Subject: [Seismica] Editor Decision

Dear Malinda Zuckerman, Alba Mar Rodriguez Padilla, J Ramon Arrowsmith:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Quantifying the erasure of earthquakes in desert landscapes". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after some revisions.

Please see the attached comments from the two reviewers. It is clear that both are enthusiastic about the work and supportive of further consideration for publication at Seismica. In general I would describe the requested revisions as "moderate". I find that the reviewer comments are exceptionally clear in this case, and as such, I will not belabor them with additional commentary or summary here. I am confident that considering these points in revision will make for stronger submission, and I look forward to seeing the the results once available. As always, thank you for considering Seismica for publication of this work. I found the manuscript very interesting.

Please note that the revised version must also adhere to Seismica's length restrictions (10,000 word and 10 display items). When you are ready to resubmit the revised version of your manuscript, please upload:

- A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
- A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
- A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.

Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either needs to be sent to reviewers again, requires further minor changes, or can be accepted.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Randy Williams

Dear Dr. Randolph Williams and Reviewers,

We thank the reviewers for their thoughtful comments, which have greatly improved the clarity and content of our manuscript. Below, we respond in detail to each of the concerns and suggestions brought up by Reviewer A (Nadine Reitman) and Reviewer B (Christoph Grützner). The editor did not provide additional comments beyond those of the reviewers.

The three most substantial changes to the manuscript are: (1) an expanded conclusions section with quantitative results for simple and complex fault zones, providing the detail requested by Reviewer A; (2) expanded discussion in Section 5.2 of the limitations of the slope change coefficient ϕ, including a distinction between stochastic surface processes, erosional features, and persistent geomorphic features; and (3) the addition of Ponti et al. (2020) mapped rupture lines overlaid on the DEM panel in Figure 7, with accompanying text in Section 4.3, suggested by Reviewer B.

Additional revisions include corrected citations, clarified notation, and several minor additions to the discussion. We also added a couple of sentences to Section 5.4, reconciling the differences between studies that predict fault location using pre-rupture data and our study, which models mappable fault length using a known fault location. The fault zone width value in the abstract was also corrected to match the results section.

We look forward to your feedback on the revised manuscript.

Sincerely,
Malinda Zuckerman, Alba Mar Rodriguez Padilla, and Ramon Arrowsmith

Dear Malinda Zuckerman, Alba Mar Rodriguez Padilla, J Ramon Arrowsmith:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Quantifying the erasure of earthquakes in desert landscapes". Thank you once again for submitting your work to Seismica.

Based on reviews I have received, your manuscript may be suitable for publication after some revisions.

Please see the attached comments from the two reviewers. It is clear that both are enthusiastic about the work and supportive of further consideration for publication at Seismica. In general I would describe the requested revisions as "moderate". I find that the reviewer comments are exceptionally clear in this case, and as such, I will not belabor them with additional commentary or summary here. I am confident that considering these points in revision will make for stronger submission, and I look forward to seeing the the results once available. As always, thank you for considering Seismica for publication of this work. I found the manuscript very interesting.

Please note that the revised version must also adhere to Seismica's length restrictions (10,000 word and 10 display items). When you are ready to resubmit the revised version of your manuscript, please upload:

A 'cleaned' version of the revised manuscript, without any markup/changes highlighted.
A pdf version of the revised manuscript clearly highlighting changes/markup/edits.
A 'response-to-reviewers' letter that shows your response to each of the reviewers' points, together with a summary of the resulting changes made to the manuscript.
Once I have read your revised manuscript and rebuttal, I will then decide whether the manuscript either needs to be sent to reviewers again, requires further minor changes, or can be accepted.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

I wish you the best with working on the revisions. Please don't hesitate to contact me with any questions or comments about your submission, or if you have any feedback about your experience with Seismica.

Kind regards,

Randolph Williams
University of Wisconsin-Madison
randolph.williams@seismica.org

Reviewer A:

Review of Zuckerman et al for Seismica

Zuckerman et al use simple landscape evolution models based on 2D diffusion to investigate timescales of surface rupture degradation for vertical fault scarps formed in earthquakes in desert environments, using the 2019 Ridgecrest and 2010 El Mayor-Cucapah earthquakes as examples and for validation. They find that with increased time post-earthquake, identifiable and mappable fault length decreases, measurable fault zone width decreases, fault zones appear more simple, and the magnitude of those decreases is highly dependent on the complexity of the original surface rupture.

This is a novel take on a persistent question in tectonic geomorphology, and this study makes a neat contribution to the literature. It is unique from prior work and adds a key piece to understanding how earthquake scarps appear in a landscape decades to millennia after an earthquake. The primary areas for improvement are adding discussion on the limitation of modeling only diffusion of the landscape, making the language more precise, and perhaps adding some nuance to the real-world applicability of the slope change coefficient metric. I think this work will be ready for publication with moderate revisions.

I very much enjoyed reading this manuscript. Thanks for the opportunity to review, and I hope these comments are helpful. – Nadine Reitman

General Comments:

The primary limitation of this work is the choice to model all scarp degradation by linear diffusive processes alone. As a landscape evolution modeler, I completely understand and am sympathetic to the need to simplify models to make them useful and amenable to interpretation. The issue is not the approach, but the lack of discussion in the manuscript of this limitation and how it might affect the results. The authors do an excellent job of also evaluating non-linear diffusion and how that affects the results, but they do not consider the other processes that alter fault scarps, particularly vertical ones. In line 276, the authors state that field investigation of the Ridgecrest scarps reveals modification by “diffusive processes, sheetwash, overland flow, and rilling”, as well as the addition of wind-blown sediment. Please add a bit of discussion of the limitations of considering diffusion alone, and how other surface processes might alter or affect the results – either at the end of the methods or in the discussion.
We agree this is an important limitation and have addressed it in two places in the manuscript. First, we added a sentence to the introduction (L91-93) acknowledging that more complex surface process idealizations are possible (e.g., runoff-driven erosion and mass wasting), while noting that diffusion captures important landscape changes and balances conceptual clarity and computational expedience with sufficient process realism. Second, we expanded Section 5.1 (L331-335) to explicitly acknowledge the limitations of diffusion-only modeling. Specifically, we

note that nonlocal idealizations (e.g., Gray et al., 2025) account for larger-motion processes important early in scarp evolution, such as ravel and sheetwash, and that at larger timescales the details of the implementation matter less as forms smooth and information is lost.

Slope change coefficient may be less useful in practice where landscapes are both diffusive and erosive. New channels are cut quite frequently in natural desert environments, increasing slope locally. Also, vegetation changes quite rapidly (even in desert environments), which complicates slope change calculations from SfM-derived topography data. The authors mention these limitations in a few places and explore vegetation change with noise renewal, but the conclusion offers that "The slope change coefficient is a simple topographic metric that may be useful to assess the state of a faulted landscape." This a bit hard to believe for real, natural landscapes. Perhaps if the landscape evaluated is clipped to around a fault trace, but even then rilling may change the balance of slope change over time, and two digital topographic datasets at very high resolution and at least decades apart would be needed to determine what the slope change coefficient is for a natural landscape.
We agree that the slope degradation coefficient has important limitations in real, natural landscapes where erosional processes such as rilling and channel incision renew and increase surface roughness independently of the tectonic signal.

We have expanded the discussion in Section 5.2 (L361-371) to distinguish between three categories of non-tectonic roughness: stochastic processes such as vegetation growth and death that renew over time; erosional features such as rills and new channels that develop and persist, locally increasing slope; and geomorphic features such as boulders and cobbles that contribute persistent background roughness changing only on very long timescales. We acknowledge phi as a useful first-order reconnaissance metric while noting its limitations for small scarps in noisy landscapes.

We also added a clarifying note that while keeping consistent DEM dimensions around the scarps mitigates bias from including flat adjacent areas, it does so at the cost of diminished sensitivity to the scarp signal (L261-262).

Finally, we added a sentence to the conclusions identifying future directions (L453-456), including empirical noise characterization, spectral analysis to isolate scarp signals from erosional features, and repeat topographic differencing.

Results show that 20–80% of original trace length remains after 10k years, which is a huge spread. It's probably worth underscoring that point in the discussion and possible conclusion – that the amount of mappable fault trace remaining after landscape diffusion depends heavily on the complexity of the original surface rupture.
We agree this point deserves more emphasis. We added a sentence to Section 5.3 (L377-379) explicitly noting that the wide range of remaining mappable trace length reflects the strong control of original rupture complexity on degradation rate — simple, single-strand fault zones retain mappable traces far longer than complex, multi-strand zones where distributed secondary scarps erode rapidly.

The expanded conclusions (L 438-443; see response to the comment on the conclusion below) also reinforce this point, now explicitly stating that for simple, single-strand fault zones roughly 72% of the original trace length remains mappable at 10,000 years, while for complex, multi-strand fault zones up to 77% of mappable length is lost between 1,000 and 10,000 years.

The manuscript would be stronger with a clear statement of how this work applies to strike-slip versus vertical fault scarps. The example datasets are primarily strike-slip ruptures with oblique components, and this research focuses on the vertical scarps. Please add a statement to the discussion about how these results are/are not applicable to different types of fault scarps.
We now address this in the discussion (L343-348) as follows:

“Our models simulate diffusive processes on vertical scarps from predominantly strike-slip earthquakes. This was the product of choosing to focus on the effect of surface processes in eroding scarps in the desert landscapes of southern California and northern Mexico. This work could be expanded to normal faults under some considerations. Because coseismic normal fault scarps tend to be taller than the vertical scarps resulting from strike-slip events, non-linear diffusion will appropriately describe the system for a longer time period than for strike-slip faults, meaning our linear approximation would be less appropriate over a longer timescale in the early years after an event.”

The conclusion seems a bit hastily written and lacks detail. Suggest incorporate more detailed information on the results of the work, such as the spatial and temporal scales of degradation and loss of mappable fault length (e.g., For simple fault scarps, XX% of the fault is still mappable after XX years. For complex scarps, XX% of the rupture is still mappable after XX years. On average over the studied locations, mappable fault length and fault zone width decrease by XX% and XX% per year/decade/century/millennia, respectively). Also consider including the impact of the work, which is nicely stated in the non-technical summary. Finally, consider stating how the slope change coefficient is calculated (if it can be done succinctly) since it is being suggested as a useful metric for future studies.
Thank you for this suggestion. We agree that the original conclusion was underdeveloped and have substantially revised it. We now include specific quantitative results: for simple, single-strand fault zones, roughly 72% of the original trace length remains mappable at 10,000 years, while for complex, multi-strand fault zones, up to 77% of mappable length is lost between 1,000 and 10,000 years. We also added that mean fault zone width decreases from ~40 m to ~2 m over 10,000 years across all 20 study sites. We have incorporated a statement on the broader impact of the work, noting that recognizing landform degradation is essential for interpreting the geologic record and improving probabilistic fault displacement hazard models. Finally, we added a brief definition of the slope change coefficient ϕ and a reference to equation 4, so that readers can readily understand and apply the metric.

How were the equations used to fit mappable line length, fault zone width, and slope change coefficient evolution over time (equations 5, 6, 7) chosen? Were other functions tested and these were found to fit best?

We chose these equations based on analytical convenience. Further work could establish equations that are rooted in the processes of the evolution of mappable line length, fault zone width, and degradation coefficient capture, but this is beyond the scope of our work. We now clarify that our fits are chosen on the basis of analytical convenience:

L236-237: “We choose equations to describe the evolution of the degradation coefficient, the fault zone width, and the mappable fault length based on analytical convenience.”

In general, the writing could be made more precise in places. See suggestions in the line comments. Some common cases: use “diffusion” instead of “surface processes” because surface processes also encompass erosion. Use “fault trace” instead of “trace”. Use “study locations” instead of “landscapes” when discussing the specific sites where analyses were performed.
We have addressed this and now refer to surface processes as diffusion throughout the manuscript other than in the introduction. We have also added the word “fault” before trace throughout.

Line Comments:

Line 14: fault segmentation and slip distribution aren’t so much “recorded as landforms” as they are “partially recorded by landforms”. Or perhaps, “which are partially recorded in the landscape”, since distributed deformation that can signal shifts in slip distribution or fault segmentation are not necessarily part of specific landforms but perhaps distributed across a few km of the landscape as distributed cracking.
L14: We have changed “recorded as landforms” to “which are partially recorded in the landscape”, as suggested.

Non-technical summary: As is so common with non-technical summaries, this is an excellent summary of the work and makes the impact more easily digestible than the abstract, but the language is still very technical, especially in the first 2-3 sentences. Non-technical audiences are unlikely to be familiar with the terms “surface rupture” and “slip distribution” and won’t know what “fault geometry” is or that “surface processes” essentially means diffusion by gravity and erosion by water. Suggest also change “rupture landforms” to “earthquake landscapes” (line 32).
We used your suggestions to clarify the non-technical summary to be more accessible. Specifically, we changed “Surface rupturing earthquakes” to “Large earthquakes”, “fault geometry and slip distribution” to “the location of the fault and how the fault moved”, “surface processes” to “erosion and gravity”, and “rupture landforms” to “earthquake landscapes”.

Suggest incorporating this sentence to the abstract: “This progressive loss of detail makes older rupture zones appear simpler and more localized than they originally were.”
We added the idea behind this sentence, “This progressive loss of detail makes older rupture zones appear simpler and more localized than they originally were,” to the abstract and made minor cuts to keep the abstract under 200 words. The new clause incorporating that sentence now reads, “Results show that 20–80% of original trace length remains after 10k years and fault

zone width decreases from a mean of 40m to ~2m, causing older rupture zones to appear narrower and less complex than initially."

Line 43: Not quite sure what "mechanical relaxation" means here. Gravitational collapse?
We refer to the process of postseismic relaxation of the bulk or afterslip. We have modified mechanical to postseismic on L46 to clarify:

Much of this information is recorded in the landscape coseismically and modified over time by surface processes and postseismic relaxation (Figure 1).

Line 47: Suggest replace "surface process vigor" with "surface process rates"
L50: Done.

Line 56: Correct citation for the Quaternary fault and fold database is: U.S. Geological Survey, 2020, Quaternary Fault and Fold Database for the Nation, accessed [Month, Day, Year], at https://doi.org/10.5066/P9BCVRCK
(https://www.sciencebase.gov/catalog/item/589097b1e4b072a7ac0cae23)
L59: We made this change.

Lines 73-75: These two sentences mostly repeat each other
L74-77: Thanks for catching that! We have removed the first one.

Lines 96-97: Suggest add "undertaken many years post-earthquake" after "remote and field mapping efforts"
L101-102: Done.

Line 105: insert "for detailed analysis" at the end of the first sentence of this paragraph.
L110: Done.

Line 112: Is there evidence that anthropogenic activities also contribute to degradation of the Ridgecrest scarps, or only natural surface processes?
There was evidence of erosion from driving (tracks) over the scarp at some locations. We avoided these sites for analysis, but now mention it in the text immediately after the sentence regarding surface processes:

L118-119: "We also observed degradation due to anthropogenic processes (e.g., tracks in Figure 7), but do not consider it in our analysis."

Line 142: Insert "scarp degradation" after "due to" and before "surface processes"
L152: Done. We have replaced surface processes by scarp degradation, since the sentence otherwise doesn't make sense.

Line 143: “hillshade” à technically, hillshade is the algorithm and shaded relief is the resulting map (I only know this distinction thanks to the USGS review and approval process). Your choice on changing it or not, as the community understands what is meant by ‘hillshade’.
L153: We opt for the more appropriate option and have changed hillshade to shaded relief in the text.

Line 144: Was the mapping all performed by the same person? Wondering about consistency across sites.
All the mapping was completed by the same person. We now specify this in the methods:

L155: “The mapping was completed by one person.”

Line 148: “systematized” à “systematic”
L159: Done.

Line 149: How/where were the topographic profiles used? I.e., systematic profiles across each site, or …?
We recognize that the profiles were not fully addressed. To clarify, we changed this step to, “We created hillshade and slope derivatives and topographic profiles from the DEMs. We mapped the morphology of each DEM using the derivative maps **and profiles** by tracing observed topographic convexities and concavities” on L162.

Line 178: Change “surface processes” to “diffusion”. Erosion is a powerful surface process that is not taken into account here.
L192: Done.

Line 179: add “fault” between “simple” and “trace”
L193: Done.

Line 201: Could omit “The prior section narrates two individual examples from our experiments.”
L217: Done.

Line 204: Could omit “All simulations were linearly diffused with a k of 1 m2/kyr.” as it is in the methods.
We opt to keep it as a reminder in case readers are not following the paper linearly, as it’s a key assumption for the results.

Line 208: Replace “landscapes” with “study locations” (or something similar)
L224: We replaced “landscapes” with “analysis locations”.

Line 242: Not sure what “landscape renewal” means. Given Figure 10, perhaps “noise renewal” is more appropriate?
L260: We now say noise renewal in the landscape to clarify:

“In Section 5.2, we explore the effect of noise renewal in the landscape on the slope change coefficient with synthetic DEMs.”

Line 258-261 – This paragraph makes an excellent point, but it probably fits better in a discussion than a results section.
We deleted this paragraph from this section (L277) and stated this interpretation in section “5.4 Implications for hazard assessment,” as we think it is a point that is helpful to practitioners. We rewrote it to better fit the hazard assessment context: (L398) "Degraded fault traces can lead to underestimation of the fault zone width and the extent of distributed deformation, impacting PFDHA and seismic hazard assessments, especially on immature strike-slip faults such as those in the ECSZ, where distributed fracturing is an important component of hazard."

Line 286: Replace “surface processes” with “diffusion” or “degradation”
L307: We replaced “surface processes” with “degradation”.

Line 318: This might be a natural place to discuss other limitations of the diffusion only modeling scheme for vertical fault scarps. (see first general comment)
Done, see response in first general comment.

Line 328-332: I’m a bit confused how Figure 10B supports the statements in these lines. It appears that the slope change coefficient is more variable (and larger!) when noise renewal is included for short scarps and steady (and smaller) with noise renewal for taller scarps, and steady without noise renewal, regardless of scarp height. Degradation of short scarps would lead to less slope change overall than tall scarps if the rest of the landscape is the same, but the slope change coefficient is larger for the short scarps, which seems that perhaps the signal is dominated by other features of the landscape? Perhaps part of my confusion is the definition of slope change coefficient. Based on equation 4, larger slope change coefficient means greater slope change (that is, more smoothing), which makes sense with Figure 10C, more smoothing and higher coefficients as time progresses. However, that logic doesn’t seem to hold up for the noise renewal case of short scarps. Perhaps it would be useful to calculate slope change coefficient for the area around the scarps and compare how that evolves relative to the landscape as a whole?
Thank you for this careful observation. You are correct that in the noise renewal case,ϕ is larger for small scarps than tall scarps at late timesteps, which is counterintuitive if one expects ϕ to scale with scarp height. This occurs because ϕ is calculated across the entire DEM rather than in a window around the scarp. For small scarps, the renewed noise dominates the slope distribution relative to the scarp signal, inflating ϕ in a way that does not reflect scarp degradation alone. For tall scarps, the scarp contributes enough to the slope distribution that ϕ behaves more intuitively even with noise renewal.

We intentionally calculate ϕ landscape-wide rather than in a scarp-focused window. A key motivation for this metric is that it could serve as a reconnaissance tool for assessing landscape degradation state without prior knowledge of scarp location. Restricting ϕ to a buffer around the scarp would require knowing where the scarp is, which defeats this purpose. We acknowledge, however, that this design choice makes ϕ less sensitive for small scarps in noisy landscapes, and we have added text at L362 to clarify this limitation explicitly. “We calculate ϕ across the entire DEM rather than in a window around the scarp, so that it could serve as a reconnaissance tool for assessing landscape degradation state without prior knowledge of scarp location. This makes ϕ less sensitive for small scarps in noisy landscapes, and we acknowledge this as a limitation of the metric in its current form. Nevertheless, ϕ can reflect tectonic signal loss at the landscape scale and remains a useful first-order metric for assessing degradation state.”

Line 349: formalize “something like”
L391: We replaced “something like” with the approximation sign.

Line 370: Please elaborate – how would the slope change coefficient be known or calibrated for a landscape? It seems that repeat lidar/SfM would be needed with a time span far apart enough to quantify landscape change. Given the slow rate of diffusion (here modeled as 0.001 m2/yr = 1 mm/yr), that seems unlikely for most modern landscapes with the resolution of most older topographic datasets.
Thank you for this important clarification. You are correct that direct empirical calibration of ϕ through repeat topographic surveys is not feasible for most landscapes given the slow diffusion rates modeled here. At L419, we have revised the text to clarify that ϕ is most useful as a forward-modeling or reconnaissance tool when regional diffusivity k is independently constrained, as it is for the ECSZ, rather than as a field-calibrated metric requiring repeat imagery. We also note that scarps of known historical age within the same landscape could serve as reference points for model validation. We acknowledge that absolute age estimation from ϕ alone remains a significant challenge and have accordingly softened the language in the discussion and conclusions.

Line 383: Suggest revise this sentence “We have used a simple landscape evolution model to simulate landscape change and then mapped the fault traces.” to à We used a simple landscape evolution model to simulate landscape change following earthquakes and recorded the resulting change in mappable fault traces.
L434: Done.

Figure Comments:

Figure 1: Beautiful figure! One minor, nitpicky comment: the secondary fault that appears at the surface as “secondary rupture cracks” is at a somewhat odd angle to the primary fault. Of course, Ridgecrest had minor faults/cracks at many angles, so also fine to leave as it, but it certainly gave me pause.
Thank you! We choose to keep it as is to honor the natural randomness in the crack orientations from the Ridgecrest earthquakes, but acknowledge it’s a funky orientation!

Figure 2: The caption should make clear that the Hudnut and OpenTopo citations are for the shaded relief topography, and not for the rupture mapping linework. In the legend, consider

changing “landscape locations” to “analysis locations”, as the entire area is the Ridgecrest or EMC landscape.
We added the rupture citations and separated the topo and rupture references in the caption text.

Figure 3: In the caption change “systematized” to “systematic”
Done.

Figure 4: Right-most column is missing Y axis labels. Define ϕ in the caption. Consider labeling the columns at the top to aid in quick figure digestion. Is the width in the second column the max width or the mean width? Suggest put the slope percentile and delta z scales in each row as there is plenty of white space. Are the slopes in the right-most column average across the landscape or only around the fault or over a transect? Please specify.
We have added a label to the y axis of the fourth column, defined phi in the caption, specified that the fault zone width is a maximum, and specified in the caption that the slopes include the entire landscape.

Figure 5: See comments for Figure 4. All of them apply.
Done.

Figure 6: Consider changing X axis labels in a, b, and c to “Time since earthquake (years)”. Since the plots in d, e, and f were made with seaborn, are they kernel density estimates (KDEs – a type of PDF), and if so, please state the kernel type used (Gaussian is the default).
Done, we now specify that we use a Gaussian kernel.

Figure 7: Consider changing “softened” to “smoothed” in the caption, and state date(s) of field photo and SfM photo acquisition.
Done (both).

Figure 10: Suggest revise x-axis labels in panels B and C to “Time since earthquake (years)” or “Years post-earthquake” or similar.
Done.

Figure S1: This figure illustrates the loss of information really well with a nice set of real-world examples and sets up the primary point of the study. Consider making this Figure 1 or Figure 2 of the primary text.
We appreciate this feedback but, unfortunately, we have maxed out the number of figures allowed by Seismica already at 10. Therefore, we choose to keep this figure as supplemental.

Figures S2 through S21: Consider comments for Figure 4.
Done.

Recommendation: Revisions Required

Reviewer B:

Review

The manuscript "Quantifying the erasure of earthquakes in desert landscapes" by Zuckerman et al. deals with the geomorphic imprint of surface-rupturing earthquakes in arid environments. Surface ruptures decay over time, so important information on past earthquakes vanishes. This manuscript explores landscape evolution models to quantify this information loss. The authors use surface rupture datasets from two recent large earthquakes in western North America and high-resolution topographic data covering those areas. The Landlab toolkit was fed with the surface rupture and DEM data and simulated linear diffusive decay of the ruptures at 0, 100, 1000, 5000, and 10000 years from now. The team then mapped fault traces on all of the resulting DEMs. The mapping results were then analysed. The authors show that fault complexity decreases over time and so does fault width. Also, mappable fault length decreases. Testing for non-linear diffusion instead of a linear diffusion model provided similar results. The authors introduced a new slope change coefficient ϕ as a measure of information loss. They conclude that fault mapping needs to take into account this information loss, meaning that we probably always underestimate fault width and fault complexity, except for very recent earthquake events. This is an issue for Fault Displacement Seismic Hazard Analysis, but also for capturing the full deformation field of paleo-earthquakes. The findings reported here are an important step forward towards better understanding and quantifying uncertainties in paleoseismology in my opinion.

The topic is worth of investigation, and it is well-suited for the journal. The manuscript will attract readers from the fields of tectonic geomorphology, active tectonics, seismic hazard analysis, paleoseismology, earthquake geology, and landscape evolution modelling.

The manuscript is easy to read, logically structured, neither too short nor too long, and well-illustrated. The supplement contains all the detailed data, but the main article works very well as it is. I found the mission statement clearly formulated, the methods nicely explained, and all the observations well-documented. The methods are absolutely suitable for investigating the topic. The authors' interpretations are supported by the data, and the conclusions are not too far-fetched. Also, the abstract describes the content of the manuscript very well. Referencing is up to date.
Thank you!

I only have very minor suggestions to improve this already very good manuscript.

- Some very minor issues with the referencing are highlighted in the annotated PDF.

We have addressed those throughout the text and in the reference list. We note specifics in the following line edit comments.

- I encourage the authors to better explain what diffusivity actually is and why it matters to choose the right value for it. I think this is important for readers not too familiar with the topic.
We addressed this comment – please see our response to comment L128b.

- There is some repetition in sections 3.1 and 3.3 that should be avoided.
We addressed this problem – see the line comment for L162-163.

- I wonder if the decrease in mapped line length with increasing time after the earthquake suffers from artifacts due to the chosen boundary conditions. Looking at all the data in the supplement, I find that in many cases (not all) the mappable sections disappear towards the borders of the analysed area. For example, in E1, R3, R4, and R6-R10. This makes me wonder if there are perhaps effects due to the choice of boundary conditions that make the ruptures disappear at the area boundaries.
Please see our comment regarding line 208 below.

I am looking forward to seeing the manuscript published.

Christoph Grützner

Jena, 2025-10-16

Recommendation: Revisions Required

Line edits:

L50-51: “allow for much of the geomorphic signal from previous events to be lost”
Do you really mean “lost”, not “preserved”?
“Low surface process rates” mean slow modification, and I guess “limited vegetation” also results in slow modification by roots etc. So the traces should be preserved for long. “Moderate EQ recurrence intervals” could mean both. (Would be good to quantify “moderate” here - perhaps state something like: “...in the order of xyz of years…”)
L54: We corrected a logical error in the sentence. In desert environments like the ECSZ, low surface process rates and limited vegetation promote preservation of the geomorphic signal from past earthquakes, not loss. Changed "lost" to "preserved."

L86: 2000?
L89: Thank you for catching this citation manager mistake. We corrected the year of the Hanks book chapter citation from 2013 to 2000. The citation key has been updated throughout the manuscript.

L124: ∇qs I think because qs is the sediment transport rate
L131: We corrected the description of q_s. It is the sediment transport rate, and ∇q_s is its spatial divergence. The previous wording incorrectly described q_s itself as the divergence.

L128a: please make sure the reader understands the difference between k and ks (as in Eq. 2)
L135: We corrected a typo in Equation 2. k_s should kS, where k is the diffusivity coefficient and S is the local topographic slope, consistent with the linear diffusion transport law described in the text and following Dietrich et al. (2003).

L128b: Here the reader needs to be told what diffusivity actually is and why a value of 1 m²/kyr is a good choice (= data from previous studies).
Actually, Arrowsmith et al. (1998) cite other papers for the value of k=1: "Morphologic dating of terrace edges,
shorelines, and fault scarps in the semiarid environments of Utah, Nevada, Idaho, and Montana produced k values of ~1 m²/kyr [Hanks et al., 1984; Nash,1984; Pierce and Colman, 1986]".So maybe only cite those papers here because Arrowsmith at al. (1998) actually found k=8.6 for their Carrizo Plain study.
L138: Following your observation that Arrowsmith et al. (1998) found k=8.6 m²/kyr for the Carrizo Plain rather than k≈1, we corrected the citations supporting k=1 m²/kyr for semiarid environments. We now cite Hanks et al. (1984), Nash (1984), and Pierce & Colman (1986), who document k≈1 in semiarid settings in Utah, Montana, and Idaho, respectively, as well as Hanks (2000).

L136: We also added a brief definition of *k* as a rate constant describing the efficiency of sediment transport by diffusive processes, so that readers unfamiliar with the parameter understand its physical meaning before encountering the morphologic age discussion that follows.

L129: 2000?
L140: Thank you again for catching this. See note at L86.

L162-163: repetition, see line 115
L152: We removed the repeated opening sentences of Section 3.3, which were identical to the opening of Section 3.1 (L122). We replaced the line with a brief orienting sentence previewing the three metrics defined in this section.

L179: add (R6)
L193: Done.

L180: add (E10)
L194: Done.

Figure 4 caption: add (orange line)

Added "(orange line)" after "fault zone width" in both captions to clarify which element in the hillshade panels represents the mapped fault zone width, as suggested.

L208: Looking at all the data in the supplement, I find that in many cases (not all) the mappable sections disappear towards the borders of the analyzed area. For example, E1, R68-10. This makes me wonder if there are perhaps effects due to boundary conditions.
We thank the reviewer for this careful observation. We investigated the boundary conditions in our Landlab implementation and confirmed that they do not introduce artifacts in the mapped fault trace lengths. We also reviewed the supplementary figures and note that fault traces do not systematically disappear at DEM borders across all sites. No changes to the manuscript were made.

Figure 5 caption: add (orange line)
Added "(orange line)" after "fault zone width" in both captions to clarify which element in the hillshade panels represents the mapped fault zone width, as suggested.

L247-248: Make clear that this is the orange line in figs. 5 and 6
L267: We added a reference to the orange line in Figures 5 and 6 to clarify that the fault zone width measurement corresponds to that element in the figures, as suggested.

Figure 7: I downloaded the surface rupture kml by Ponti et al. (2020) and it looks as if there was another, parallel strand of ruptures mapped here, which I hardly see on the DEM (should be close to the bottom of the image here). Perhaps it would be good to add a panel to this figure showing the mapped ruptures as lines overlaying the hillshade?
We thank the reviewer for this suggestion — we had not considered overlaying the mapped ruptures on the DEM panel and think it significantly improves the figure. We added the Ponti et al. (2020) mapped rupture lines as an overlay on panel D in magenta. The parallel strand identified by the reviewer is indeed mapped in the Ponti et al. dataset but is barely visible on the DEM, which is consistent with the central argument of this paper — secondary and parallel strands degrade rapidly and become difficult to detect even just 4.5 years after the earthquake. We have updated the figure caption accordingly and added a sentence to Section 4.3 (L303) noting that the Ponti et al. (2020) dataset documents this parallel strand, which is barely detectable on our DEM.

Figure 8 caption: I think you have to refer to equation (8) here; it took me a while to understand what the parameter "n" is in the figure
We updated the caption to reference both equations (3) and (8), so that the parameter n appearing in the figure legend is defined for the reader (n is introduced in equation 8).

L343: As far as I see, Yuan et al. (2022) did not use deep learning or other automated methods.
L385: Removed Yuan et al. (2022) from the citation supporting automated detection methods, as that study does not use deep learning or other automated mapping approaches. The citation for this claim now refers only to Sare et al. (2019).

References: Hanks 2013: I think the original paper is perhaps from 2000:
https://pubs.usgs.gov/publication/70185741
Your doi-link here also leads to a 2000 publication.
Yes, this is the correct citation. See note at L86.

Dear Malinda Zuckerman, Alba Mar Rodriguez Padilla, J Ramon Arrowsmith:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Quantifying the erasure of earthquakes in desert landscapes". Thank you once again for submitting your work to Seismica.

The two original reviewers have now had time to review your revised manuscript. As you will see, both seem to be very satisfied with the changes made. I will be happy to officially accept the manuscript for publication once the following items are resolved:

1) Please address the remaining minor comments from the reviewers. In particular, note some of the comments provided by Reviewer B, who indicates that there may be old versions of the figures still in place in the revised manuscript.

2) Please provide original source files for the manuscript text (latex or word) in addition to separate, high-resolution versions of each figure for typesetting.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

As always, thank you for considering Seismica for the publication of this work. I look forward to seeing it in print.

Kind regards,

Randy Williams

Reviewer A:

Nice revisions! This study seems ready for publication, just a few very minor comments:

Line 23: consider changing “initially” to something like “immediately after an earthquake”

Line 24: Can “slows” be quantified? For example, is it 2x or 10x slower?

Line 59-60: The rupture maps in these databases are actually quite variable from earthquake to earthquake. Some are incredibly detailed and others quite general.

Line 180: Consider removing new paragraph starting here

Figures 4 and 5 are still missing y axis labels in column 4.

Line 380: Should “within ~ kt > …” be “<”?

Recommendation: Accept Submission

Reviewer B:

This is the revised version of the manuscript by Zuckerman et al.: “Quantifying the erasure of earthquakes in desert landscapes”. The authors addressed both my remarks and the more extensive ones by the other reviewer. I find the manuscript in very good shape. It has improved in many small points and especially discussion and conclusions are clearer and more conservative now. I think the manuscript should be accepted.

The author should, however, please check if the manuscript has the latest versions of the figures. In their reply to the reviewers the authors mention a couple of changes to Figs. 4 and 5, like added labels to the Y-Axes in the fourth column, but I can’t see any changes to these figures.

With Fig. 6, it’s not 100% clear to me if the authors followed the other reviewer’s suggestion to change the X-axis labels. If so, the old version of the figure is still in the manuscript.

Christoph Grützner

Jena, 2026-04-08

23 June 2026

Dear Randy Williams,

Thank you again for your handling of these revisions on our manuscript, "Quantifying the erasure of earthquakes in desert landscapes". Thank you for the second pass. The manuscript has improved from further scrutiny — especially from the reviewers noticing we forgot to include the updated figures.

We have addressed the reviewers’ comments below in blue and included changes and notes in the marked-up PDF. The most substantial change to the manuscript was the addition of the updated Figures 4, 5, and 6. We also updated the code in our Zenodo repository to include the figure changes.

Sincerely,

Malinda Zuckerman, Alba Mar Rodriguez Padilla, and Ramon Arrowsmith

Dear Malinda Zuckerman, Alba Mar Rodriguez Padilla, J Ramon Arrowsmith:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Quantifying the erasure of earthquakes in desert landscapes". Thank you once again for submitting your work to Seismica.

The two original reviewers have now had time to review your revised manuscript. As you will see, both seem to be very satisfied with the changes made. I will be happy to officially accept the manuscript for publication once the following items are resolved:

1) Please address the remaining minor comments from the reviewers. In particular, note some of the comments provided by Reviewer B, who indicates that there may be old versions of the figures still in place in the revised manuscript.

2) Please provide original source files for the manuscript text (latex or word) in addition to separate, high-resolution versions of each figure for typesetting.

If you deem it appropriate, please check that the revised version of your manuscript recognises the work of the reviewers in the Acknowledgements section.

Please note that Seismica does not have any strict deadlines for submitting revisions, but naturally, it is likely to be in your best interest to submit these fairly promptly, and please let me know of any expected delays.

As always, thank you for considering Seismica for the publication of this work. I look forward to seeing it in print.

Kind regards,

Randy Williams

Reviewer A:

Nice revisions! This study seems ready for publication, just a few very minor comments:

Line 23: consider changing “initially” to something like “immediately after an earthquake”
We made this change.

Line 24: Can “slows” be quantified? For example, is it 2x or 10x slower?
It is not possible to simply quantify this rate of change in the abstract. Given that the process assumption is diffusive, it really also depends on the curvature of the landscape elements, and is not linear in change initially. That it slows is correct, and so we have left it unchanged.

Line 59-60: The rupture maps in these databases are actually quite variable from earthquake to earthquake. Some are incredibly detailed and others quite general.

Very true. We added a clause at the end of the sentence addressing this: “...though the level of detail varies among earthquakes depending on data availability and mapping resolution.”

Line 180: Consider removing new paragraph starting here
We made this change. There is a note on L180 in the marked-up copy.

Figures 4 and 5 are still missing y axis labels in column 4.
Our mistake for not including updated figures. Labels and titles are added now. We did not add scale bars under each row, as we preferred to include some white space.

Line 380: Should “within ~ kt > …” be “<”?
Great catch. We made this change. There is a note on L382 in the marked-up copy.

Recommendation: Accept Submission

Reviewer B:

This is the revised version of the manuscript by Zuckerman et al.: “Quantifying the erasure of earthquakes in desert landscapes”. The authors addressed both my remarks and the more extensive ones by the other reviewer. I find the manuscript in very good shape. It has improved in many small points and especially discussion and conclusions are clearer and more conservative now. I think the manuscript should be accepted.

The author should, however, please check if the manuscript has the latest versions of the figures. In their reply to the reviewers the authors mention a couple of changes to Figs. 4 and 5, like added labels to the Y-Axes in the fourth column, but I can’t see any changes to these figures.

With Fig. 6, it’s not 100% clear to me if the authors followed the other reviewer’s suggestion to change the X-axis labels. If so, the old version of the figure is still in the manuscript.
Updated Figures 4, 5, and 6 are now included.

Christoph Grützner

Jena, 2026-04-08

Recommendation: Accept Submission
