries in the multi-dimensional feature space that separate the classes."

Figure 2. The legend shows items for Active, Recently active, and Inactive fumaroles. But only the fumarole temperature is shown on the panel-3 map. The actual fumarole point icons were left off. Please add these to the map.

We have added the fumarole point symbols to the map in panel 3.

<u>Results</u>

Figure 3 (top of pdf page 10), I found very difficult to understand. After I realized it was a modification of Brigham and Crider (2022, B&C) Figure 7, then it all made sense… I recommend bringing B&C Figure 7 forward intact into the methods section (as it is in B&C) without modification.

We have revised Figure 3 to improve clarity, including a schematic representation that better illustrates how profiles are arranged along a scarp section. The figure caption (Lines 771-775) has been updated: "Figure 3. a) Morphologic classes determined by inspection of scarp-normal profiles taken from each unit. b) Example of possible arrangement of profiles of different morphologic classes along a section of scarp (schematic 3D representation of profile arrangement). c) Schematic illustration of how morphologic variability is measured in a moving window along the scarp section (modified from Brigham and Crider, 2022)."

Figure 5. I wish to praise Figure 5. It is excellent in that it illustrates graphically what the method section was describing. It helped me a lot in understanding the method.

We thank the reviewer for this positive feedback.

<u>Discussion</u>

Paragraph at Lines 345-354. The discussion (and the introduction) seem to assume that the hydrothermal and fumarolic processes took place after the Gap Event earthquake and were responsible for all of the reduction in the morphologic variability… But the images, Figure 4c i and ii appear to show color changes and morphology patterns consistent with extinct geothermal pools that formed relative to a different gravitational potential. That is to say, they are tilted and can't hold water now.

We appreciate the reviewer raising this important point about the chronology of hydrothermal activity and its relationship to scarp formation. We would like to clarify that the visible alteration patterns in Figure 4c represent in-situ vadose zone alteration rather than tilted former geothermal pools. The color changes and patterns observed are characteristic of acid-sulfate alteration occurring within the unsaturated zone above the water table, where steam and gases rise from depth and condense, producing the observed clay-rich and iron-stained zones. This type of alteration forms in place and does not require ponded water at the surface.

We have added a new paragraph (Lines 326-341) clarifying the chronology:

"The chronology of hydrothermal activity relative to the Gap Event rupture differs between sections. In section B, radiocarbon dating indicates that the sinter deposits formed between approximately 3.4 and 2.5 ka (Lutz et al., 2002, 2003). The sinter terrace is broken and vertically offset by approximately 3-5 meters by the Gap Event fault, demonstrating that sinter formation predates the rupture (Lutz et al., 2003). The silicified material thus records pre-rupture cementation, and post-rupture degradation is expressed through block toppling and collapse of the cemented apron. In section C, fumarolic alteration began approximately 5 ka (Lutz et al., 2002) and continues today. The scarp in this section has experienced both syn- and post-rupture alteration, with ongoing acid-sulfate processes weakening materials and promoting rapid erosion. The visible color changes and alteration patterns in section C represent in-situ vadose zone alteration rather than tilted former geothermal pools; this type of alteration forms in place within the unsaturated zone where steam and gases rise from depth and condense."

Further, the geologic map of Figure 2 shows the fault location through unit C had to be inferred. Finally, when I look at the site of unit C in maps.google.com in 3D view, the terrain of unit C appears to be a continuation of a scarp so large it is not covered completely by the unit boundary shown in Figure 2. The left (start) boundary cannot really be defined by the change of slope and curvature criteria B&C use in their methodology. Please address more clearly how unit C was located given the apparent absence of a clear slope and curvature indicator as that used by B&C.

The reviewer raises an important point about the difficulty of defining the scarp in section C. We acknowledge that the scarp in the altered zone is difficult to trace and that automated identification of the crest and toe is challenging. In this section, the scarp slope relaxes until it reaches a change in lithology, creating what is effectively a fault-line scarp rather than a fault scarp in the strict sense.

We have added discussion of this issue in the Results section (Lines 234-237): "The scarp slope relaxes until it reaches a change in lithology, creating a fault line scarp (Figure 4c.i) or is assimilated into the larger hillslope (Figure 4c.ii). In this section, we supplemented the curvature-based algorithm to determine the crest and toe with field observations and manual verification."

We have also added discussion in Section 5.1 (Lines 321-322): "Scarp slopes have relaxed into the background hillslope or terminate abruptly at lithologic boundaries, creating fault-line scarps where altered material meets a more competent unit."

Lines 375-376. Though it is true that tracking morphologic variability does not require assumptions about geomorphic processes, it should be explained that it also cannot distinguish among different types of erosive geomorphic processes, and this limits the utility of this type of analysis for identifying hydraulic boundaries.

The reviewer is correct. We have added clarifying text (Lines 360-366):

"Tracking morphologic variability does not require assumptions about geomorphic processes; however, it also cannot distinguish among different types of geomorphic processes. Instead, clear changes in morphologic variability over short distances along a single fault scarp coincide with mappable distinctions in material properties, which suggest different erosive properties at work. This case study thus addresses the influence of material properties on form patterns rather than identifying specific properties."

Lines 428-430. This sentence should specify that the method requires only high-resolution topographic data.

Revised (Lines 410-412) to: "…the morphologic variability metric offers a complementary method that requires only high-resolution topographic data and can map boundaries of hydrothermally-modified zones."

Line 450. The sentence claims the morphologic variability method is adaptable to new landform types. This should be clarified or removed. Specifically, the method explained in detail in B&C intrinsically depends on automatically detecting the top and bottom of the scarp… what other landform types would submit to this type of analysis?

We have revised this section (Lines 429-434) to specify appropriate landform types:

"The morphologic variability method is adaptable to new sites or landform types, since new morphological types can be added without retraining a large model. Appropriate landform types include linear landforms with defined start and end points along cross-sectional profiles such as terrace risers, shoreline and paleo-shoreline scarps, levees, coastal cliffs, moraine crests and volcanic fissure scarps."

After Line 468 and before Line 469, one or two paragraphs should be added which discusses the sensitivity of the process to unit selection… How sensitive are these results to which part of the scarp are selected for analysis?

We have added a new paragraph (Lines 463-474) addressing unit selection sensitivity:

"The sensitivity of our results to area selection within each unit merits discussion. Area A was selected as a baseline: a scarp in alluvial/colluvial material with known models of diffusion-based erosion (e.g., Wallace, 1977; Bucknam and Anderson, 1979; Nash, 1980; Hanks, 2000). Areas B and C were defined based on mappable lithologic distinctions (silicified materials and hydrothermally altered materials, respectively). We deliberately avoided areas affected by alluvial fans or dissected by rangefront channels to isolate the influence of material properties on scarp form. The morphologic variability metric is designed to detect relative changes along the scarp, so the absolute boundaries matter less than capturing representative sections of each lithology. If the entire ~600 m of scarp were analyzed as a single unit, the method would still detect variability changes, but the lithologic associations would be less clear without the a priori subdivision by material type. Future applications might analyze entire scarps continuously and use changes in the variability metric to identify previously unknown lithologic or structural boundaries."

<u>Global Issues</u>

An important claim in this paper is at Line 118-9… the paper does not provide enough information to support this claim without more detailed qualification. For example, had the authors included the range front alluvial channel that crosses the fault (just west of unit B, see Line 210), would it not also have had a low morphological variability? And if it did, wouldn't that be a counterexample to finding such changes with the B&C approach?

The reviewer makes an excellent point. We agree that low morphologic variability could result from processes other than hydrothermal alteration, including channel dissection or other geomorphic overprinting. We have qualified our claims in the unit selection discussion (Lines 467-468): "We deliberately avoided areas affected by alluvial fans or dissected by rangefront channels to isolate the influence of material properties on scarp form."

<u>Response to Reviewer 2</u>

Summary: This manuscript uses the Stillwater Seismic Gap in Dixie Valley as a natural laboratory to quantify form and variability in fault scarp morphology… I recommend minor revisions and subsequent publication.

We thank Reviewer 2 for their positive assessment and constructive suggestions.

<u>Main Comments</u>

I am not sure the authors can confidently assert that the scarp formed during a single event, given the 2-2.5ka uncertainty in the paleoseismic data. The Fairview Peak-Dixie Valley earthquake sequence itself is evidence that multiple events can happen within a small area close in time.

We appreciate this important concern. We note that the 1954 Fairview Peak and Dixie Valley earthquakes, while close in time (occurring within minutes of each other), ruptured separate faults and did not produce repeat ruptures on the same scarp.

Paleoseismic investigations by Caskey (2002) and Wesnousky et al. (2003) indicate that the Gap Event "represents the only surface rupture along the range front in the last 12 ka" (Caskey, 2002). Trench studies in a lateral-spread graben near Dixie Comstock constrained the age of the causative earthquake to between 3.7 ka and 2.0 ka, with additional constraints from faulted sinter deposits narrowing this to 2.5–2.0 ka (Wesnousky et al., 2003; Lutz et al., 2003). While the trench studies demonstrate a single event in the Holocene record, we acknowledge that they do not directly rule out the possibility of multiple closely-spaced ruptures that would be indistinguishable in the stratigraphic record.

From Caskey (2002, p. 91): "Exploratory trenching on the scarp near Willow Canyon and about 5 km north of the quarry revealed that Qfm, Qbg, and lower Qfy deposits are offset 1.5-2.5 m by a single pre-1954 event (Bell and Katzer, 1987, 1990). The event produced much larger displacements in the Stillwater gap area where it represents the only surface rupture along the range front in the last 12 ka."

From Wesnousky et al. (2003, p. 8): "In summary, if we combine the constraints from the trench and the faulted opaline sinter found along the range front fault to the north, then the age of the most recent fault movement along the fault south of the DVGF is constrained to have occurred between 2.0 and 2.5 ka."

In addition, given the height of the largest scarps (7 m), I don't think it can be ruled out that they are composite.

This is a reasonable question given the observed scarp heights. The Gap Event is associated with a large-magnitude (Mw ~7.3) earthquake, as estimated from the 45+ km rupture length and average slip of 2-4 m (Wesnousky et al., 2003). A single-event displacement of 7 m is definitely at the upper end of typical values, but it is not unprecedented for Basin and Range normal faults (see text and Figure 6 in Pérouse and Wernicke, 2015). Furthermore, there is certainly error related to using our automatic scarp height measurement algorithm as a strict measure of fault displacement, since it is also influenced by geomorphic features/processes (for example, if talus is deposited at the base of a scarp that cuts a tilted terrain, the toe of the scarp will be appear to be lower in elevation and the measured scarp height will be greater).

We have added a sentence in the Geologic Setting section (Lines 117-120) to highlight the consensus that the scarp is due to a single event: "Paleoseismic investigations indicate that these scarps represent the only surface rupture along this section of the range front in the last 12 ka, with the Gap Event constrained to between 2.0 and 2.5 ka (Caskey, 2002; Wesnousky et al., 2003; Lutz et al., 2003)."

I think both of these points deserve more discussion/clarification either in the intro or the discussion.

We have added discussion (Lines 267-271) addressing the possibility od a compound scarp and our reasoning as to why this methodology still works in that case:

"Regardless of whether the scarp represents a single event or multiple ruptures, all three sections we examined experienced the same rupture history. Therefore, the distinct morphologic variability values we document for each section cannot be attributed to differences in rupture history and must reflect differences in material properties and post-rupture geomorphic processes."

Do the authors have a sense from their analysis of whether it is possible to separate the presence of hydrothermal alteration in scarp formation versus scarp degradation?

This question gets at a fundamental challenge in interpreting scarp morphology in hydrothermally active settings. We have added discussion of this distinction and the likely chronology of hydrothermal activity and faulting at this site (Lines 326-341):

"The chronology of hydrothermal activity relative to the Gap Event rupture differs between sections. In section B, radiocarbon dating indicates that the sinter deposits formed between approximately 3.4 and 2.5 ka (Lutz et al., 2002, 2003). The sinter terrace is broken and vertically offset by approximately 3-5 meters by the Gap Event fault, demonstrating that sinter formation predates the rupture (Lutz et al., 2003). The silicified material thus records pre-rupture cementation, and post-rupture degradation is expressed through block toppling and collapse of the cemented apron. In section C, fumarolic alteration began approximately 5 ka (Lutz et al., 2002) and continues today. The scarp in this section has experienced both syn- and post-rupture alteration, with ongoing acid-sulfate processes weakening materials and promoting rapid erosion. The visible color changes and alteration patterns in section C represent in-situ vadose zone alteration rather than tilted former geothermal pools; this type of alteration forms in place within the unsaturated zone where steam and gases rise from depth and condense. Distinguishing the relative contributions of formation versus degradation processes remains challenging, but the contrasting morphologic variability values suggest that material properties at the time of rupture (whether strengthened by cementation or weakened by alteration) exert a primary control on subsequent scarp evolution."

Minor Comments

First paragraph of the intro: I think another form of first order variability in scarp height/morphology that ought to be mentioned here is coseismic slip variability - which will result in (even drastic) changes in scarp height even if the material is relatively consistent along strike.

We agree this is an important point and have added mention of coseismic slip variability (Lines 50-54): "...fault scarps can exhibit significant along-strike variability in form, particularly in jointed bedrock (e.g. Brigham and Crider, 2022), reflecting lateral differences in scarp characteristics such as coseismic slip variability, rock mass characteristics, fault structures, or degree of geomorphic modification..."

Line 133 extra space between 7 and m.

Corrected (Line 117): "...2-3 m, up to 7 m."

Lines 222-224 - I recommend referencing Brigham and Crider here, since I had to go to the paper to see the detailed normalization approach. Could point readers to eq. 2 in Brigham and Crider (2022) for the details.

We have added a reference (Lines 169-170): "...and the processed profile coordinates form the basis of the classification (see Brigham and Crider, 2022, equation 2 for normalization details)."

Figure 2 - is the geologic map from the literature or created by the authors based on field/drone observations?

The geologic map was created by the authors based on field observations and interpretation of the SfM-derived imagery. We have added a statement to the figure caption clarifying this.

I struggled to understand panel b in figure 3 without additional context. I looked at the Brigham and Crider reference to see if that could help and found that version of the method reference figure much easier to follow. I suggest the authors adopt a similar approach, showing a sample scarp with the profiles along for panel b.

We have revised Figure 3 to improve clarity, including a schematic representation that better illustrates how profiles are arranged along a scarp section.

Are the profiles detrended to remove the influence of pre-EQ topography? I could see some of the classes reflecting variability in pre-EQ topography and not being related to scarp formation or degradation processes.

The profiles are not explicitly detrended. Our method focuses on the scarp face itself (between the automatically detected crest and toe) rather than the broader landscape. Pre-existing topography could influence scarp form, particularly in section C where the scarp merges with the background hillslope. We have added acknowledgment of this issue in the discussion of fault-line scarps (Lines

321-323): “Scarp slopes have relaxed into the background hillslope or terminate abruptly at lithologic boundaries, creating fault-line scarps where altered material meets a more competent unit.”

Line 323 - extra space between variability and Fault.

Corrected.

Round 2 Review

Recommendation by Associate Editor 2 ("anonymous")

Completed: 2026-02-19 08:28 PM
Recommendation: Accept
Reviewer Comments
For author and editor
This is the first revision of the manuscript after getting comments from two reviewers and an associate editor. The main complaints from the reviewers were:
Reviewer 1:
- Clarify in the Methods sections about the use of curvature in identifying the crest and toe of scarps.
- The methods of this study relied on reading the details in Brigham and Cider (2022). Indicate that this is a case study for Brigham and Cider (2022).
- Can this method be applied to other types of landforms?
Both reviewers:
- Fault in Section C (see Figure 2) is not clear, therefore, clarify and add details to indicate that the scarps are results of a single rupture rather than reworked topography.
- Clarify and add details to Figure 3 to make it clear about the classification of the profiles.
During the initial review, an associate editor assessed that the comments from the reviewers were valid and reasonable and suggested to the authors to make substantial changes to the manuscript to resolve the issues raised by the reviewers.
I think the authors did make substantial changes to the manuscript, addressed all the concerns raised by the reviewers by adding new paragraphs in the Methods and Discussion sections. Overall, the authors revised or added clarifying text that accounts for ~12% of the initial manuscript text. I also commend the authors' effort to make substantial changes to Figures 2 and 3. For example, the authors added the updated fields of fumaroles and temperatures in Figure 2 and added detailed examples of slope classifications in panel b of Figure 3. I also would like to commend the authors' efforts in providing their high-resolution raster and vector data in an open repository, which conforms to the Geomorphica's guideline for data accessibility.
In sum, I think the revised manuscript can be published as submitted. Obviously any typos and grammar will be checked during the copy-editing stage. I am suggesting very minor changes listed below that can be made during the copy-editing. The line numbers below refer to the line numbers in the revised manuscript without the edit track:
Ln 168–169: I suggest changing the phrase to "... step to identify the crest of and toe of a scarp) and..."
Ln 332: It would be useful to reference Figure 2 as "... cemented apron. In section C (Figure 2)", just to remind the readers where things are.
Figure 3: please increase the font size of Vs1 and Vs2 etc.

Decision by Editor-in-Chief (Alice Lefebvre)

2026-02-20
Accept

Congratulations! I am pleased to inform you that your manuscript entitled "Hydrothermal processes influence the along-strike morphologic variability of fault scarp profiles: applying supervised machine learning to Holocene scarps in the Stillwater Seismic Gap, Dixie Valley, Nevada, USA" has been accepted for publication in Geomorphica.

I will now assign a copy editor who will get in touch with you soon to further process your manuscript. You will be required to upload the final version of your manuscript, tables, images, supplementary material, non-English abstracts and alt text for all your figures, tables and equations to follow our new guidelines to help screen-reader accessibility.

I would appreciate it if you already include the small copy editing suggested by the anonymous AE before uploading the final version of your manuscript.