Reviewer Comments

Reviewer A

For author and editor

This paper investigates the relationship between surface creep during the June to August 2021 creep event on Calaveras Fault and the seismicity at depth. It builds an enhanced earthquake catalog that better characterizes the spatiotemporal pattern of the earthquakes using various earthquake detection and location methods. In particular, the depth of the seismicity is validated against different velocity models and location algorithms. The results are very interesting. Two seismicity migrations are identified (March to May and June to August migrations) from the enhanced catalog, which differs from the interpretation of continuous spatial migration of events from March to August by Bilham (2021), indicating there may not be aseismic slip at depth during the surface creep event.

My main concern of the paper is the interpretation of seismicity and surface creep (discussed below, section 1). Since the title of the paper is “Microseismicity at the Time of a Large Creep Event on the Calaveras Fault is Unresponsive to Stress Changes”, relationship between seismicity at depth and surface creep seems to be one of the most important points of the paper. However, I think this relationship is not well elaborated. Some of the wording is not accurate and may lead to ambiguous interpretations. Text and figures can be improved to be more rigorous and better match each other. In summary, the paper requires a major revision before it can be accepted.

1. Interpretation of seismicity and surface creep

- Line 94 and 199: There is no citation for Bilham 2021 in the reference.
- Line 200: Label the 3 clusters in Fig. 4c.
- Line 201: Explain what “lack of pattern in depth” means?
- Line 256: Please elaborate what “consistent detail” is?
- Line 257: How many “other regional short-term migrations” have you identified?
- Line 258: In what aspect does the two migrations “resemble each other”? Please describe the two migrations first before comparing them. Are there any difference between them?
- After reading the paper, it is still unclear to me what the authors think the relationship between the surface creep and the seismicity at depth is. For instance, in Line 259, it says migration pattern may not be an “indicative of aseismic creep at depth”. In Line 283-285, it says the surface creep event “does not have obvious manifestations in seismicity”. Do you mean that the seismicity and surface creep are completely independent? Or the seismicity is triggered by surface creep but there is no aseismic slip at depth? Please modify section 4.2 and elaborate in more details of the supports/arguments for there is (or no) relationship between the surface creep and the seismicity at depth.

Can we interpret the March-May migration as a low amplitude aseismic slip at depth that can't be measured by the creepmeter?

If the surface creep is not responsible for the apparent southward migration of the seismicity, what might be the cause of that?

1. Velocity models and depth of seismicity:

- What are the different velocity models like? Can you provide depth profiles of the different models in the supplement?
- Fig. S4c. It is not very clear what "using multiple velocity models" means? Is it a 3D velocity model? How is traveltime calculated "using multiple velocity models"?
- Station correction term. Could you clarify how is station correction term calculated? Are they from NCEDC or results from HYPOINVERSE?
- For the grey dots in Fig. 4, are the locations from NCEDC catalog or inversion results from this paper?

1. Descriptions that need to be improved:

- Line 97-105: If the seismicity in June occurred "at the time of the onset" of the accelerated surface slip, and the earthquake cluster in July/August is "contemporaneous" with the largest surface displacement, what does the "lag" between aseismic slip at depth and slip at surface refer to?
- Line 138: Are the 1,893 earthquakes located in the red box in Fig. 1, what are the spatial criteria selecting these events?
- Line 204-205: What is the "Double-difference Earthquake Catalog for Northern California", where is this catalog cited in the paper? Which catalog are the grey events in Fig. 4 from?
- Line 228: What does "old seismicity patterns" refer to? What is the logic between Line 227-228 and Line 225-226?
- Line 247-249: Add a citation for the 2nd Why will a "bump" increase shear stress on the fault?
- Line 258: Label the two migrations in Fig. 4.

1. The following sentences are a little difficult to understand, please consider rephrasing them:

- Line 78-79: The largest … at depth.
- Line 81-83: Standard … (Segall, 2010).

1. Figures

Fig. 1

- Line 49. Add locations of Gilroy and Hollister to Fig. 1
- Add a figure summary after “Figure 1” before introducing each symbol.
- Introduce the inserted map.
- Add a scale bar.

Fig. 2

- Add x, y labels to Fig. 2b
- Line 110: hand->and
- A) label “number of earthquakes/day”

1. Minor issues

- Line 96: CO2, subscript
- Line 140: missing a preposition after 0.6

Reviewer Comments

Reviewer B

For author and editor

The manuscript "Microseismicity at the Time of a Large Creep Event on the Calaveras Fault is Unresponsive to Stress Changes" by Huang et al. analyzes the relation between seismicity and a large creep event on the Calaveras fault in 2021. For their analysis, the authors improved the earthquake catalog using different detection techniques finding 3 times the number of events, and then compared their improved catalog with surface slip measurements. From their analysis, the authors conclude that their results favor that seismicity is controlled by structural heterogeneities and the creep event has little influence on seismicity patterns.

The manuscript seems to be of particular importance since it contradicts a previous conclusion by Bilham (2021), though this reference is not listed on the bibliography (a quick google search also did not return any article or abstract). The analysis appears to be sound, and the conclusions are well-supported by their results.

My main comments/questions regarding the manuscript are:

1. Regarding the different conclusion to Bilham (2021), if the authors have access to the catalog use by Bilham (is it the NCEDC catalog?), I think it would be helpful to have a figure comparing both catalogs. Maybe a second figure 4 with the catalog used by Bilham will help make the difference more obvious to the reader.

1. I have a few questions regarding the magnitude of completeness. First, is the migration analysis done using only earthquakes above the magnitude of completeness? Figure 4 appears to suggest that.

1. How is the magnitude of completeness estimated? Line 188 says completeness was estimated using a maximum likelihood estimation following Aki (1965), but from what I understand it is the b-value that is estimated using maximum likelihood and the limit magnitude needs to be inputted on the b-value estimation.

1. Also regarding completeness, there seems to be a "kink" in the magnitude distribution around magnitude 1. Do the authors have an explanation for this "kink"? This is to question if the magnitude of completeness difference between the catalogs is actually that large? The magnitude distribution is very similar on both catalogs almost until

magnitude 1, so why is the original catalog completeness 1.4? Would considering a smaller magnitude of completeness in the original catalog change the results or is the improved detected catalog essential for the conclusion?

1. On the interpretation of migration (section 4.2), I wonder if it would be possible to quantify some of the descriptions, perhaps calculating the average along-fault or along latitude epicenter location. In particular, it is not obvious to me from figure 4 the assertion on line 258 that similar migration patterns are observed at the mentioned periods.

I leave some additional smaller comments below. I hope the authors find my comments to be constructive, and best of luck with the review process.

Line 123 – the description of the instruments is a bit confusing to me. Do all the stations used have broadband, short-period instruments and accelerometers, or the stations used include these different types of instruments?

Line 132 – what window overlap was used to perform the PhaseNet detection?

Line 207 – I'm confused by the use of a coefficient of 0.8 to identify clusters, whereas in the relocation procedure the correlation coefficient of 0.9 was used. Why the different thresholds?

Supplementary Figures – Figures S2 and S4 appear to be detached of their captions.

Figure S3 has a typo in "difference".

Dear Dr. Llenos,

Please find enclosed the response to the reviews of '*Microseismicity at the Time of a Large Creep Event on the Calaveras Fault is Unresponsive to Stress Changes*' by Litong Huang, Susan Y. Schwartz, and Emily E. Brodsky. We responded to all the reviewers' comments and revised our paper accordingly.

The original reviewer comments are indicated in black with our responses shown in blue and the changes made to the text in red.

Thank you!

Litong Huang
Susan Y. Schwartz
Emily E. Brodsky

Dear Litong Huang, Susan Y Schwartz, Emily E Brodsky:

I hope this email finds you well. I have reached a decision regarding your submission to Seismica, "Microseismicity at the Time of a Large Creep Event on the Calaveras Fault is Unresponsive to Stress Changes". 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 are supportive of the work being published as a Research Article in Seismica. However, they suggest that some revisions are needed before publication. I and the reviewers all found the topic and results quite interesting, but some additional details would be helpful, particularly in support of the interpretation of the results.

Please find the Reviewer Comments appended below. 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 response, 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,

Andrea Llenos
U.S. Geological Survey
andrea.llenos@seismica.org

Reviewer A:

The manuscript “Microseismicity at the Time of a Large Creep Event on the Calaveras Fault is Unresponsive to Stress Changes” by Huang et al. analyzes the relation between seismicity and a large creep event on the Calaveras fault in 2021. For their analysis, the authors improved the earthquake catalog using different detection techniques finding 3 times the number of events, and then compared their improved catalog with surface slip measurements. From their analysis, the authors conclude that their results favor that seismicity is controlled by structural heterogeneities and the creep event has little influence on seismicity patterns.

The manuscript seems to be of particular importance since it contradicts a previous conclusion by Bilham (2021), though this reference is not listed on the bibliography (a quick google search also did not return any article or abstract). The analysis appears to be sound, and the conclusions are well-supported by their results.

We are very encouraged by reviewer A's positive evaluation of our results and insightful comments.

We thank reviewer A for pointing out the absence of Bilham (2021) in the bibliography. However, the primary importance of the work is not necessarily to contradict Bilham. We now make this clear by reframing the abstract and intro to state that 'This exceptionally well-instrumented event provides an excellent opportunity to investigate the relationship between earthquakes and large surface creep.'

Nonetheless, we include the citation to the Bilham work which was presented at the SCEC annual meeting:

Bilham, R., Langbein, J. O., Ericksen, T. L., Nevitt, J. M., Brooks, B. A., & Mencin, D. J. (2021, 08). Fault-zone gas venting and aseismic slip: ventilation or lubrication?. Poster Presentation at 2021 SCEC Annual Meeting.

My main comments/questions regarding the manuscript are:

1. Regarding the different conclusion to Bilham (2021), if the authors have access to the catalog use by Bilham (is it the NCEDC catalog?), I think it would be helpful to have a figure comparing both catalogs. Maybe a second figure 4 with the catalog used by Bilham will help make the difference more obvious to the reader.

Thank you for asking to make this clear. Bilham(2021) used the NCEDC catalog, which is the starting point of this study and plotted in Figure 2. Now we clarify:

Line 93: Bilham et al. (2021) suggested a potential southeastward migration of microseismic clusters **from examing the Northern California Earthquake Data Center (NCEDC) catalog**(Figure 2) spanning from March to August, associated with the 2021 Calaveras creep event. Concurrently, they observed an increase in Radon and CO2 gas emissions detected at a proximal borehole.

2. I have a few questions regarding the magnitude of completeness. First, is the migration analysis done using only earthquakes above the magnitude of completeness? Figure 4 appears to suggest that.

We thank the reviewer for pointing out the confusion regarding the magnitude of completeness in our migration analysis. The migration analysis actually included all detected earthquakes within a specific distance from the fault (within 1 km on the west side and 3 km on the east side). We have added more detailed information to the caption for Figure 4 to clarify this point.

Line 219: In the figure caption, we added: " All detected earthquakes are selected within 1km on the left side and 3km on the right side to the Calaveras fault."

3. How is the magnitude of completeness estimated? Line 188 says completeness was estimated using a maximum likelihood estimation following Aki (1965), but from what I understand it is the b-value that is estimated using maximum likelihood and the limit magnitude needs to be inputted on the b-value estimation.

Thank you for pointing out the need for clarification. In this study, we searched for the magnitude of completeness (Mc) that minimizes the average squared error of the fitted Gutenberg-Richter line, and we used Maximum Likelihood Estimation (MLE) following Aki (1965) for the b-value estimation. Upon reviewing our code, we found an error where we inadvertently minimized the average absolute error instead of the average squared error. We have corrected this error and updated Figure 3 accordingly.

Line 190: Notably, our earthquake magnitude of completeness is 0.5, determined by minimizing the average squared error of the fitted Gutenberg-Richter law, with the b-value estimated using Maximum Likelihood Estimation (Aki, 1965).

Figure 3: Replotted to reflect the corrected estimation method.

4. Also regarding completeness, there seems to be a "kink" in the magnitude distribution around magnitude 1. Do the authors have an explanation for this "kink"? This is to question if the magnitude of completeness difference between the catalogs is actually that large? The magnitude distribution is very similar on both catalogs almost until magnitude 1, so why is the original catalog completeness 1.4? Would considering a smaller magnitude of completeness in the original catalog change the results or is the improved detected catalog essential for the conclusion?

We appreciate the reviewer's insightful observation regarding the "kink" in the magnitude distribution. This observation has prompted us to closely examine the data. The upward bend toward smaller magnitudes around M1.0 is indeed intriguing. However, an upward bend indicating more complete reassures us that our magnitude completeness estimation is not lower than it should be. We attribute this "kink" to the heterogeneity in magnitude distribution along the fault. Specifically, the southern part around latitude 36.85 has better equipment coverage and is more active as being closer to the fault junction, resulting in more smaller earthquakes detected in this area which are illustrated by the lower red/orange dots in Fig. 4a.

Regarding the comparison of magnitude completeness, we observe more earthquakes in our catalog between M1~M1.4 on the histogram in Fig. 4b. This suggests that the original catalog's completeness level of 1.4 is reasonable.

By showing that our catalog has a lower magnitude of completeness, we aim to show that even with more smaller earthquakes detected and a significant improvement in our catalog, we still do not observe a continuous migration pattern. This strengthens our conclusion of the absence of migration.

5. On the interpretation of migration (section 4.2), I wonder if it would be possible to quantify some of the descriptions, perhaps calculating the average along-fault or along latitude epicenter location. In particular, it is not obvious to me from figure 4 the assertion on line 258 that similar migration patterns are observed at the mentioned periods.

We are glad the reviewer thinks these patterns are different. Our point here is that some initial data inspection might have erroneously identified a pattern. We tried to make this clear in line 267 where we state "it would be imprudent to interpret the migration pattern as indicative of aseismic creep at depth." Therefore, adding an additional figure trying to quantify a pattern that we advise against interpreting would be very confusing. Instead, we now rephrase the original line 266 sentence to state, "For instance, the seismicity pattern observed from March to May and from June to August (Figure 4) both show a southward migration." The reframing discussed above of the abstract and intro might also help set the correct tone here.

Line 264: we rephrase: "For instance, the seismicity pattern observed from March to May and from June to August (Figure 4) both show a southward migration. "

Line 267: we rephrase: "Therefore, it would be imprudent to rule out the possibility that these migrations are coincidentally arranged background seismicity and interpret the migration pattern as indicative of aseismic creep at depth."

I leave some additional smaller comments below. I hope the authors find my comments to be constructive, and best of luck with the review process.

Line 123 – the description of the instruments is a bit confusing to me. Do all the stations used have broadband, short-period instruments and accelerometers, or the stations used include these different types of instruments?

Thank you for addressing the confusing sentence. The stations include these different types of instruments. We now rephrase the sentence to clarify:

Line 123: The 21 seismic stations used for this study include a variety of instruments: some stations have broadband seismometers, others have accelerometers, and some are equipped with short-period seismometers. The waveform data used for each station depends on the specific instruments available at that station.

Line 132 – what window overlap was used to perform the PhaseNet detection?

The waveform we used for PhaseNet detection is one waveform per day per channel, so there is no overlap between daily waveforms. However, we adapted template matching and visual inspection to avoid missing detections.

Line 207 – I'm confused by the use of a coefficient of 0.8 to identify clusters, whereas in the relocation procedure the correlation coefficient of 0.9 was used. Why the different thresholds?

Thank you for highlighting this point of confusion. We tested cross-correlation thresholds ranging from 0.4 to 0.9 with 0.1 spacing. The different thresholds are chosen based on their respective purposes. In the relocation map, we aim to show the earthquake locations as accurately as possible, so we used a higher coefficient threshold of 0.9. To identify clusters, we aim to demonstrate that most of our earthquakes are repeated or near-repeated historical events. Therefore, we selected a threshold of 0.8 as a representative value. To clarify, we have added a table in the supplement showing the clustered rates.

Added table S2 to the supplement.

Supplementary Figures – Figures S2 and S4 appear to be detached of their captions.

Figure S3 has a typo in "difference".

Thank you! We rearranged the figures in the supplement and fixed the typo.

Recommendation: Revisions Required

Reviewer B:

This paper investigates the relationship between surface creep during the June to August 2021 creep event on Calaveras Fault and the seismicity at depth. It builds an enhanced earthquake catalog that better characterizes the spatiotemporal pattern of the earthquakes using various earthquake detection and location methods. In particular, the depth of the seismicity is validated against different velocity models and location algorithms. The results are very interesting. Two seismicity migrations are identified (March to May and June to August migrations) from the enhanced catalog, which differs from the interpretation of continuous spatial migration of events from March to August by Bilham (2021), indicating there may not be aseismic slip at depth during the surface creep event.

My main concern of the paper is the interpretation of seismicity and surface creep (discussed below, section 1). Since the title of the paper is "Microseismicity at the Time of a Large Creep Event on the Calaveras Fault is Unresponsive to Stress Changes", relationship between seismicity at depth and surface creep seems to be one of the most important points of the paper. However, I think this relationship is not well elaborated. Some of the wording is not accurate and may lead to ambiguous interpretations. Text and figures can be improved to be more rigorous and better match each other. In summary, the paper requires a major revision before it can be accepted.

Thank you for highlighting the need for clearer elaboration of our arguments. This research was motivated by a well-recorded large creep event on the Calaveras fault, and we aimed to investigate whether there is a connection between the surface creep event and the seismicity. Upon enhancing the earthquake catalog, our findings indicated that the migration is not robust; it is neither continuous along the fault nor monotonic. Consequently, we concluded that no evidence supports a relationship between seismicity at depth and surface creep.

We appreciate Reviewer B's comments very much, as they have helped us recognize that our initial emphasis might have been misleading, potentially causing confusion about the relationship between seismicity at depth and surface creep. We have rephrased the abstract and non-technical summary to articulate our workflow and argument better. We also replotted Figure 5 and rewrote section 4.1 to make it less ambiguous.

1. Interpretation of seismicity and surface creep

- Line 94 and 199: There is no citation for Bilham 2021 in the reference.

Thank you for pointing this out. Now we include the citation to the Bilham SCEC presentation here:

Bilham, R., Langbein, J. O., Ericksen, T. L., Nevitt, J. M., Brooks, B. A., & Mencin, D. J. (2021, 08). Fault-zone gas venting and aseismic slip: ventilation or lubrication?. Poster Presentation at 2021 SCEC Annual Meeting. https://www.scec.org/meetings/2021/am/poster/094

- Line 200: Label the 3 clusters in Fig. 4c.

Thank you for highlighting the need for clarification of the figure. However, the revised abstract makes it clear that we are not claiming migrations and thus it's better we do not show the non-migrations to avoid further confusion.

- Line 201: Explain what "lack of pattern in depth" means?

Thank you for requesting clarification on this. "lack of pattern in depth" means that we did not observe a systematic vertical migration of seismicity, which would indicate a propagating aseismic slip. If the aseismic slip related to the surface creep were influencing the deep earthquakes, we would expect to see a consistent upward or downward migration pattern in the depth of seismicity. However, our results did not reveal such a pattern. Therefore, we conclude that the seismicity lacks a coherent pattern in depth, suggesting no evidence for a propagating aseismic slip.

Line 203: We rephrase the sentence: "The patchiness of the migration and the absence of a systematic vertical pattern suggest that the earthquake locations are not strongly influenced by the surface creep event."

- Line 256: Please elaborate what "consistent detail" is?

By 'consistent detail,' we were trying to say that the migration pattern is mainly represented by the three event groups (blue, green, and orange dots in Fig.4b) but is neither continuous along fault nor monotonic along a uniform direction. Instead, the seismic activity appears to be sporadic. Therefore, it would be imprudent to interpret the migration pattern as indicative of aseismic creep at depth.

Here we rephrase:
Although we observe increases and decreases of seismic activity at various locations over time that are consistent with a southeastward migration, the overall pattern is neither continuous along the fault nor monotonic, lacking consistent detail. Furthermore, it is indistinguishable from other regional short-term migrations.

- Line 257: How many "other regional short-term migrations" have you identified?

We refer to the migration from March to May and from June to August, both of which show a southeastward trend. By stating that the overall pattern is indistinguishable, we mean that the migration is neither continuous nor monotonic and is not unique enough to serve as evidence of deep aseismic propagation. We rephrase:
Furthermore, it is indistinguishable from other regional short-term migrations. For instance, the seismicity pattern observed from March to May and from June to August (Figure 4) both show a southward migration.

We also replotted Figure 5 to show the 2 similar migration patterns more clearly.

- Line 258: In what aspect does the two migrations "resemble each other"? Please describe the two migrations first before comparing them. Are there any difference between them?

The two migrations we refer to are the March to May migration and the June to August migration. Both exhibit a southeastward trend, which is why we noted their resemblance. Specifically, seismic activity appears to move in a generally southeastward direction in both periods, although the patterns are continuous along the fault. We use these two migrations as examples to illustrate that the overall migration is not robust. Therefore, comparing them might divert readers' attention and cause confusion. To clarify, we have rephrased:
Line 198-203: We rephrase: However, contrary to expectations, the newly added events that fill the NCEDC temporal gap in earthquake activity in April and May introduce more variability to the overall migration pattern rather than reinforcing and clarifying its continuous southward propagation. The newly discovered April to May seismicity locates south of the June cluster and therefore interrupts the monotonic southward migration pattern (Figure 4c) initially noted by Bilham et al. (2021).

- After reading the paper, it is still unclear to me what the authors think the relationship between the surface creep and the seismicity at depth is. For instance, in Line 259, it says migration pattern may not be an "indicative of aseismic creep at depth". In Line 283-285, it says the surface creep event "does not have obvious manifestations in seismicity". Do you mean that the seismicity and surface creep are completely independent? Or the seismicity is triggered by surface creep but there is no aseismic slip at depth? Please modify section 4.2 and elaborate in more details of the supports/arguments for there is (or no) relationship between the surface creep and the seismicity at depth.

Thank you for highlighting the need for further elaboration on this crucial statement. We appreciate your attention to detail. Our findings indicate that the seismicity and the surface creep are not directly related, as the seismic patterns do not show any systematic response to the creep event.

Specifically, the lack of continuous and monotonic migration patterns in the seismicity suggests that there is no direct correlation between the surface creep and seismicity at depth.

We revised section 4.2 based on your comments mentioned above. Additionally, we have revised the abstract to ensure our argument is clear from the outset, preventing any confusion.

We also realized that our section 4.1 was not clear to show that the migration is not robust. Thus we replotted figure 5 and revised the section 4.1.

Line 223-231: We add: We generated moment accumulation plots for two distinct time periods to more clearly illustrate how the smaller events in our expanded earthquake catalog disrupt the monotonic southward migration pattern identified between March and August 2021 in the NCEDC catalog. Both time periods indicated in Figure 5, March 20-May 20 and May 21-September 1 possess an apparent southward migration of seismic moment and are not strongly distinguishable from the background pattern (gray lines in Figure 5). This indicates that the level of seismic activity during this period was not significantly stronger than historical patterns, implying that the seismicity is not sufficiently distinct to robustly indicate an influence by the surface creep, even though the timing appears to align well.

- Can we interpret the March-May migration as a low amplitude aseismic slip at depth that can't be measured by the creepmeter?

  This is an interesting and inspiring interpretation. While the lack of creepmeter coverage could result in missed creep events, we observed no obvious signals in the GPS and InSAR data to support this hypothesis. Additionally, the March-May migration is not well-characterized enough to be indicative of an aseismic slip. It could simply be a coincidence of background seismic activity rather than a result of aseismic slip.

- If the surface creep is not responsible for the apparent southward migration of the seismicity, what might be the cause of that?

  Thank you! We rephrase:
  L.267: Therefore, it would be imprudent to rule out the possibility that these migrations are coincidentally arranged background seismicity and interpret the migration pattern as indicative of aseismic creep at depth.

2. Velocity models and depth of seismicity:
   - What are the different velocity models like? Can you provide depth profiles of the different models in the supplement?

   Thank you for your question regarding the velocity models used in our study. In this study, we employed several 1D velocity models, meaning each velocity model is characterized by distinct depth layers and constant P-wave velocities within those layers. Different velocity models have different depth layers and different velocities at those layers.

   We added table S3 as an example of the 1D velocity model.

- Fig. S4c. It is not very clear what “using multiple velocity models” means? Is it a 3D velocity model? How is traveltime calculated “using multiple velocity models”?

Thank you for your question. “Using multiple velocity models” refers to using multiple 1D velocity models for various regions, not a 3D velocity model. HYPOINVERSE calculates traveltime by averaging these models' travel times and travel time derivatives based on weighted regions. For instance, if a location is influenced by two models, the travel times from each are weighted and averaged, ensuring smooth transitions between regions. We added this information to the supplement:
L. 50: HYPOINVERSE allows the use of 1D velocity model or multiple velocity models. In a 1D velocity model, the seismic velocity varies only with depth and is constant within each depth layer. Multiple velocity models consist of many 1D velocity models for specific areas, accounting for reginal variations. Transition regions between these models are managed using weighted averages of travel times and derivatives, ensuring transitions across different areas.

- Station correction term. Could you clarify how is station correction term calculated? Are they from NCEDC or results from HYPOINVERSE?

Thank you for your question regarding the station correction term. We did not calculate station corrections ourselves; instead, we used the station correction files provided by NCEDC. This approach ensures that our earthquake locations are consistent with historical event locations in the NCEDC catalog. To clarify this in the manuscript, we have rephrased the relevant section.
L.161: This approach ensures consistency as we also adopt the same station corrections and velocity models used in the NCEDC catalog (Figure S2).

We also added the link to download the station correction files and velocity model files in the section ‘Data and code availability.’

L.312. The station correction files and velocity models can be found at https://ncedc.org/pub/catalogs/ncsn/hypoinverse/.

- For the grey dots in Fig. 4, are the locations from NCEDC catalog or inversion results from this paper?

Thank you for bringing up this point. The grey dots represent historical events in the NCEDC catalog, while we adopted the location from the Double Difference catalog and the real-time Double Difference catalog. To clarify, we phrase:
L. 206: The distribution of earthquakes revealed by our enhanced catalog is very similar to the historical seismicity distribution between 2012 and 2020 *(represented as gray dots in Figure 4b)* from the Double-difference Earthquake Catalog for Northern California and the Real-Time Double Difference catalog *(Waldhauser, F. and D.P. Schaff, 2008; Schaff, D.P. and F. Waldhauser, 2005)*.

3. Descriptions that need to be improved:

- Line 97-105: If the seismicity in June occurred “at the time of the onset” of the accelerated surface slip, and the earthquake cluster in July/August is “contemporaneous” with the largest surface displacement, what does the “lag” between aseismic slip at depth and slip at surface refer to?

We appreciate the reviewer’s insightful question regarding the observed lag between aseismic slip at depth and slip at the surface. The "lag" refers to the spatial difference between the maximum surface creep and maximum earthquake activity. Specifically, earthquakes reached the area beneath the creepmeter in June, when surface creep had just started. When surface creep peaked in August, the earthquakes had already migrated south. To clarify, we rephrase these lines:
Line 98-103: Following a hiatus in seismicity, earthquake activity picked up southward, locating directly beneath the creepmeter in June coincident with the onset of accelerated surface slip. Seismicity then moved farther south culminating in a notable earthquake cluster in July/August that coincided with the largest surface displacement; however, by this time the seismic activity had already moved past the creepmeter to the south (Figure 2).

- Line 138: Are the 1,893 earthquakes located in the red box in Fig. 1, what are the spatial criteria selecting these events?
  They are plotted as the pink dots in Figure 1, as mentioned in the Figure 1 caption ‘Template earthquakes from the Double Difference Real Time (DDRT) Catalog between 2012-2021 are shown as pink circles’. They were selected from a broader area to fully encompass our research region. To clarify, we revised Line 139:
  For this analysis, we drew upon 1,893 earthquakes from the Real-Time Double-Difference Catalog (DDRT) spanning 2012 to 2021 as template events*, plotted as pink dots in Figure 1* (Waldhauser and Schaff, 2008; Schaff and Waldhauser, 2005).
- Line 204-205: What is the “Double-difference Earthquake Catalog for Northern California”, where is this catalog cited in the paper? Which catalog are the grey events in Fig. 4 from?

Thank you for pointing this out. We have now added the citation to this Catalog in the text. The Double-Difference Earthquake Catalog contains the same events as the NCEDC catalog but with improved locations using the double-difference relocation method to decrease the error of velocity models. The grey dots in Figure 4 represent historical events in the NCEDC catalog from 2012 to 2020, but we adopted the location from the Double Different catalog and the real-time Double Difference catalog.

We added the citations to this line: Waldhauser and Schaff, 2008; Schaff and Waldhauser, 2005

- Line 228: What does “old seismicity patterns” refer to? What is the logic between Line 227-228 and Line 225-226?

The old seismicity patterns refer to the accumulated seismic moment of history events from 1984 to 2020, as plotted with the grey line in Figure 5. Line 227-228 discuss that the timing of the seismicity migration correlates well with the creep event, while line 225-226 indicate that the level of seismic activity during this period was not significantly stronger than historical patterns. This implies that the seismicity is not characterized enough to robustly indicate an influence by the surface creep, even though the timing seems to align well. We added these explanations after line 223-232:

We generated moment accumulation plots for two distinct time periods to more clearly illustrate how the smaller events in our expanded earthquake catalog disrupt the monotonic southward migration pattern identified between March and August 2021 in the NCEDC catalog. Both time periods indicated in Figure 5, March 20-May 20 and May 21-September 1 possess an apparent southward migration of seismic moment and are not strongly distinguishable from the background pattern (gray lines in Figure 5). This indicates that the level of seismic activity during this period was not significantly stronger than historical patterns, implying that the seismicity is not sufficiently distinct to robustly indicate an influence by the surface creep, even though the timing appears to align well.

- Line 247-249: Add a citation for the 2nd Why will a “bump” increase shear stress on the fault?

Thank you for bringing this to our attention. We revised the sentence and added a citation. Line 253-255: Alternatively, the presence of a significant geometric anomaly, such as a large ‘bump’ on the fault surface, could increase the frictional resistance, effectively stabilizing this segment of the fault (Eijsink et al., 2022).

- Line 258: Label the two migrations in Fig. 4.

Thank you for highlighting the need for clarification of the figure. However, the revised abstract makes it clear that we are not claiming migrations and thus it’s better we do not show the non-migrations to avoid further confusion.

4. The following sentences are a little difficult to understand, please consider rephrasing them:

- Line 78-79: The largest … at depth.

Thank you for highlighting the need to clarify these sentences.

Revised to: The largest creep events sometimes skip intermediate surface creepmeters, implying that the slip is continuous at depth while segmented on the surface.

- Line 81-83: Standard … (Segall, 2010).

Revised to: Furthermore, standard elastic theory would suggest that the depth of creep events is comparable to their horizontal extent, implying that the large creep events with significant horizontal extent are likely to extend into the seismogenic zones (Segall, 2010).

5. Figures

Fig. 1

- Line 49. Add locations of Gilroy and Hollister to Fig. 1
- Add a figure summary after "Figure 1" before introducing each symbol.
- Introduce the inserted map.
- Add a scale bar.

Thank you for bringing our attention to the details. We revised the figure and caption as suggested.

Fig. 2

- Add x, y labels to Fig. 2b
- Line 110: hand->and
- A) label "number of earthquakes/day"

We revised the figure and caption as suggested.

6. Minor issues

- Line 96: CO2, subscript

Revised to CO_2

- Line 140: missing a preposition after 0.6

We added a 'for'.

Line 143: During 2021, there were 5413 picks detected as matched events with a cross-correlation coefficient higher than 0.6 *for* at least four stations.

Recommendation: Revisions Required

Round 2 Reviewer Comments

Reviewer A Comments

For author and editor

I appreciate the authors' efforts in revising the manuscript based on the first round of reviews. The changes made have significantly improved the clarity of the paper, making it more accessible to a broader audience.

The paper examines the microseismicity near the creep event on the Calaveras Fault, suggesting that it is unresponsive to stress changes. The three major pieces of evidence presented are: (1) the moment release rate of the microseismicity is not significantly higher than the background pattern, (2) newly detected seismicity disrupts the monotonic migration identified in Bilham et al., 2021, and (3) the spatial distribution of seismicity aligns with historical earthquakes in the region. I believe each of these points would benefit from further discussion regarding the robustness of the observations. My detailed comments are as follows:

4.1 Distribution of Accumulated Seismic Moment

1. **Fig. 5:** The accumulated seismic moment spans several orders of magnitude in Fig. 5. The grey line represents the average over 36 years, while the colored lines show the averages over 2 and 4 months in the two panels. I wonder if the grey line is dominated by several large magnitude events whose recurrence intervals are longer than the 6-month window considered in this study. Additional clarification is needed to justify whether comparing the long-term accumulated seismic moment rate with these shorter time windows is appropriate.
2. **Lines 229-231:** The authors mention that “the level of seismic activity during this period was not significantly stronger than historical patterns.” Are you referring to a specific segment along the fault in Fig. 5, or the entire segment shown in Fig. 5? It appears that the seismic moment release between 3-5 km and 15-20 km is higher than the background. Given that the paper’s title suggests that microseismicity is unresponsive to stress changes from the surface creep event, it is crucial to provide a statistical test to validate that seismic activity is not significantly stronger than the background, or to explain more clearly what “not significantly stronger” means in this context.
3. **Spatial Distribution of Seismicity:** The paper states that the detected earthquakes in 2021 follow the pattern of historical earthquakes (e.g. Lines 294-295). A persistent seismicity gap at 39N is noted, with low seismicity rates even during the surface creep event. While structural heterogeneities can influence the spatial distribution of seismicity, the response of seismicity to surface creep is a temporal behavior. Areas with more historical earthquakes might produce more earthquakes due to stress perturbations from external sources. Should we expect that seismicity influenced by a surface creep event would follow a different spatial distribution than historical earthquakes? The overall spatial distribution of seismicity and the temporal migration

or increase in seismicity rates are somewhat distinct phenomena. I encourage the authors to elaborate further on why a sequence of earthquakes following the spatial pattern of historical earthquakes does not necessarily indicate it is influenced by stress changes.

4.2 Interpretation of Migration

1. **Definition of "Background" Seismicity (Line 268):** I am unclear about the authors' definition of "background" seismicity in this section. In statistical seismology, "background seismicity" refers to the baseline level of earthquake activity in a region, independent of specific earthquake sequences or clusters, such as aftershocks or swarms. Can the authors clarify what they mean by "background" seismicity in this context? Are the earthquakes from March to August considered baseline seismicity in the statistical seismology sense, implying they don't involve any transients?

Other Comments

- The authors estimate that the horizontal dimension of the surface creep event in 2021 is about 12 km. Are there any additional constraints or evidence on the propagation direction of this event beyond the point measurement at station XSH1?

Reviewer B Comments

For author and editor

I appreciate the authors' clarifications and changes to the manuscript. After reading the revised manuscript, I think this new version makes it easier to follow the authors' arguments and the conclusions clearer. Nonetheless, I still have some concerns that hopefully are easy to address:

- I understand the authors' attempt to avoid confusion in the reader, but I would still encourage them to make it clear, on figure 4 for example, the cluster migrations they are referring to. I believe it will make it easier for the readers to follow the discussion and the observation that similar migrations happen at multiple periods.

- Regarding the seismicity gap, since the entire catalog was used in the analysis is the gap robust/unique when considering only the earthquakes above magnitude of completeness?

- Could you mark on Figure 1 or 4a where the along fault distances of Figure 5 start?

Minor:
- Supplementary file line 53: "reginal"

Dear Dr. Llenos,

Please find enclosed the response to the second-round reviews of '*Microseismicity at the Time of a Large Creep Event on the Calaveras Fault is Unresponsive to Stress Changes*' by Litong Huang, Susan Y. Schwartz, and Emily E. Brodsky. We responded to all the reviewers' comments and revised our paper accordingly.

The original reviewer comments are indicated in black with our responses shown in blue and the changes made to the text in red.

Thank you!

Litong Huang
Susan Y. Schwartz
Emily E. Brodsky

Dear Litong Huang, Susan Y Schwartz, Emily E Brodsky:

I hope this email finds you well. I have reached a decision regarding your revised submission to Seismica, "Microseismicity at the Time of a Large Creep Event on the Calaveras Fault is Unresponsive to Stress Changes". 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. The reviewers and I appreciate the changes that were made in response to their previous reviews and believe that the revised version is much clearer. However, they suggest that a few more revisions are needed before publication, primarily to clarify a few points particularly around the seismicity gap and interpretation of migration. I believe that their comments can be fairly easily addressed with some minor revisions.

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,

Andrea Llenos

U.S. Geological Survey

andrea.llenos@seismica.org

Reviewer A:

I appreciate the authors' clarifications and changes to the manuscript. After reading the revised manuscript, I think this new version makes it easier to follow the authors' arguments and the conclusions clearer. Nonetheless, I still have some concerns that hopefully are easy to address:

We sincerely appreciate your constructive feedback and have made revisions to the manuscript accordingly.

- I understand the authors' attempt to avoid confusion in the reader, but I would still encourage them to make it clear, on figure 4 for example, the cluster migrations they are referring to. I believe it will make it easier for the readers to follow the discussion and the observation that similar migrations happen at multiple periods.

We agree with Reviewer A's suggestion that marking the clusters in Figure 4 will make it clearer for readers to follow. Accordingly, we have added notations such as 'Cluster 1' and 'Cluster 2' to Figure 4 to indicate the main clusters. Additionally, we have adjusted the text in the manuscript to align with these labels, making it easier for readers to follow the discussion.

Figure 4 and caption: notations indicating the three main clusters are added.

Line 204: The newly discovered April to May seismicity locates south of the June cluster and therefore interrupts the monotonic southward migration pattern *from cluster 1 to cluster 3*(Figure 4c) initially noted by Bilham et al. (2021).

Following the same logic to make the migration patterns easier to follow, we added 2 gray dashed arrows in Figure 4c. We revised:

Line 266: For instance, the newly discovered April to May 2021 seismicity located in the most southern section led to a seismicity pattern from March to May with a southward migration, similar to the one from June to August (*dashed gray arrows in* Figure 4c).

- Regarding the seismicity gap, since the entire catalog was used in the analysis is the gap robust/unique when considering only the earthquakes above magnitude of completeness?

Thank you for bringing this possible question to our attention. We agree that considering only the earthquakes above the magnitude of completeness is necessary and more compelling. However, we also see a value to presenting all the new earthquakes we detected in the paper. To reconcile these competing needs, we replotted Figure 4 and Figure 5 with the events above corresponding Mc and put them in the supplementary figures as additional information.

Added: Figure S4 and Figure S5.

- Could you mark on Figure 1 or 4a where the along fault distances of Figure 5 start?

Thank you for highlighting this. The along-fault distances of the earthquakes are measured from the northwest point of the reference line to the projections of the earthquakes on this line. The reference line, which is parallel to the central segment of the fault, has been added as a gray dashed line in Figure 4a. We also added the latitude axis in Figure 5 in addition to the distance axis.

Added the reference line in Figure 4a.

Added to Figure 4 caption: The gray dashed line in the map view is the reference line for along fault distances of the earthquakes.

Added: Line 224: The along-fault distances of the earthquakes are measured from the northwest point of the reference line in Figure 4a to the projections of the earthquakes on this line, and they are further illustrated in Figure S7.

Added Figure S7.

Minor:
- Supplementary file line 53: “reginal”

Thank you for pointing out this typo.
Supplementary Line 53: reginal ⟶ regional.

Recommendation: Revisions Required
.

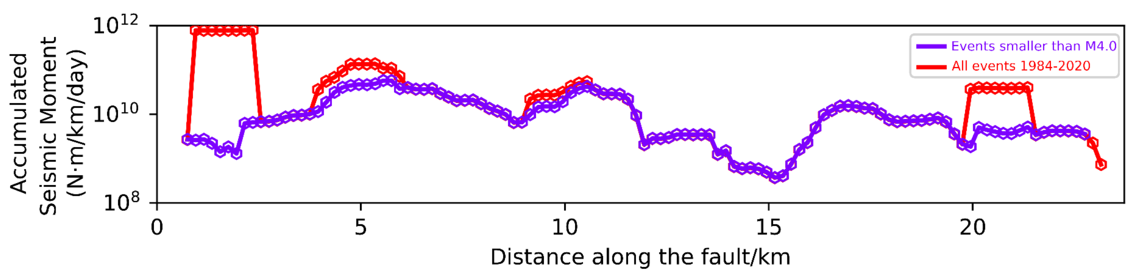
Reviewer B:

I appreciate the authors' efforts in revising the manuscript based on the first round of reviews. The changes made have significantly improved the clarity of the paper, making it more accessible to a broader audience.

The paper examines the microseismicity near the creep event on the Calaveras Fault, suggesting that it is unresponsive to stress changes. The three major pieces of evidence presented are: (1) the moment release rate of the microseismicity is not significantly higher than the background pattern, (2) newly detected seismicity disrupts the monotonic migration identified in Bilham et al., 2021, and (3) the spatial distribution of seismicity aligns with historical earthquakes in the region. I believe each of these points would benefit from further discussion regarding the robustness of the observations. My detailed comments are as follows:

4.1 Distribution of Accumulated Seismic Moment

1. **Fig. 5:** The accumulated seismic moment spans several orders of magnitude in Fig. 5. The grey line represents the average over 36 years, while the colored lines show the averages over 2 and 4 months in the two panels. I wonder if the grey line is dominated by several large magnitude events whose recurrence intervals are longer than the 6-month window considered in this study. Additional clarification is needed to justify whether comparing the long-term accumulated seismic moment rate with these shorter time windows is appropriate.



Thank you for this insightful comment. To address your concern, we compared the accumulated seismic moment from 1984-2020 for all events and those smaller than M4.0. The comparison revealed a similar distribution, indicating that large events with longer recurrence intervals do not dominate the area. We have added this figure to the supplementary.

Figure S8 added.

2. **Lines 229-231:** The authors mention that “the level of seismic activity during this period was not significantly stronger than historical patterns.” Are you referring to a specific

segment along the fault in Fig. 5, or the entire segment shown in Fig. 5? It appears that the seismic moment release between 3-5 km and 15-20 km is higher than the background. Given that the paper's title suggests that microseismicity is unresponsive to stress changes from the surface creep event, it is crucial to provide a statistical test to validate that seismic activity is not significantly stronger than the background, or to explain more clearly what "not significantly stronger" means in this context.

Thank you for pointing out this ambiguity. We were referring to the entire segment shown in Fig. 5 in the sentence "was not significantly stronger than historical patterns", though some segment has higher seismic moment release than the background, generally and averagely the entire segment is not more active. We were trying to say that the seismicity activity during the research time window is not different enough to be distinguished from the historical events. To avoid any misunderstanding or unintended statistical implications, we will remove the word "significantly" from the text.

Line 231: This indicates that the level of seismic activity during this period was not ~~significantly stronger~~ higher than historical patterns, implying that the seismicity is not sufficiently distinct to robustly indicate an influence by the surface creep, even though the timing appears to align well.

3. **Spatial Distribution of Seismicity:** The paper states that the detected earthquakes in 2021 follow the pattern of historical earthquakes (e.g. Lines 294-295). A persistent seismicity gap at 39N is noted, with low seismicity rates even during the surface creep event. While structural heterogeneities can influence the spatial distribution of seismicity, the response of seismicity to surface creep is a temporal behavior. Areas with more historical earthquakes might produce more earthquakes due to stress perturbations from external sources. Should we expect that seismicity influenced by a surface creep event would follow a different spatial distribution than historical earthquakes? The overall spatial distribution of seismicity and the temporal migration or increase in seismicity rates are somewhat distinct phenomena. I encourage the authors to elaborate further on why a sequence of earthquakes following the spatial pattern of historical earthquakes does not necessarily indicate it is influenced by stress changes.

We agree that earthquakes triggered by stress changes would also follow structural spatial heterogeneity. However, if stress change had triggered these events, we would expect a significantly higher level of seismic activity compared to the background, which is not evident in Figure 5. Therefore, we do not have strong evidence that these earthquakes were influenced by stress changes related to the surface creep event. Here we revised:

Line 301: Our results show that one of the largest surface creep events recorded on the Calaveras Fault, with 16 mm of displacement, does not have obvious manifestations in seismicity. *The pattern of the accumulated seismic moment in the study time window is not distinguishable from that of the prior seismicity, which indicates that these earthquakes are not necessarily influenced by the temporal stress change of the surface creep event.* The location of all detected earthquakes in 2021 is consistent with the pattern of historical seismicity. The accumulated seismic moment between 2000 and 2020 near latitude 36.9 is significantly lower than other regions of the fault and this seismicity gap is evident during the 2021 creep event. Our results suggest that seismicity here is mainly controlled by persistent structural heterogeneity that has existed since at least 1984 ~~rather than temporal stress change generated by the surface creep event~~.

4.2 Interpretation of Migration

1. **Definition of "Background" Seismicity (Line 268):** I am unclear about the authors' definition of "background" seismicity in this section. In statistical seismology, "background seismicity" refers to the baseline level of earthquake activity in a region, independent of specific earthquake sequences or clusters, such as aftershocks or swarms. Can the authors clarify what they mean by "background" seismicity in this context? Are the earthquakes from March to August considered baseline seismicity in the statistical seismology sense, implying they don't involve any transients?

Thank you for pointing out that 'background seismicity' is not well-defined and might be misleading. By 'background seismicity' we intended to refer to spontaneous and independent events that are not necessarily triggered by aseismic slip. We will revise the text to clarify.

Line 268: Therefore, it would be imprudent to rule out the possibility that these migrations are coincidentally arranged *spontaneous* seismicity and interpret the migration pattern as indicative of aseismic creep at depth.

Other Comments

- The authors estimate that the horizontal dimension of the surface creep event in 2021 is about 12 km. Are there any additional constraints or evidence on the propagation direction of this event beyond the point measurement at station XSH1?

Unfortunately, creepmeter XSH1 is the only functional creepmeter in the interested region. We also checked the InSAR data and GNSS data, and neither showed an evidential signal about this creep event.

Recommendation: Revisions Required
