Dear Steve, Daniel and Reviewer B,

Thank you very much for your time and comments regarding our submission.

Our replies to your comments, suggestions and corrections are in this blue text style.

A couple of specific questions we have for the editor are signaled by the word "**EDITOR**" in bold.

We also made additional grammar and language corrections, and added some text and references for clarification and completeness, mainly in the context of changes made in response to your comments. We also found that the 2007 Alum Rock earthquake also ruptured southeast along the S Calaveras fault; we added mention of this to the S Calaveras relocation section and to the discussion of curved faults and rupture directivity.

Best regards,

Anthony and Pierre

[Seismica] Editor Decision

2023-03-16 05:42 PM

Dear Anthony,

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Major California faults are smooth across multiple scales at seismogenic depth". Thank you once again for submitting your work to Seismica.

I am pleased to say that I have now received two peer-review reports for your manuscript. Both reviewers feel that your results are thought-provoking, and they are both broadly supportive of your work being published in a Research Article format in Seismica. However, they suggest that some revisions are needed before publication.

*In particular, both reviewers flag up the potential implications of your results for more complex, less mature fault systems in California. Reviewer A refers to the San Jacinto fault, and Reviewer B raises the possibility of using a known complex rupture, such as the 2019 Ridgecrest sequence, as a test to see whether the apparent resolved smoothness is real. Although I understand that the title of the paper specifically refers to *major* faults in California, accounting for some of this end-member complexity might be important to at least discuss.*

See replies to reviewers on these issues. We now further discuss the immature, Mount Lewis seismicity as showing complex, orthogonal damage zone faulting, clarified our discussion of the immature Monte Cristo sequence, and have added analysis and comparison with Hauksson et al. 2022 of the complex, 2021 Calipatria sequence in the Brawley Seismic Zone. We have in many places replaced "mature" with "major".

These sorts of discussions might be particularly pertinent and timely considering the recent earthquake sequence in Turkey. In addition, thinking aloud, I am also wondering if there is somehow a parameter that can be used to describe the level of smoothness/roughness of these relative earthquake catalogues so that more quantitative comparisons than be made without visual inspection; this might not be easy to quantify, but I thought it was worth considering.

We do this with the SVD fit of planes for Parkfield, which is a special case since most of the seismicity is clearly (visually!) associated with a single plane, and for Mount Lewis (where the main rupture is isolated from extensive damage zone seismicity. In both cases the fit (mean absolute deviation) is similar to or less than the likely relative location errors for nearby events (especially for NLL-SSST-coherence), so it does not seem meaningful to use such measures to quantify fault surface roughness (or the width of a core damage zone) beyond stating that the faults are not rough (i.e. smooth) over many scales. We also provide the (visual) comparison of part of the Southern Calaveras seismicity with a circular arc. But in general performing quantitative measures associating seismicity to a smooth surface of a main rupture surface is very difficult, because either there is much nearby damage zone or splay seismicity (e.g. Southern Calaveras) that would have to be arbitrarily excluded from the measure, or the potential surface is defined by the boundary of volume-filling seismicity (e.g. SW San Francisco), which seems to be quite a difficult computational geometry problem.

Please find below the comments some further specific comments from me, along with the review reports from Reviewers A and B.

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.

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.

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.

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,

Stephen Hicks

Thanks very much for the positive decision on our manuscript, and for your time, very helpful comments, suggestions and advice on the submission.

Specific comments from the editor:

Abstract

- L26-27: I am a bit confused by this sentence "… while differential-timing relocation improves fine-scale but not larger, multi-scale precision". Could you please check and consider re-wording.

DONE. Changed to “while differential-timing relocation mainly improves the finest scale precision”

- L33-36: This is a very long sentence – could it be split into two sentences or reworded?

DONE. Changed to “Our relocated seismicity at seismogenic depth along major fault segments and around large-earthquake ruptures often defines smooth, narrow, planar or arcuate, near-vertical surfaces across the sub-km to 10’s of km scales. These results show that multi-scale smooth fault segments are characteristic of major, strike-slip fault zones and may be essential to large earthquake rupture. Our results suggest that smoothness and curvature of faults influences earthquake initiation, rupture, rupture direction and arrest, and can define earthquake hazard.”

Introduction

- L84: Please change “planer” to “planar”.

DONE.

Methods

- I assume that there are same number of events in the reference catalogues (e.g., NCSN-DD) compared with NLL-SSST-Coherence, but please confirm this.

The number of NCSN-DD relocated events is typically less than the number of NCSN or NLL-coherence-SSST event since hypoDD relative location is only performed for events with a minimum number of high cross-correlation connections to nearby events. We have noted this in the introductory paragraph to section 3, and specified the number of events in the captions for figures with NCSN-DD events plotted.

Discussion

- L453: Earlier in the Introduction, it is stated that SSST is best-suited for cases where there is a large variation in raypaths across a study area and the seismicity distribution covers a large area relative to the stations. However, the Discussion (L453) says, “for cases of poor station and ray coverage, NLL-SSST-coherence may produce higher relative location accuracy and better depth control than do cross-correlation based, differential-timing methods”. On first impression, these two statements sound slightly contradictory, especially regarding the raypath coverage. Could you please clarify.

In the methods section 2 our objective is to *inform* the reader how SSST (not just NLL-SSST) works *in theory and in ideal cases* – we use the term “SSST corrections are most effective”. In the Discussion we are noting how the full NLL-SSST-coherence procedure (not just SSST) works *in practice* (e.g., never ideal station coverage or data availability) *relative* to other methods, as indicated by the Mendocino example in the current paper. We attempt to clarify these qualitative differences in objective by modifying the methods section sentence with: “Spatial-varying, SSST corrections are most effective *for improving relative locations on all scales* when…”, and the Discussion sentence with “NLL-SSST-coherence may ***retain*** higher relative location accuracy and better depth control”, where we replaced “produce” with “retain”.

L515-516: Could you please mark the approximate 1906 rupture on the appropriate figure?

The 1906 rupture is coincident with and defines the San Andreas Fault in the map area. Thus it seems not necessary (or possible) to mark separately the rupture. Instead, we have noted this relation in the text :"… the onshore surface trace of the San Andreas Fault ***(SAFZ and cyan line in Figure 2)*** …" and in the figure caption: "… the SAFZ south of San Francisco, ***where the SAFZ and 1906 rupture is coincident with the cyan line***.".

L518: Please change "planer" to "planar".

DONE. Here and elsewhere(!).

L605: Please change "might identified" to "might be identified".

DONE.

L611: Please change "These relations provides …" to "These relations provide …"

DONE.

Figure 1

- I really like analogy, but I just want to double-check with you that you are happy for the minor (presumably a relative), to be in the public domain. Have you check with their parents?

Thanks! The image show my daughter and myself about 20 years ago. She is no longer a minor and agrees to the use of the figure.

- In general, please alter the format of many in-text citations – e.g. "... through teleseismic waveform analysis by (Zhou, McNally and Lay, 1993)" should instead read "through teleseismic waveform analysis by Zhou, McNally and Lay (1993). Also, please make sure that citations and references are conform to Seismica's requirements (APA reference style).

We used this format temporarily during review due to a limitation of the Zotero reference system. With your help, we have found an adequate work-around for correcting this format in the revised manuscript. The reference type was APA v5, but somehow in Zotero the format differs from the APA example on the Seismic web site, and also Zotero does not use DOI's but URL's for APA. You have informed us that what is important is the author list is not truncated in the bibliography. We have settled on using Geophysical Journal International in Zotero as this correctly used DOI's and seems to truncate long author lists no more than APA in Zotero (e.g. both truncate at 7 authors).

Reviewer A:

Thanks very much for your thorough, positive and very helpful review.

In this manuscript, Lomax and Henry study the nature of fault geometry at depth using a new earthquake location technique called NLL-SST-coherence. The study area for this problem is in California, USA, with a focus on prominent earthquake sequences along major strike-slip systems in the state. Their relocated catalog outlines relatively smooth an narrow fault surfaces at depth, rather than geometrically complex and intricate fault zones. The authors posit that this property may be characteristic of mature strike-slip systems. These results suggest something of a disconnect between some of the surface complexity of mapped fault traces and the geometry within the seismogenic zone at depth. This would have important implications for earthquake hazard and rupture, as the primary slip surfaces on which these ruptures nucleate, propagate, and arrest may not be well-represented in their surface geometry.

Overall, this is an excellent and well-written manuscript that I would recommend for publication in Seismica after minor revisions by the authors. The differences between their location methodology and existing "high-precision" catalogs are thought-provoking and have relavance for our understanding of active fault systems. I have a few "big-picture" suggestions to explore, and then list some more minor line-by-line comments in the notes below.

One point that I think is missing from the paper but is worth discussing is the relation between the complexity of mapped surface traces and the complexity at depth. This paper demonstrates that the fault surfaces at depth are often simpler, which makes intuitive sense. But a question that is relevant to hazard and other problems is whether or not surface complexity is correlated with complexity in the seismic zone, or whether they are completely uncoupled.

We agree completely that this point and the general geometrical and physics relation between shallow and deeper faulting are fundamental issue to explore and re-asses, once it is accepted that main earthquake rupture surfaces may be smooth. It is certainly beyond the scope of our study and our current competences to explore these issues in detail. In the section Discussion → "Faulting at shallow versus seismogenic depth" we already state "These relations provide further evidence that surface traces and offsets of strike-slip fault zones reflect complex, shallow deformation, perhaps involving braided and upwards diverging fault structures". We have added further discussion of shallow structures followed by the sentence: "For basic understanding of large earthquake faulting and hazard it is important to better define the geometrical transitions and physical connections between complex shallow faulting and potentially simpler and smoother fault segments at seismogenic depth." We have also added a few additional points and references in this section and in discussion of the Southern Calaveras seismicity. In the final conclusions we have added: "This relation [shallow complexity versus smoothness at depth] has important implications for earthquake hazard assessment"

A related point that should be clarified is just a matter of presentation but is important caveat. Most of the systems studied here are in Northern California and on mature and fast-slipping faults. There are a number of other, notoriously complex fault systems throughout California that are not addressed (the San Jacinto comes to mind). I'm not suggesting that you repeat this analysis in a comprehensive way, but rather that the title, abstract, and conclusions reflect this focus.

Mt. Lewis, Monte Cristo, Ridgecrest and some of the Mendocino seismicity relate to less mature, immature and complex seismicity. In general, we have not studied much Southern California seismicity and large sequences primarily because waveform data are not available from SCEDC until after 2008 for 100Hz (preferred for NLL-coherence) and after 1999 for 20Hz (https://scedc.caltech.edu/data/cloud.html), and secondarily because our current codes use FDSN date services, whose connections and throughput seems to be limited at SCEDC, where priority is apparently given to AWS.

However, in part to address your, the editor's and the second reviewer's concerns about complex, multi-fault ruptures, we have added analysis of the complex, 2021 Calipatria sequence in the Brawley Seismic Zone. We use Calipatria because it is a clear but relatively simple example of multi-scale and near-orthogonal faulting for which we already had available NLL-SSST-coherence relocation. San Jacinto looks like a great case to apply NLL-SSST-coherence, but we have not done this yet, and the full San Jacinto area seems the have way too many events for our current codes and available computing hardware.

Finally, in the Discussion it would be good to briefly describe whether or not adding a differential time relative relocation method after the NLL-SST step would further enhance the results. This is the impression I get from Figure 1 (along with other readers I am sure), and we would be curious if there are any drawbacks to this beyond the obvious labor/computing costs.

I (AL) have wondered about this for some time, often suggested this to those who work with differential-time relative relocation methods. I suspect applying differential-time relative location after NLL-SSST would be a good procedure. And application after NLL-SSST-coherence may also have advantages, as NLL-coherence can collect noisy, outlier locations back into their correct clusters.

But I am not clear that there is always improvement on the finest scale using (HypoDD) differential-time relative relocation compared to NLL-SSST-coherence. We did not discuss in the initial submission, but the NLL-SSST-coherence relocations for Mount Lewis show apparent higher precision than HypoDD in both epicenter and depth, especially in the foreshock distribution around the M/S hypocenter, the early aftershocks, and for some of the orthogonal, damage-zone faulting structures. It is not clear why this should be, as the station distribution around the seismicity is very good. Perhaps related to the large depth range of the seismicity, or the quality of the very early (1986) digital waveform recordings? Perhaps, since it uses a grid-search and other differences, GrowClust would give different results than HypoDD.

In any case, we have added to the Discussion: "It is possible that applying cross-correlation based, differential-timing methods after NLL-SSST relocation would produce optimal multi- and finest-scale location precision. And applying these methods after NLL-SSST-coherence may also have advantages, as NLL-coherence can collect noisy, outlier locations back into their correct clusters."

Thanks for the interesting read!

Daniel Trugman, University of Nevada, Reno

Line-by-line comments and suggestions (bold/star comments need greatest attention):

Line 68: With the possible exception of the recent paper by Beeler, I think the literature consensus is that roughness is in fact scale-dependent (self-affine) and not self-similar. You mention this in a few sentences, so there potential for a bit of confusion when you first describe faults as self-similar.

Thanks. We have extended and attempted to clarifiy these sentences.

Line 80: Since this concept is critical to the paper, it may be worth defining what you mean by "seismogenic depth". Clearly there can be shallow earthquakes, or earthquakes that nucleate at greater depth and rupture the surface, but this is not likely what you mean. The reason this is important is that presumably there is some transition from the smooth, simple structures you describe at depth to some complex network at the surface. A greater discussion of this transition would be worthwhile (but not in the introduction).

We modified this sentence to: "The seismogenic depth, a brittle zone where most co-seismic slip and energy release occurs, is around 3-14 km (Marone & Scholz, 1988) for many large faults in California. At these depths"

Also see modifications in our reply to your comment above starting "One point that I think is missing from the paper..."

Line 87: Some of the more recent dynamic rupture models (e.g. Eric Dunham and Alice Gabriel's groups come to mind) include fault roughness (if band-limited) because it has important implications for high-frequency ground motions.

We agree that there is modeling with imposed fault properties and also geometrical complexity (e.g. splay faults) that, in particular, target observed, high-frequency ground motions. We have added mention that "Multi-scale smooth faults ... should radiate relatively less high-frequency energy than rough faults..." in reply to Reviewer A's comment on Line 550. Our focus, however, is observational constraint on the geometry of main earthquake rupture and consequences for earthquake rupture physics and hazard. One reason we introduce that "[faults] are usually modeled as ... smooth, near-planar surfaces" in the Introduction is that in the Discussion "Earthquake rupture modeling" section we conclude that for main rupture (not splay and shallow faulting and seismicity), it may be best to use planar or smoothly curved fault segments. We do, in the Discussion section, mention using "complicated and rough model fault geometries" and "continuum mechanics-based numerical modeling" – we could add more text and references here, but we consider that it steps outside the focus of the paper, and the bibliography is already quite long.

Line 143: GrowClust actually doesn't require gradient information because it is not a matrix-based approach, the relative locations are derived from a nested grid-search approach to minimize a robust travel time norm.

Yes, but, even grid-search methods such as GrowClust and NonLinLoc will be sensitive (in a numerical manner) to gradients in velocity, or, more precisely, gradients in travel-times, which control changes in travel-time and in ray directions.

Changed to: "However, these procedures depend on ***high-quality initial locations***, good station and ray coverage, ***and a good velocity model which produces accurate travel-times and gradients of travel-time***."

Line 149: Maybe worth mentioning that while these methods have recently been extended to 3D velocity models (Trugman et al., 2022) they still require reliable absolute locations to start with if the final locations are going to be accurate and not just precise.

See our reply to the preceding comment. And, in the following sentence it is noted that "[These procedure may fail to resolve meaningful, larger and multi-scale differences … because of low accuracy and precision in the underlying, arrival-time locations"

Line 170: Would be good to cite the original SSST work from the Shearer group

This is cited in the previous sentence, we have added another citation earlier in that sentence when "SSST" is first mentioned. There is previous work that is tightly related to SSST (we cite only Pavlis & Hokanson, 1985); Richards-Dinger & Shearer (2000) give more details.

and (briefly) mention any differences from NLL-SSST.

In different studies the Shearer group SSST work seems to use at least 3 different procedures for spatial smoothing to develop SSST time corrections: N neighbors, fixed, spherical distance, and shrinking box! Additionally, there are other key aspects of NLL-SSST, especially the use of the EDT misfit function, which very efficiently removes outlier data, thus reducing bias and noise in the accumulated corrections. All this seems too much to discuss and a distraction in the context of the current paper. However, we have added the sentence: "NLL-SSST uses smooth, Gaussian distance kernels for accumulating SSST corrections, while Richards-Dinger & Shearer (2000) use a fixed number of neighboring events, and Lin & Shearer (2005) use fixed distance and shrinking-box approaches."

Line 218: This section seems to be missing a few key details. Can you describe how coherency is defined and measured? Is it aggregated across stations? Out to what distance?

We modified and added the text, giving: "We measure coherency as the maximum, normalized cross-correlation between waveforms from one or more stations for pairs of events within a specified distance after NLL-SSST relocation (5-10 km in this study). We take the maximum station coherence between the target event and each other event as a proxy for true inter-event distances and thus as stacking weights to combine NLL-SSST location probability density functions (PDF's) over the events."
And we added at the end of this paragraph: "See ... Supplementary File S1 for NLL-SSST-coherence processing parameters used in this study"

Line 236 / Figure 1: [First, I like the figure!]

Thanks!

Perhaps best to address in the discussion, but this section and Figure motivate the question of whether NLL-SSST could be further refined with a differential time relocation at the end? This would allow the multiscale refinement to give you the bottom right of the figure. With this as the input catalog, you may imagine that the finest scales could then be obtained through precise correlation.

See our comments above on this issue. Yes, we prefer to address this in the discussion, because of lack of tests of this hybrid procedure and evidence (e.g. Mount Lewis) that differential time relocation may not always give better finest scale results.

Line 245: Based on Figure 2, these are all in "Northern California" so the title and abstract may need updated to reflect this.

Parkfield is geographically in Central California, and Ridgecrest (discussed, though not analyzed in this study) is in Southern California. In any case, we have added 2021 Calipatria sequence in Southern California to this study. We also changed the beginning of this sentence to "We apply or discuss NLL-SSST-coherence relocations for recent large-earthquake sequences..."

The waveform-relocated catalogs for Southern California do not use HypoDD.

We did not use waveform-relocated catalogs for Southern California in this section which these sentences introduce. We do now discus HYS seismicity for Calipatria in the Discussion; we qualify this procedure as "GrowClust type", since HYS seems to be based on a precursor to GrowClust.

Figure 2: I would suggest removing the white boxes in this figure; it is not clear what "referenced" in this work means (there are dozens of references and many more study areas than listed here) and even if the meaning was clear if these areas are not covered in detail than it is best to leave them out of the figure.

We have corrected the phrase to "NLL-SSST-coherence relocations presented (magenta) and ***discussed*** (white) in this work".

Line 276: When fitting the plane, is this a single surface for the study region, or something with along strike variation?

Clarified to: "a singular value decomposition (SVD) fit of a single plane to the each of the respective hypocenter sets"

Line 309: This is an interesting finding – what is the physical interpretation of this arc?

This is discussed in the section Discussion → "Rupture physics". We have modified the line to read: "a circle of radius 428 km centered to the south-southwest (Supplementary Figure S2), ***the significance of which we discuss later***."

Line 463: This might be a good point to describe how NLL-SSST-coherence could be used in tandem with differential time methods (or if there is a problem with this approach, describe that)?

Yes, this is where we added discussion on this issue.

Line 499: I imagine the NLL-SSST-coherence approach would still yield a complex fault network for Ridgecrest? If not, that would be quite surprising!

Yes, for Ridgecrest NLL-SSST-coherence epicenters for a sub-set of events are very similar to the various, published high-res catalogs, including complex and orthogonal faulting over multiple scales (see Figure X below in a reply to Reviewer B). But we cannot currently run enough events with our current codes and computing hardware to fully test NLL-SSST-coherence for this vast and highly productive aftershock sequence..

Instead, we add discussion on the Mount Lewis sequence, which shows complex, orthogonal damage zone faulting, and have added analysis and comparison with Hauksson et al. 2022 of the complex, 2021 Calipatria sequence in the Brawley Seismic Zone.

Line 516: Where is this analysis described? I was curious why this sequence didn't show up in the manuscript…

Our analysis of and familiarity with the South Napa sequence are preliminary and we did not intend to fully present or interpret this sequence here. We though this single sentence added useful information and evidence to the current discussion. But if the **EDITOR** also thinks more material is required here, we propose to simply remove this sentence.

Line 550: Note also that the presence or absence of fault roughness has important implications for radiated energy at high frequencies (Trugman and Dunham, 2014; Graves and Pitarka, 2016).

Good point! We have extended the preceding sentence with: "Multi-scale smooth faults ... should radiate relatively less high-frequency energy than rough faults (Madariaga, 1977; Shi & Day, 2013; Trugman & Dunham, 2014)."

Line 608: As mentioned above, it is important to understand if complexity at the surface is correlated with complexity at depth.

Yes! We have modified this sub-section, see our comment above on shallow complexity vs smoothness at depth.

Zenodo links in the SI need to be updated once finalized.

Done. https://doi.org/10.5281/zenodo.7802678

Video S1 and S2 are neat! Would it be a lot of work to compile similar ones for the other datasets?

Thanks! We find in general that seismicity, which is at least 5-dimensional (x, y, z, t, M) and can be highly complex, is best understood with 3D, interactive or animated visualization. Often, apparent relations and patterns that appear in static, 2D sections disappear or are shown with 3D viewing to be elements of more complex features, while many oblique or more complex patterns are only discernible with such 3D viewing.
For all included seismicity studies besides Parkfield we have added animations, as these help understand geometrical relations we raise for these studies.

Reviewer B:

Thanks very much for your extensive, thorough and very helpful review. It is particularly useful and thought-provoking to have your comments as a non observational seismologist.

The issue of what faults look like at depth, and how their surface trace geometry relates to their functional slip surfaces, is a long-term question that becomes particularly relevant when evaluating how fault geometry can control seismic hazard. This study uses a new seismicity relocation method to address this question for moderately large strike-slip ruptures in California. The authors find that the source faults for all of their target earthquakes are much more smooth/planar than previously thought, and that it is therefore valid to consider most major faults as planar for seismic hazard assessment.

I am writing this review from the perspective of someone who works on fault geometry and its controls on ruptures, but not as an observational seismologist. That said, I'm afraid that I am not currently convinced that the new method isn't overly simplifying seismogenic faults, and that there isn't a built-in assumption that more precise must mean smoother and vice versa. I have two main reasons for this uncertainty. One is that some of the example earthquakes (Parkfield and South Napa) have multiple documented strands of rupture/afterslip that do appear to be separate for at least part of the seismogenic depth in geodetic or other seismic models. If the fault is actually very smooth, where does this come from?

We touched on this issue in the Discussion → "Faulting at shallow vs seismogenic depth" sub-section: "These relations provide further evidence that surface traces and offsets of strike-slip fault zones reflect complex, shallow deformation, perhaps involving braided and upwards diverging fault structures ... and not directly simpler or hidden slip surfaces at seismogenic depth ... where most co-seismic slip and energy release occurs." And we added a paragraph at the end of this sub-section further discussing the transition from complex shallow faulting to smooth faulting at depth.

For Parkfield, it is generally accepted that the background seismicity and 2004 rupture define a single main fault surface at seismogenic depth, and that this surface falls not along the curved, main San Andreas fault, but instead along and under the straighter Southwest Fracture Zone (e.g., Simpson *et al.* 2006; Thurber *et al.* 2006). For example from (Thurber *et al.* 2006):

> "The seismicity is concentrated on a single planar fault surface at depth through Cholame Valley, where two principal fault traces are observed at the Earth's surface (Figs. 5 and 6) (Rymer et al., 2006). The absolute locations place the earthquakes below the surface trace of the Southwest Fracture Zone (SWFZ) rather than the main surface trace of the SAF where surface creep is observed. This is consistent with the observation of coseismic slip in the 2004 mainshock on the SWFZ and only postseismic slip on the main trace (Rymer et al., 2006). Unfortunately, the lack of shallow seismicity makes it unclear how the seismogenic fault at depth relates to the surficial fault traces. There is, however, a suggestion of a flower structure ("a series of vertical faults of compressional duplexes that are subparallel to the parent fault; the vertical faults divide upthrown blocks that spread near the surface in a manner resembling a flower" [Engelder, 1998]) in the vicinity of the 2004 mainshock (Fig. 6b, Y # 20 km). This hypothetical flower structure is a possible, and perhaps likely,

> connection from the single fault surface at depth to the Earth's surface, but this is not constrained by our results."

The originality of our work is not rediscovering that there is a single main seismogenic fault surface at Parkfield, but instead showing that this surface is smooth and highly planar. We have added in the introduction to the Parkfield study mention of the generally accepted, single main fault surface at seismogenic depth.

But the other, bigger reason is that the authors don't test their method on any events that are genuine multi-fault ruptures on distinct structures. The San Andreas and West Napa faults are still considered individual structures despite their possible multiple strands, so interpreting them as largely planar is within reason. But without a test on a dataset that shouldn't produce a single plane, it's hard to rule out oversimplification and demonstrate that the method isn't intrinsically equating "more precise" with "more planar."

I think the revised version of this study needs to include a test of this method on a confirmed complex, distributed multi-fault rupture. The 2019 Ridgecrest sequence would be a good test case, since there is a lot of data, and at least some of the faults involved were further apart than in the Parkfield or Napa cases. If that test case shows that NLL-SSST does capture the complexity of the fault network involved, that demonstrates that it isn't working in favor of planarity, which makes the other results more credible in comparison.

These are very good points. We did already note in sub-section Discussion → "NLL-SSST-coherence methodology" that:

> "For Mount Lewis, the NLL-SSST-coherence relocations match well detailed features of the high-precision NCSS-DD (Figure 6) and (Kilb & Rubin 2002) relocations, including definition of the main rupture surface, complex secondary structures and fault sets perpendicular to the main rupture, without exhibiting additional smooth or linear features that might be artifacts of the NLL-SSST-coherence methodology."

But perhaps we did not illustrate convincingly that NLL-SSS-coherence does not oversimplify distributed multi-scale, multi-fault rupture. For this purpose, we have added in this section comparison of NLL-SSS-coherence with the high-precision relocations of Hauksson et al. (2012, 2022) for the complex, 2021 Calipatria sequence in the Brawley Seismic Zone.

For Ridgecrest, NLL-SSST-coherence epicenters for a sub-set of events are very similar to the various, published high-resolution catalogs, including complex and orthogonal faulting over multiple scales (see Figure X below). But we cannot currently run enough events with our current codes and computing hardware to fully test NLL-SSST-coherence for this vast and highly productive aftershock sequence.

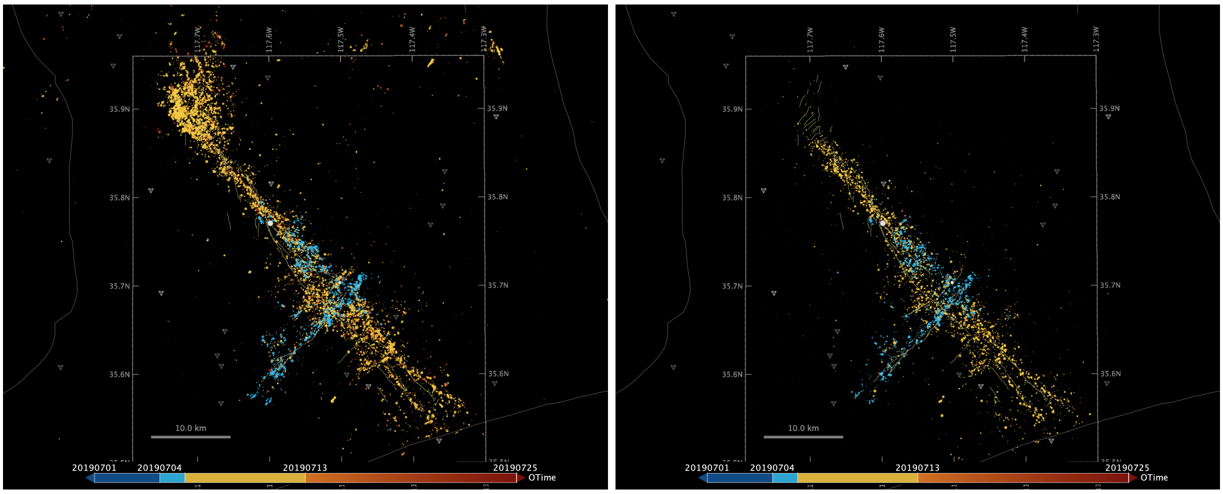

Figure X. High-precision 2019 Ridgecrest sequence M≤1.0 relocations within the same time period for (left) catalog and template-matching events from Ross et al. (2019) and (right) NLL-SSST-coherence catalog event relocations for a sub-area (due to computational limitations).

I also think the discussion section would benefit from some discussion of how to reconcile planarity and smoothness with empirical (e.g., Wesnousky, 2008; Biasi and Wesnousky, 2017) and modeling (e.g., Harris and Day, 1993; Oglesby, 2005; Lozos, 2021) studies that show how many ruptures end at geometrical complexities;

We do not contest this as we are discussing smoothness on fault segments, which can end at geometrical complexities. We did mention and reference in the southern Calaveras relocations sub-section that the 1984 Mw 6.2 rupture terminated in an area of complexity in the seismicity at depth, and that the 1979 M 5.8 main rupture likely terminated at a right step in fault segments. And in the Discussion → "Rupture physics" subsection we stated "The arrest of earthquake rupture is likely favored at barriers such as kinks, steps or other fault complexities at the limits of smooth segments…"

Perhaps we were not clear that we find and interpret multi-scale smooth faulting to occur along *fault segments* (at seismogenic depth) and not, in general, along the major part of large earthquake ruptures or large fault systems (one exception being the 100's km long straight section of the central San Andreas fault zone). So throughout the manuscript we have added or clarified text to more clearly emphasize this point, such as using terms like "multi-scale smooth fault segments" instead of "multi-scale smooth faulting", and further specifying "at seismogenic depth".

and with the concern that rupture simulators and hazard models overestimate the number of very large earthquakes compared to what the paleoseismic record suggests (Biasi and Scharer, 2019; Hough et al., 2020). If the faults are actually smooth and planar, why do ruptures stop where they do, and why are the hazard models producing more large earthquakes than we actually see?

On the latter point, we emphasize that we are considering fault segments, not the full rupture length of very large earthquakes, which may involve multiple segments and multiple faults. However, this is an interesting point with regards to very large San Andreas fault earthquakes, since the San Andreas fault clearly has long, smooth, nearly straight stretches that may consist of one or a few, smooth segments at seismogenic depth. We thus added to the section "Discussion → Rupture physics":

> "It is possible that on the largest scales, e.g. for the 1857 M 7.9 and 1906 M 7.9 rupture zones along the San Andreas fault, main rupture may occur on one or few long, smooth segments. In this case features of rupture physics of smooth faults discussed above combined with a possible strong locking of smooth faults due, for example, to efficient healing by cementation or other chemical processes on thin, smooth fault surface (Muhuri et al. 2003; Williams & Fagereng 2022), may be explanations for potentially long recurrence intervals and resulting large size of these events. Additionally, the seismicity patterns southwest of San Francisco, where upper crustal seismicity occurs mostly off the SAF with dominantly extensional focal mechanisms, is consistent with near complete release of shear stress after the 1906 earthquake. Thus, for a given length scale, a smooth segment may be expected to take more time than a rough segment to reload back to a critical state after rupture."

We also expand on our statement in the first paragraph of the "Rupture physics" sub-section: "and perhaps enables the occurrence of larger earthquakes (Goebel et al. 2017, 2023)", by adding at the end of this paragraph:

> " Laboratory experiments indicate that smooth faults have larger co-seismic slip, lower residual stress and fewer aftershocks compared to rough faults (Goebel et al., 2023)."

Beyond these larger concerns – which I hope will not be too difficult to implement – I have some more specific comments on individual lines and figures within the manuscript. I go through those below.

Line 27: If you're going to use the abbreviation for the method in the abstract, please also spell out what it stands for/means.

"NLL-SSST-coherence" is the *name* of the method, not an abbreviation. We do define the component abbreviations within this name when appropriate in the main text. If the **EDITOR** requires explaining the abbreviations in this name, we will have to replace it with something vague like "a new method", as there is not enough space in the 200 word Seismic abstract limit to define abbreviations. We note that similar method names containing abbreviations are often present without explanation in abstracts, e.g. "Hypoinverse, Velest, and hypoDD" in (Kroll *et al.* 2013), "hypoDD" in (Holland 2013), "SIMULPS" in (Haslinger & Kissling 2001).

Lines 33-36: This sentence is awkward. Please consider splitting it into two sentences, perhaps like this: "These relocations suggest that multi-scale smooth faulting is characteristic of mature strike-slip fault zones, and is necessary for large ruptures. Fault smoothness and curvature influence rupture initiation, behavior, and arrest, and can help define rupture hazard zones."

Thanks, done.

Lines 68-91: A lot of the sentences in this paragraph are very long. Please consider splitting some of them up.

Thanks again, done.

Lines 70-71: Since this paper hinges on a specific definition of smoothness vs. roughness, please elaborate on what some other definitions are, and please emphasize why you chose the specific definition that you did here.

We do this in the last paragraph of the Introduction: "… multi-scale smooth (i.e., have fractal or Hausdorff dimension ~2.0 over a specified range of length scales)"

Lines 82-87: This sentence has a lot of unnecessary commas, and a few typos.

Thanks, modified.

Line 84: How can the San Andreas in Parkfield be both twisting and planar? This seems contradictory.

We agree. These are conflicting results from different studies that make different conclusions. Pointing out conflicting interpretations is the purpose if this paragraph. We modified to "***various*** high-precision, differential-timing relocations..."

And why is "predominantly" in quotation marks? This also seems to imply some nonplanarity in this context.

That is the exact term used in (Thurber *et al.* 2006). We have removed the quotation marks.

Lines 90-91: What are the size limits for these segments? Are the examples in the rest of your paper within these size limits?

The point here is that fault segments (as opposed to longer fault zones) are smooth and near-planar; segment size is not relevant here (and may vary over orders of magnitude, as developed later in the paper). To clarify, we have changed the sentence from "However, the geometry of mature faults ... smooth, near-planar surfaces, at least on segments of limited size." to "However, the geometry of faults segments ... smooth, near-planar surfaces."

Lines 92-94: This sentence is also pretty awkward. Please consider rephrasing a little, perhaps like this. "Thus, current understanding of multi-scale geometrical complexity and smoothness of large seismogenic faults is based on conflicting results. This makes it particularly complicated to relate faults' surface traces with their geometry at seismogenic depths, and to interpret how this geometry may affect rupture physics."

Thanks. We modified this paragraph along the lines of your suggestion.

Line 97: Please cite some sources for seismicity highlighting the earthquake initiation process.

This is a huge literature, and also involves earthquake physics, lab experiments, controversies such as active, "cascading" foreshocks versus "passive" foreshock activity due to a larger "aseismic" process. For this general sentence in the introduction, which includes two other basic but broad concepts, we would need to add many references. But this seems unnecessary here, and also excessive given the already very long bibliography of our paper. We have added (Scholz 2019) as a general reference for this paragraph. We do cite sources specific to earthquake initiation as relevant in the analysis of specific earthquake sequences and in the sub-section "Discussion→ Rupture physics".

If this were a given, all earthquakes would have foreshocks!

More and more foreshocks are indeed being found with better and closer instruments and with new, waveform and machine-learning based event detection techniques. And “seismicity” includes definition of the hypocenter, so only the initiation of (very slow?) rupture processes without a resolvable hypocenter would not be highlighted by information from seismicity.

There are also a lot of sources out there (e.g., Dieterich, 1994) that describe nucleation as a largely aseismic process.

We agree and did not intend to exclude these processes. We changed the phrase from “Seismicity shows...” to “Seismicity can show...”

Lines 104-105: Please define which scales of smoothness you're going to use.

This sentence is for a general case. The scales are defined for our case and methodology in the next paragraph.

Line 106: This is the first time you use the abbreviation for your method's name. Please spell out what it stands for when you introduce it.

As noted above, this is the method name, not an abbreviation. The various components of the method and their abbreviations are defined later, where more fluid and relevant in the manuscript.

Line 106 (and throughout): When you're referring to a source as part of the phrasing of a sentence, you only need to put the parentheses around the year. For example: “Extending the work of Lomax and Henry (2022), we apply…”
When the citation is a reference but isn't part of the sentence structure, then you put the whole thing in parentheses.

Yes, we are fully aware of this. This formatting is due to a limitation of the Zotero citation software we use. We had informed the editor that we would correct manually before manuscript acceptance. Instead, in discussion with the editor we have now found a work-around for this problem.

Lines 121-132: This whole paragraph is one long sentence. Please break it up.

The bulk of the sentence is a list with extensive citation. We have broken it up into 2 sentences and made the list grammatically parallel.

Paragraph starting line 163: A figure demonstrating these steps with waveforms, not just photographs, would be helpful here.

Illustration of these steps would require numerous figures showing seismic rays and their traveltime fields, and improvements of seismicity maps, but does not involve waveforms. This would be much material and description to add, while it seems outside the focus of this paper and is fully described and illustrated in the first references in this paragraph.

Lines 188-190: The comma between “multiplet or family” and “these events” should be a semicolon instead.

Thanks, corrected.

Line 220: “Test” doesn't need to be capitalized in the middle of a sentence.

Fixed.

Lines 220-222: Why did you use an explosion, which is inherently non-double-couple and not a result of frictional sliding on a plane, to test a method for relocating earthquakes to planar surfaces? Please elaborate.

All earthquake location methods described in this paper use ray theory and absolute or differential *time* measurements of energy onsets on seismogram waveforms. The main effect of the source mechanism on these onsets is to change their waveform polarity and amplitude, not their onset timing. So these methods are applicable to any seismic source that produces impulsive energy onsets and are practically *insensitive to the source mechanism*, be it double-couple, extensional (crack opening), or explosion. Explosion observations and experiments are widely used in development, testing and calibration of earthquake location algorithms because they are "*ground-truth*" sources for which the absolute spatial location and origin time are precisely known. The location and origin time of natural seismic events at depth can only be estimated (through earthquake location!) - depth and origin time are often particularly poorly constrained since seismic stations are usually only distributed on the Earth's surface and not below the sources.

As the above seems too much to add to one sentence/point in the manuscript, we have simply changed "using explosion data" to "using controlled-source, explosion data" to allude to the above issues.

Line 232: "Finest-scale" should be hyphenated.

Corrected.

Line 253: Why are you only applying NLL-SSST down to 4 km and not deeper? Most faults have a deeper seismogenic depth than that, so if the goal is to refine our understanding of fault geometry at depth, why not go to full depth?

These values do not refer to *depth* of seismicity used but to the scale of spatial smoothing over hypocenters used to accumulate SSST travel-time corrections. To avoid the ambiguity, we changed "down to 4 or 2 km" to "with a smallest smoothing length of 4 or 2 km".

Section starting line 267: Please describe some basic rupture characteristics of this earthquake, for readers who may not be familiar with it.

We have added some basic description, especially as relevant to later discussion.

The San Andreas Fault in Parkfield has several well-described strands that each exhibit some aseismic creep (e.g., Rymer et al., 2006; Simpson et al., 2006). In particular, the Southwest Fracture Zone shows distinct seismicity and creep from the main fault (e.g., Murray and Langbein, 2006), to the point where it accommodated both rupture and afterslip from the 2004 earthquake (e.g. Langbein et al., 2005; Rymer et al., 2006; Murray and Langbein, 2006). Even if you can relocate the seismicity to a single plane,

Indeed, we and many others determine the main fault at seismogenic depth to be a single, planar (straight) fault along and under the (straight) Southwest Fracture Zone. We have added statement of this in the first paragraph of this sub-section, and labeled the Southwest Fracture Zone on the map figures.

the aseismic slip suggests partitioning and complexity at least in the top several km of the fault. How do you reconcile these observations with the idea of a single planar fault?

We discuss this later and follow previous work e.g. (Graymer *et al.* 2007). For example in the discussion: "These relations provide further evidence that surface traces and offsets of strike-slip fault zones reflect complex, shallow deformation, perhaps involving braided and upwards diverging splay faults and flower structures (e.g. Christie-Blick & Biddle, 1985; Graymer et al., 2007; Richard, Naylor, & Koopman, 1995), and not directly simpler slip surfaces at seismogenic depth (e.g. Schaff et al., 2002) where most co-seismic slip and energy release occurs."

Line 284: The 1966 Parkfield earthquake also had a moment magnitude of 6.0.

Reference? We cannot find clear statement of this. We have changed to "M ~6"

Lines 286-287: Most of the discussion of why the 2004 Parkfield earthquake was "late" and why it propagated in the opposite direction from previous Parkfield events centers around the effects of the 1983 Coalinga earthquake (Toda and Stein, 2002) and (to a lesser degree) the 2003 San Simeon earthquake (Johanson and Bürgmann, 2010). How does this fit into the context of your geometrical argument?

Our geometrical argument is that a straight fault may host rupture in opposite directions on the same patch – there is no preferred direction due to fault geometry (e.g. curvature). So a planar Parkfield fault gives no particular constraint on rupture direction or timing, thus allowing and being compatible with other explanations such as modification of the stress field by nearby earthquakes.

Discussion of the effects of stress changes due to previous earthquakes in the context of the effect of curved, smooth faults can be more clearly done with large earthquakes along the North Anatolian fault. In the section "Discussion → Rupture physics" we have extended our discussion of preferred rupture direction along this fault noting that this effect may explain why the epicenter of the 1943 Ms 7.3 Tosya event was not in an area of increased stress, as well as adding the example of the preceding, 1939 Ms 7.9 Erzincan event.

You may also want to discuss whether even the most complex interpretation of the San Andreas Fault geometry at Parkfield fits within the range of fault geometries that stop ruptures in empirical and/or modeling studies.

Probably not northwest of Cholame, and maybe not even at the surface stepover features at the north end of the 1857 rupture zone – this may imply that fault surface properties and stress history are mainly controlling rupture location and limits for M6 Parkfield events. But as our emphasis in this paper is that faults are *smoother* than previously interpreted, it does not seem relevant to speculate on physical consequences of opposite end-member results.

Section starting line 302: Please describe some basic rupture characteristics of the 1979 and 1984 earthquakes, for readers who may not be familiar with them. Motivate why you chose to look at these events in particular!

We have added some description and a reference.

It may help to move the content that's currently in lines 316-320 to earlier in this section.

This seems appropriate if this sub-section was the focus of our whole study. We have not done this here as it would require additional text and complexity at the beginning of the sub-section, and introduces a possibly confusing, back reference to these previous results on the events that are relevant to our results that have only just been described.

Also: the Calaveras fault is a partially-creeping fault, and the interface between creeping and locked sections is where most seismicity occurs (Schaff et al., 2002). If there's much less seismicity on the creeping parts of the fault, how might that affect your ability to resolve the geometry there?

If, at each horizontal position along the fault zone, there is no seismicity within at least two depth ranges, then the 3D geometry cannot be directly determined from hypocenter patterns. But along the stretch of the Calaveras fault that we examine (of which the stretch studied by (Schaff *et al.* 2002) is a subset), there is almost always some seismicity within two or more different depth ranges. Strictly, we cannot exclude that the fault diverges from a smooth surface in patches of no seismicity, but this explanation seems highly unlikely since almost the availability seismicity from different depths concentrates strongly along an arc in map view—this explanation would not be the simplest solution given the available information.

Line 334: Please introduce some characteristics of this earthquake sequence, for readers who may not be familiar with it.

Done.

Section starting like 353: Please give some more background information for the Mount Lewis earthquake as well.

Done.

Line 370: Did you mean "respectively" instead of "receptively"?

Yes, thanks.

Lines 371-374: Earlier in the paper, you describe smoothness and planarity as hallmarks of a more mature fault system, but here, you attribute the Mount Lewis earthquake to an immature fault system that still has a smooth, simple, planar main fault. This seems contradictory. Please explain how both of these things can be true.

In reply to one of your main concerns, we have throughout changed terms like "geometry of mature faults" to "geometry of faults segments" and "mature" to "major", and we also specify that we find "rupture zones" of large and moderate earthquakes that are smooth. For example, in the Discussion→ "Relocation results" section we summarize: "These NLL-SSST-coherence relocations define multi-scale smooth faulting over at least 10's of km for segments of mature, strike-slip fault zones, and over the likely rupture zones of large and moderate earthquakes along these faults and on less mature faults."

Line 394: Do you mean the San Gregorio Fault? That's the one that's west of the San Andreas.

Golden Gate fault is correct, but "northwest" may be confusing – the stepover is to the northwest but it puts the Golden Gate fault to the northeast of the extension of the San Andreas fault. We removed "northwest" as "offshore" and details on the map make this relation clear.

Lines 400-403: Please cite some sources for why/how these rock property contrasts control seismicity and faulting.

There does not seem to be much study of off fault seismicity with regards to geologic structure. (Liu *et al.* 2003) state:

> "One question that has seldom been asked is, What do aftershocks tell us about the geologic structure of fault zones that produce large earthquakes? Their almost universal association with large crustal earthquakes suggests that they must be caused by the stress perturbations induced by the mainshock rupture. But are they reruptures of the mainshock plane, or are they the result of subsidiary faulting in the fault blocks that bound the primary fault?".

We have changed the sentence to be more speculative and general "***may*** indicate a strong contrast in ***geologic structure***" and cited (Liu *et al.* 2003).

Lines 406-411: Can you rule out nonvertical dip on the San Andreas? The Santa Cruz Mountains section of the fault isn't quite vertical (e.g., Schwartz et al., 1990), and this area isn't far from there. The cloud of seismicity you have in Figure 7 has room for dipping structures. If you've ruled out dip as an explanation, please explain why.

The cloud almost certainly contains dipping faults and fractures, this is compatible with the focal mechanisms of the background seismicity. However, it seems to be generally argued and supported by some observations that the San Andreas proper is vertical under the northern San Fransisco peninsula. We have added "These relations **and evidence from seismicity (Zoback et al., 1999) and reflection seismics (Hole, Thybo, & Klemperer, 1996)** suggest that the active fault surface that hosted 1906 rupture at seismogenic depth may be represented by a vertical plane".

Lines 446-450: This seems like something that should go in the methods section, or maybe even the introduction, when you're motivating the need for a more precise relocation method.

Again, it seems best to us to provide this information where it is needed, instead of some kind of explicit or implicit back-reference to earlier material. Moreover, this is detail about other methods, not the method that we are using. We did already include the base underlying this detail in the Introduction "Relative location methods use … differential timing between events at individual stations to determine fine-scale, inter-event spatial relations"

Lines 464-465: Can you please elaborate on/demonstrate this? As I mentioned earlier, one of my biggest questions/concerns is that this method might overly smooth things, and that it would be very helpful to test it on something we know is rough/immature to show that it does capture those details where/when they do exist.

Done, see reply to your earlier concern.

Line 497-499: Many Ridgecrest aftershock relocations outline several planes, not just the two that hosted the largest coseismic slip (e.g. Shelly, 2020; Jin and Fialko, 2020). Please cite/discuss these interpretations here, and not just your own previous work.

We are not referring to the full Ridgecrest sequence with its multitude of parallel, orthogonal and oblique planes. The key points here are that relocated seismicity shows that the two main *M6.4 foreshock* ruptures involved two *planer* surfaces and that they are *non-intersecting at different depths*. The other high-precision relocation studies do not show these latter relations. We have included reference to two of these other studies and separately stated that the NLL relocations additionally show non-intersecting planes at different depths.

Lines 516-519: Surface rupture and afterslip for the South Napa earthquake also shows several active fault strands (e.g., DeLong et al., 2016; Floyd et al., 2016). How do your results relate to this?

As noted above, our analysis of and familiarity with the South Napa sequence are preliminary and we did not intend to fully present or interpret this sequence here. We though this single sentence added useful information and evidence to the current discussion. But if the **EDITOR** also thinks more material is required here, we propose to simply remove this sentence.

Line 526: Ending a whole section with a colon is awkward.

Fixed.

Lines 535-538: There are a lot of newer modeling and observational papers addressing rupture arrest at geometrical complexities. Please cite some of these as well.

As we are introducing basic concepts and not criticizing or directly working on them, it seems most appropriate to reference the early, pioneering work and also include a modern review (Scholz 2019), as we have done. If the **EDITOR** considers that we should add more recent focussed studies, we can try and find some—neither of us is that familiar with the recent literature, it may be vast.

Lines 539-540: What is the significance of whether a fault follows a circular curve? Please explain this. You described it for the Calaveras earlier, but didn't go into why this is significant from a rupture physics standpoint.

This is explained in the rest of the same sentence and in the following sentences: "there may be negligible geometrical interactions and resulting backstresses (Dieterich and Smith, 2010) impeding strike-slip rupture displacement. In this case…". For a perfectly smooth fault that follows exactly a circular curve there would be ***absolutely no*** (as opposed to ***negligible***) geometrical barrier interaction, but as this is not a realistic case we do not see need to explicitly state this.

Lines 544-546: Other studies suggest the same thing. Please cite some, to show your results are consistent with past work.

We have added some references. We have not found clear reference in other studies to rupture physics with regards to a circular (or event smoothly curved) fault. Most modeling studies use a kink or set of very small kinks, some of which we already cite.

Line 567: Typo on "North Anatolian Fault."

Thanks, fixed.

Lines 581-585: How are you defining smooth in this case? Just on the slip surface, or effectively planar?

The same as in the rest of our manuscript, main rupture patches and fault segments are multi-scale smooth curved or planar surfaces. We have changed "smooth" to "multi-scale smooth".

For example, many of the faults involved in the Kaikōura rupture were highly curved, and the whole rupture trace zigzags. How can this be considered smooth? Please explain and cite things!

This is the geometry of shallow and surface traces, we are always referring to individual fault segments at seismogenic depth (which can be curved), and not a full, multi-segment rupture. However, there is a serious problem in that, for large earthquakes, particularly multi-segment, the aftershock seismicity is mainly or almost entirely off of the main rupture surfaces (almost by definition since main rupture releases stress across the main fault). We have added note and references for this, and have explicitly stated earlier that the fault segments "***may*** be multi-scale smooth".

We have added the missing reference for Kaikōura.

Lines 605-607: This is another thing that various seismic, geodetic, laboratory, and modeling studies support. Please cite some of them here, both to back yourself up and to show that your results are consistent with previous interpretations.

Done.

Lines 619-622: I think this is worth mentioning earlier, like when you are first introducing these earthquakes and why you chose to apply your method to them.

Done for Mount Lewis, referenced for Monte Cristo.

Lines 631-633: Ending the paragraph here almost seems like an incomplete thought. With this in mind, do you have thoughts on how rupture modelers should approach defining fault geometries?

We added "available information from aftershock or background seismicity, and geophysical and geologic studies should always be considered for constructing fault segments at seismogenic depth."

Figures, in general: Please make all of your axis/lat-long labels and text annotations larger, and make lines (including faults) thicker/heavier. These are currently thin/small enough that they're difficult read even at full page scale, and when these are formatted in the journal, they may be even smaller.

We have increased the size of text within the limits of the plotting software and figure formats/layout/content for all examples: Parkfield, Mount Lewis, Mendocino, "southwest of San Francisco". We have changed the layout of the Parkfield figure to allow a larger presentation of each panel.

Figure 1: This example is very cute, and a nice illustration! But I think it would be helpful to show a waveform example next to the photograph example.

Thanks! Please see our reply to your comment starting "Paragraph starting line 163".

Figure 2 (and elsewhere): Please don't use red and green annotations/markings on top of each other. These colors together are very bad for colorblind people.

Thanks for noting this. We have modified the colors and color scales for this figure. We have also modified all the relocation figures to better support various types of color blindness. It is not possible to do this perfectly when there are numerous categories of color (e.g. background seismicity, foreshocks, mainshock, aftershocks, plus faults and other annotations), but we think these changes have simplified and improved the figures for all readers.

Figure 7: Please indicate where these cross sections (panels b and c) are on the bigger map (panel a).

We better indicated the view of panel b on the map and have replaced the panel c) cross section with a map view of the deep seismicity.

REFERENCES

Biasi, G. P., & Scharer, K. M. (2019). The current unlikely earthquake hiatus at California's transform boundary paleoseismic sites. Seismological Research Letters, 90(3), 1168-1176.

Biasi, G. P., & Wesnousky, S. G. (2017). Bends and ends of surface RupturesBends and ends of surface ruptures. Bulletin of the Seismological Society of America, 107(6), 2543-2560.

DeLong, S. B., Donnellan, A., Ponti, D. J., Rubin, R. S., Lienkaemper, J. J., Prentice, C. S., ... & Parker, J. W. (2016). Tearing the terroir: Details and implications of surface rupture and deformation from the 24 August 2014 M6. 0 South Napa earthquake, California. Earth and Space Science, 3(10), 416-430.

Dieterich, J. (1994). A constitutive law for rate of earthquake production and its application to earthquake clustering. Journal of Geophysical Research: Solid Earth, 99(B2), 2601-2618.

Floyd, M. A., Walters, R. J., Elliott, J. R., Funning, G. J., Svarc, J. L., Murray, J. R., ... & Wright, T. J. (2016). Spatial variations in fault friction related to lithology from rupture and afterslip of the 2014 South Napa, California, earthquake. Geophysical Research Letters, 43(13), 6808-6816.

Harris, R. A., & Day, S. M. (1993). Dynamics of fault interaction: Parallel strike−slip faults. Journal of Geophysical Research: Solid Earth, 98(B3), 4461-4472.

Hough, S. E., Page, M., Salditch, L., Gallahue, M. M., Lucas, M. C., Neely, J. S., & Stein, S. (2021). Revisiting California's past great earthquakes and long−term earthquake rate. Bulletin of the Seismological Society of America, 111(1), 356-370.

Jin, Z., & Fialko, Y. (2020). Finite slip models of the 2019 Ridgecrest earthquake sequence constrained by space geodetic data and aftershock locations. Bulletin of the Seismological Society of America, 110(4), 1660-1679.

Johanson, I. A., & Bürgmann, R. (2010). Coseismic and postseismic slip from the 2003 San Simeon earthquake and their effects on backthrust slip and the 2004 Parkfield earthquake. Journal of Geophysical Research: Solid Earth, 115(B7).

Langbein, J., Borcherdt, R., Dreger, D., Fletcher, J., Hardebeck, J. L., Hellweg, M., ... & Treiman, J. A. (2005). Preliminary report on the 28 September 2004, M 6.0 Parkfield, California earthquake. Seismological Research Letters, 76(1), 10-26.

Lozos, J. C. (2021). The effect of along-strike variation in dip on rupture propagation on strike-slip faults. Geosphere, 17(6), 1616-1630.

Murray, J., & Langbein, J. (2006). Slip on the San Andreas Fault at Parkfield, California, over two earthquake cycles, and the implications for seismic hazard. Bulletin of the Seismological Society of America, 96(4B), S283-S303.

Oglesby, D. D. (2005). The dynamics of strike-slip step-overs with linking dip-slip faults. Bulletin of the Seismological Society of America, 95(5), 1604-1622.

Rymer, M. J., Tinsley, J. C., Treiman, J. A., Arrowsmith, J. R., Clahan, K. B., Rosinski, A. M., ... & Bawden, G. W. (2006). Surface fault slip associated with the 2004 Parkfield, California, earthquake. Bulletin of the Seismological Society of America, 96(4B), S11-S27.

Schaff, D. P., Bokelmann, G. H., Beroza, G. C., Waldhauser, F., & Ellsworth, W. L. (2002). High−resolution image of Calaveras fault seismicity. Journal of Geophysical Research: Solid Earth, 107(B9), ESE-5.

Schwartz, S. Y., Orange, D. L., & Anderson, R. S. (1990). Complex fault interactions in a restraining bend on the San Andreas fault, southern Santa Cruz Mountains, California. Geophysical Research Letters, 17(8), 1207-1210.

Shelly, D. R. (2020). A high−resolution seismic catalog for the initial 2019 Ridgecrest earthquake sequence: Foreshocks, aftershocks, and faulting complexity. Seismological Research Letters, 91(4), 1971-1978.

Simpson, R. W., Barall, M., Langbein, J., Murray, J. R., & Rymer, M. J. (2006). San Andreas fault geometry in the Parkfield, California, region. Bulletin of the Seismological Society of America, 96(4B), S28-S37.

Toda, S., & Stein, R. S. (2002). Response of the San Andreas fault to the 1983 Coalinga−Nuñez earthquakes: An application of interaction−based probabilities for Parkfield. Journal of Geophysical Research: Solid Earth, 107(B6), ESE-6.

Wesnousky, S. G. (2008). Displacement and geometrical characteristics of earthquake surface ruptures: Issues and implications for seismic-hazard analysis and the process of earthquake rupture. Bulletin of the Seismological Society of America, 98(4), 1609-1632.

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Graymer, R.W., Langenheim, V.E., Simpson, R.W., Jachens, R.C. & Ponce, D.A., 2007. Relatively simple through-going fault planes at large-earthquake depth may be concealed by the surface complexity of strike-slip faults. *Geological Society, London, Special Publications*, **290**, 189–201, Geological Society of London. doi:10.1144/SP290.5

Haslinger, F. & Kissling, E., 2001. Investigating effects of 3-D ray tracing methods in local earthquake tomography. *Physics of the Earth and Planetary Interiors*, **123**, 103–114. doi:10.1016/S0031-9201(00)00204-1

Hauksson, E., Stock, J.M. & Husker, A.L., 2022. Seismicity in a weak crust: the transtensional tectonics of the Brawley Seismic Zone section of the Pacific–North America Plate Boundary in Southern California, USA. *Geophysical Journal International*, **231**, 717–735. doi:10.1093/gji/ggac205

Hauksson, E., Yang, W. & Shearer, P.M., 2012. Waveform Relocated Earthquake Catalog for Southern California (1981 to June 2011). *Bulletin of the Seismological Society of America*, **102**, 2239–2244. doi:10.1785/0120120010

Holland, A.A., 2013. Earthquakes Triggered by Hydraulic Fracturing in South-Central Oklahoma. *Bulletin of the Seismological Society of America*, **103**, 1784–1792. doi:10.1785/0120120109

Kilb, D. & Rubin, A.M., 2002. Implications of diverse fault orientations imaged in relocated aftershocks of the Mount Lewis, ML 5.7, California, earthquake. *Journal of Geophysical Research: Solid Earth*, **107**, ESE 5-1-ESE 5-17. doi:https://doi.org/10.1029/2001JB000149

Kroll, K.A., Cochran, E.S., Richards−Dinger, K.B. & Sumy, D.F., 2013. Aftershocks of the 2010 Mw 7.2 El Mayor-Cucapah earthquake reveal complex faulting in the Yuha Desert, California. *Journal of Geophysical Research: Solid Earth*, **118**, 6146–6164. doi:10.1002/2013JB010529

Liu, J., Sieh, K. & Hauksson, E., 2003. A Structural Interpretation of the Aftershock “Cloud” of the 1992 Mw 7.3 Landers Earthquake. *Bulletin of the Seismological Society of America*, **93**, 1333–1344, GeoScienceWorld. doi:10.1785/0120020060

Ross, Z.E., Idini, B., Jia, Z., Stephenson, O.L., Zhong, M., Wang, X., Zhan, Z., *et al.*, 2019. Hierarchical interlocked orthogonal faulting in the 2019 Ridgecrest earthquake sequence. *Science*, **366**, 346–351. doi:10.1126/science.aaz0109

Schaff, D.P., Bokelmann, G.H.R., Beroza, G.C., Waldhauser, F. & Ellsworth, W.L., 2002. High-resolution image of Calaveras Fault seismicity. *Journal of Geophysical Research: Solid Earth*, **107**, ESE 5-1-ESE 5-16. doi:10.1029/2001JB000633

Scholz, C.H., 2019. *The Mechanics of Earthquakes and Faulting*, Cambridge University Press.

Simpson, R.W., Barall, M., Langbein, J., Murray, J.R. & Rymer, M.J., 2006. San Andreas Fault Geometry in the Parkfield, California, Region. *Bulletin of the Seismological Society of America*, **96**, S28–S37. doi:10.1785/0120050824

Thurber, C.H., Zhang, H., Waldhauser, F., Hardebeck, J., Michael, A. & Eberhart-Phillips, D., 2006. Three-Dimensional Compressional Wavespeed Model, Earthquake Relocations, and Focal

Mechanisms for the Parkfield, California, Region. *Bulletin of the Seismological Society of America*, **96**, S38–S49. doi:10.1785/0120050825