Reply to Reviewer A

This review is for the revised version of the original manuscript. The authors took great care in addressing each of the review comments, and it is evident in their manuscript, which is significantly improved. In particular, the authors moved the discussion of potency density tensor inversion (a key part of their methodology) to the main text and elaborated on the concept on dynamic overshoot. These main revisions, along with the other minor revisions help frame the paper and put the results into the bigger context of earthquake periodicity and coseismic stress release. As the paper stands, I think it is nearly acceptable for publication. However, I think a couple clarifying statements and slight reorganization would polish the paper and would be quick to implement. I have included my minor comments below. I do not think the manuscript needs to undergo another round of peer review.

Thank you very much for your positive evaluation of our revised manuscript. Following your comments, we have made minor revisions to further improve the paper.

Minor Comments:

- I appreciate the separation of PDTI, data, and model set up. However, the paper jumps right into the methodology of PDTI without context. In the final paragraph of the introduction, I would revise the first paragraph to say "…we estimate the seismic source process of the 2025 Kamchatka earthquake **using potency density tensor inversion of** teleseismic data…"

  We thank the reviewer for the helpful comment. We have modified it as suggested.

- I would recommend clarifying in section *1 Potency Density Tenso*r Inversion that PDTI uses teleseismic body waves for the inversion. Currently, you just state that teleseismic body-wave analysis is advantageous, but it is not clear the PDTI is a type of teleseismic body-wave analysis.

  We thank the reviewer for the helpful suggestion. We have revised the statement accordingly to reflect your suggestion.

  Modified text: "*In Potency Density Tensor Inversion (PDTI) **of teleseismic body waves***"

  Added text: "*PDTI takes advantage of the sensitivity of teleseismic body waves to changes in the focal mechanism, allowing us to estimate fault geometry and rupture evolution during the earthquake.*"

- Should the second paragraph of *Potency Density Tenso*r be moved to *Model Setup*?

  We thank the reviewer for the comment. We understand that the suggestion is based on the idea that the description of smoothing and related settings might fit better in the Model Setup section. However, because determining the hyperparameters (the strength of smoothing and the variance of Green's function errors) using ABIC is a fundamental part of the PDTI methodology, we would prefer to keep the current structure.

- In your response to this question regarding the slip deficit accumulated over 5.8 years, you stated that you clarified in the paper that this was an intraplate earthquake. I do not see this change reflected in the revised manuscript.

Thank you very much for pointing this out. We have corrected this oversight in the revised manuscript.

- Figure 1c: In the previous review, you were asked to add a colorbar and to clarify the dip angle, but I do not see those comments addressed in the review or in the revised manuscript. Is it possible to add a discrete colorbar showing the shades of red and how many overlapping models correspond to the different shades? Regarding the dip angle, it assumed to be the same as the angle used for the 2025 model (16 deg)?

  We thank the reviewer for the helpful comment. Following your comments, we have added the colorbar. We have also added a clarification regarding the dip angle in the figure caption.

- In the previous review, you were asked how many waveforms were retained after removal of low SNR waveforms. Regarding your revised sentence, I don't think it was necessary to add "(in degrees)" as the degree symbol is already included. I also think it would be better to mention that you downloaded data from all available stations but then specify that 80 were retained after

  We thank the reviewer for the helpful comment. We have revised the manuscript accordingly.

Reply to Reviewer B

Thank you for responding to and addressing the reviewers' comments. The manuscript is clearer, more self-contained, the conclusions stronger, and overall greatly improved as a result of the latest changes.

Thank you very much for your positive evaluation of our revised manuscript. Your comments were extremely helpful in clarifying key points and strengthening the overall presentation. Following your comments, we have made minor revisions to further improve the paper.

I only have a few minor comments:

- Line 16 - This statement is not fully supported and can be improved to more accurately reflect the authors' conclusions, similar to Line 240. Since probabilistic hazard forecasting uses the average recurrence interval to define a probability distribution and has a non-zero probability associated with shorter recurrence, the hazard takes even short recurrences into account. However, hazard forecasting typically does not take into account residual strains after an event, so it would be more accurate to say something like - Residual strains after a large event that can potentially result in shorter recurrence are not considered in hazard forecasting.

  We thank the reviewer for the critical comment. We have revised the statement accordingly to reflect your suggestion.

  Modified text: "*This unusually short recurrence interval challenges conventional seismic-cycle models ~~used for hazard forecasting~~*. "

  Added text: "*Therefore, the residual strains that remain after a great earthquake and are not considered in current hazard forecasting can lead to shorter recurrence.*"

- Line 50 - Though the authors have already included the convergence rate of 8 cm/yr on line 40, the total convergence of 6 m would be easier for readers to understand if this sentence also included the convergence rate, e.g., - plate convergence over 73 years at an average rate of 8 cm/yr is only about 6 m…

  We thank the reviewer for the helpful comment. We have modified it as suggested.

- Fig 1e - What is the reason that the Crescent City tidal waveform for the 1952 event does not extend to the left axis, while all other waveforms do?

  We thank the reviewer for the helpful comment. This is because the NOAA digital data for the Crescent City station does not include the record prior to the tsunami arrival. To clarify this, we have added an explanation in the figure caption.

- Line 89 - The "(in degrees)" in parentheses is unnecessary. My interpretation of the comment by previous reviewers is that it would be helpful to also include the associated distance range in km.

  We thank the reviewer for the helpful comment. We have modified it as suggested.

Reply to reviewer #1

This study models the rupture process of the recent 2025 Kamchatka earthquake using potency density tensor inversion to evaluate how an M9-class earthquake ruptured in nearly the same location as another M9-class earthquake only 73 years prior. Based on tectonic plate rates, the accumulated slip deficit should have only been around 6 m, but the 2025 event had over 9 m of slip along some patches. The authors evaluated whether the slip of the 1952 and 2025 events occurred along complementary patches of the fault (instead of the same patches) but concluded that the 2025 event experienced dynamic overshoot. This has important implications for seismic hazard forecasting, which currently assumes large earthquakes essentially reset the fault, and does not consider possibility of dynamic overshoot.

The writing of the paper is fairly clear. However, I think it could benefit from reorganization and needs a more in-depth discussion of certain topics. It seems like there is a lot of important information lacking in the main text. In particular, descriptions of the methodologies used are mostly contained within the supplement. I know a fast report is meant to be a concise paper, but I found it challenging to follow the paper without understanding much of the methodology. If the authors choose to keep these method descriptions in the supplement, they should add a little more info to the main text providing context for the methodologies.

Lastly, the main conclusion of the paper (dynamic overshoot) is not discussed nearly enough. It is unclear to me from your paper what exactly dynamic overshoot is, how it occurs, etc… There does not seem to be much existing literature on this topic either. The authors should expand this section. It may take longer to address my concerns than would be expected for a Fast Report. The authors and handling editor might consider moving this manuscript to the regular issue. My full list of comments are attached.

We sincerely thank the reviewer for this constructive and thoughtful review. We have carefully revised the manuscript following your comments. The description of the Potency Density Tensor Inversion (PDTI) has been incorporated into the main text, and the discussion on dynamic overshoot has been substantially expanded. We believe that your comments have greatly improved the quality and clarity of our paper.

Although I, as the corresponding author, may not fully understand the editorial policies of *Seismica*, we hold great respect for the journal's commitment to social equity in science. I sincerely hope that the *Fast Reports* section will continue to feature high-quality papers that provide new perspectives even shortly after major events, and I would be delighted if our work could contribute to that vision.

General Comments

It would be useful to have a separate paragraph describing the data and processing used for this study before you start talking about PDTI.

We thank the reviewer for the helpful suggestion. Following your advice, we divided the original section into three subsections: PDTI method, Data, and Model setup. We placed the Data and Model setup subsections consecutively, as we believe presenting them together provides a clearer understanding of the study framework.

Throughout the paper, you use the phrases "strain accumulation/release" and "stress accumulation/release". Is “strain” or “stress” the more appropriate word? Please use consistent nomenclature, unless you really are referring to two separate things.

We thank the reviewer for the helpful comment. In this study, we primarily discuss the accumulation and release of stress on the plate interface, but in some contexts we use strain as a qualitative expression of deformation accumulated through interplate coupling. We have checked the entire manuscript to ensure that both terms are used consistently and appropriately according to the context.

Please provide comparison of the depths (hypocenter and main slip) and tsunami heights for the 1952 and 2025 earthquakes.

We thank the reviewer for this insightful comment. In general, the accuracy of hypocentral depth determination differs significantly between the horizontal and vertical directions. Unless stations are situated directly above the source, the uncertainty in depth estimation can be large, and it also depends strongly on the number and spatial distribution of available stations. Therefore, comparing the hypocentral depths of the 1952 and 2025 events, which were determined under very different observational conditions, would not be meaningful.

Similarly, the depth of the main slip area also involves substantial uncertainty, particularly for the 1952 earthquake, as its source model is poorly constrained and depends strongly on the assumed structure and fault geometry. In contrast, the 2025 event is better resolved by modern seismic and geodetic observations. For this reason, we focused primarily on the horizontal distribution of slip rather than its depth, as this approach provides more robust and interpretable results for comparing the two events.

At the same time, we agree with the reviewer that information on the depth extent of the 2025 rupture is important. We have therefore created a new figure (Fig. S1) and added the following statement to the manuscript: “***Eight of the nine models show peak slip at depths of approximately 20–40 km (Fig. S1).***”

Regarding tsunami heights, we have already presented waveform comparisons in the figures with amplitude scales explicitly shown. Because the peak tsunami amplitudes depend on the processing methods (e.g., tidal correction, filtering, and record length) and site conditions, listing or directly comparing maximum values could be misleading. For these reasons, we have chosen not to present a numerical comparison of tsunami heights in the manuscript.

We would like to emphasize that we have carefully considered your suggestion.

In the supplement, you mention that “potency density” and “potency-rate density” are referred to as “slip” and “slip rate” in the main text. Please add this same clarification in the main text.

We thank the reviewer for the helpful comment. This issue has been resolved by moving the detailed description of the PDTI method to the main text.

In Section 2.1, it is really unclear to me where the synthetic waveforms came from that you compare with the velocity seismograms. Are these the Green’s functions you discuss?

We thank the reviewer for the important comment. Our procedure is somewhat specialized and should have been explained in more detail. The synthetic waveforms were obtained by convolving the 0.1 s–sampled Green's functions (for each knot and each tensor component used in the inversion) with the estimated potency-rate density functions, and then summing the convolved waveforms. The resulting 0.1 s–sampled synthetic seismograms were linearly interpolated to 0.05 s sampling before being compared with the observed velocity seismograms. We have added this explanation to the manuscript for clarity.

"***For comparison with the observed waveforms, synthetic seismograms were computed for all nine models by convolving the 0.1 s–sampled Green's functions for each spatiotemporal node and basis tensor component with the estimated potency-rate density tensor functions, summing the resulting waveforms, and linearly interpolating them to 0.05 s sampling.***"

Please elaborate on the concept of dynamic overshoot, as this context is critical for the readers to fully grasp the conclusion of your paper. I have not found a lot of information in the literature about this. You mention that your focal mechanism trends support the hypothesis of dynamic overshoot, but you do not elaborate on why.

We sincerely thank the reviewer for this very important and constructive comment. We agree that the concept of dynamic overshoot required further clarification. Dynamic overshoot refers to a phenomenon in which the dynamic fault slip temporarily releases more shear stress than the static level required for rupture arrest, resulting in a local reversal of shear stress (negative stress change). In our case, focal mechanisms of normal-faulting aftershocks along the shallow portion of the subduction interface indicate that the stress orientation reversed locally immediately after the mainshock, which supports the occurrence of dynamic overshoot.

After reading your comment, we examined the focal mechanisms in more detail and found that several strike-slip earthquakes with nodal planes nearly parallel to the plate interface occurred near the subduction boundary, close to the area where low-angle reverse faulting took place. This observation suggests that the shear stress in the plate-convergence direction may have approached zero in these regions.

We have added two detailed explanations in the Discussion section.

"***Dynamic overshoot refers to a process in which fault slip exceeds the equilibrium stress level during rupture, resulting in a local reversal of shear stress within the large-slip area (e.g., Oglesby et al., 1998; Oglesby et al., 2001; Ide et al., 2011).*** "

"***It is worth noting that a strike-slip earthquake with a nodal plane nearly parallel to the plate interface also occurred southwest of the low-angle normal-faulting event, in the southwestern part of the large-slip area, suggesting that the shear stress in the plate-convergence direction was nearly zero, consistent with a local stress reversal associated with dynamic overshoot.*** "

Does this subduction zone have a defined accretionary prism? In Ide et al. (2011) they mentioned that the compliant accretionary prism amplified shallow slip.

We thank the reviewer for the helpful suggestion. The Kamchatka subduction zone is generally considered to be in a state of tectonic erosion rather than active accretion. To our knowledge, detailed seismic surveys of the trench region using reflection or refraction data have not yet been conducted. Therefore, unlike the Japan Trench, it is difficult to make detailed structural inferences or discussions similar to those presented by Ide et al. (2011).

"Temporal knots" and "spatial knots" are a little confusing. I would suggest replacing "knots" with "nodes" or something similar.

Thank you for your suggestion. We have revised the terminology accordingly and replaced "knots" with "nodes" throughout the manuscript.

Line by line comments

Line 66: "across approaches" can you clarify what types of approaches you are referring to?

We thank the reviewer for the comment. Following the reviewer's suggestion, we have clarified the expression as "*across* ***different inversion*** *approaches* ***(e.g., seismic, geodetic, and tsunami analyses)***" (Modifications are shown in ***bold***.).

Line 71: Since latitude and longitude degrees are not equivalent, could you replace the 30-90 degree epicentral range with distance in km?

We thank the reviewer for the comment. The epicentral distance range of 30°–95° approximately corresponds to 3,335–10,554 km. As the use of degrees is a standard convention in teleseismic analyses, we have retained the degree notation in the manuscript while clarifying the equivalent distance range here.

Line 72: What was your minimum signal-to-noise ratio for records you kept?

We thank the reviewer for the comment. We excluded stations where the P-wave onsets could not be reliably identified. For clarity, we have revised the corresponding sentence in the manuscript as follows:

"*P-wave onsets were* ***manually*** *picked, and stations* ***for which the P-wave onsets could not be reliably identified*** *were excluded.*"

Lines 75-76: Why is the model's temporal spacing 1.1s (as opposed to 1 s?)

We thank the reviewer for the comment. The temporal interval of 1.1 s was chosen to limit the total number of model parameters within a computationally feasible range. For clarity, we have added the following explanation to the manuscript:

"***The temporal node interval was adjusted so that the total number of model parameters remained within a computationally feasible range.***"

Lines 79-80: What is meant by "until the Green's functions retained sufficient amplitude"

We thank the reviewer for the comment. For clarity, we have added the following explanation to the manuscript:

*"**(corresponding to the time at which 95% of the total P-wave Green's function energy was reached)**"*

Lines 103: "**9** models **explain** the observed" à "**nine** models **match** the observed"

We thank the reviewer for the suggestion. We have revised the phrase.

Line 104: Could you elaborate on what you mean by "high-frequency components" and "data points"

We thank the reviewer for the comment. In this sentence, "high-frequency components" refers to the short-period waveform features observed in the 0.05 s–sampled seismograms, which are well reproduced by the synthetic waveforms even though the inversion was performed using 1.1 s–sampled data. It is difficult to define a specific frequency range because the term is used qualitatively to describe the waveform details reproduced beyond the model's temporal resolution. To clarify this, we have slightly revised the sentence in the manuscript.

"*The synthetic waveforms reproduced by the nine models match the observed velocity waveforms well, including* ***short-period features evident in the 0.05 s–sampled data from stations*** *that were not used in the inversion.*"

Line 118: Sentence starting with "Results from both…" I would restructure to be "Results from both the PDTI and BP analyses suggest that two episodes of slip acceleration occurred during EP3 based on the teleseismic P-wave records with the highest temporal resolution."

We thank the reviewer for the suggestion. We have revised the sentence as suggested.

Line 126: Instead of "tend to avoid" I would say something like "are mostly located outside of"

We thank the reviewer for the suggestion. We have revised the sentence as suggested.

Lines 137-138: You mention that previous studies thought the 1952 event involved deeper parts of the fault and that your slip model for the 2025 event showed large shallow slip. How deep did they think the 1952 slip was? According to the USGS finite fault model for the 2025 event, the hypocenter was around 35 km with the largest slip occurring around 15-25 km depth, which still seems a deeper to me.

We thank the reviewer for this thought-provoking comment. According to Johnson and Satake (1999), the large slip during the 1952 earthquake was concentrated at depths of approximately 23–45 km. However, their fault geometry appears to be inconsistent with the depth of the plate interface in this region. After careful consideration, we concluded that discussing absolute depth values here could be misleading, given the large uncertainties in the fault geometry and depth estimation.

"*In fact, Johnson and Satake (1999), based on tsunami waveform inversion, argued that the 1952 rupture* ***produced large slip concentrated near the southeastern coastline of the Kamchatka Peninsula and the northern Kuril Islands, whereas in this study, the large-slip area of the 2025 event is located farther offshore.***"

Line 156-157: Can you clarify how your tsunami simulation results for the 2025 event suggest the same region was the main tsunami source for the 1952 event?

We thank the reviewer for the comment. Our tsunami simulations for the 2025 event demonstrated that the observed tsunami waveforms are best reproduced when the source is located in the large-slip area identified by our PDTI analysis. The digitized tsunami records from 1952 show waveform shapes similar to those of the 2025 event during the initial 60 minutes (Fig. 1f). This similarity in waveform pattern suggests that both tsunamis were likely generated by sources in approximately the same region. To clarify this connection, we have slightly revised the sentence in the manuscript.

"*These findings, **together with the similarity of the initial tsunami waveforms between the two events**, suggest that the same region is likely to be the main tsunami source for the 1952 event as well.*"

Line 184: What would be the expected slip deficit accumulated over 5.8 years?

We thank the reviewer for the comment. The cited case (Fukushima et al., 2018) refers to intraplate earthquakes that occurred within the overriding plate. Therefore, the concept of slip deficit does not apply in this context. We have clarified this by explicitly describing it as an intraplate earthquake in the manuscript.

Line 191: Can you explicitly mention which results suggest that dynamic overshoot occurred?

We thank the reviewer for the comment. We have revised the sentence as follows:

"*Our results, **particularly the occurrence of low-angle normal-faulting aftershocks within the large-slip area**, suggest that dynamic overshoot occurred and that the 2025 Kamchatka earthquake released most of the accumulated strain in the large-slip region.*"

Line 195: Is it really plausible that a 7.4 event 8 years ago could have triggered the 2025 event?

We thank the reviewer for the comment. The sentence refers to the magnitude 7.4 foreshock that occurred about 10 days before the 2025 mainshock, not eight years earlier. To avoid misunderstanding, we have revised the sentence to explicitly describe it as a foreshock occurring 10 days prior to the mainshock.

"*Foreshock activity, such as the magnitude 7.4 event that occurred **about 10 days before and** 40–50 km northeast of the 2025 Kamchatka epicentre (U.S. Geological Survey Earthquake Hazards Program, 2017)*"

Line 210: Is this the first time you mention the shear stress dropping to negative values locally? If so, this should also be discussed early in the paper. Also, how was this determined, and what does it imply?

We appreciate the reviewer's thoughtful comment. We have added a discussion of dynamic overshoot and local stress reversal in the Discussion section.

Figures

Figure 1

Caption: "corner period of **3 hour**" should be "corner period of **three hours**"

Caption: “(a,b) Aftershocks for the **1952 (a) and 2025 (b)**” should be “(a,b) Aftershocks for the **(a) 1952 and (b) 2025**”

Panels (a, b): The yellow of the stars is too similar to the yellow of the circles. I would suggest more visually distinct colors.

Panel c: Please add a color scale bar.

Can you include depth along dip? What is the assumed dip angle for the 1952 event?

Figure 3: Please clarify in the caption this is for the 2025 event.

Figure S1: Font size is too tiny. You could select a subset of the stations if you don’t have enough space after increasing size.

Thank you very much for your careful and constructive review. We have revised the manuscript to reflect the intent of all your comments. In particular, we have added a new figure (Fig. S1) to present detailed information on the depth distribution of the coseimsic slip.

Reply to reviewer #2

Yagi et al have compiled a timely and comprehensive slip-rate evolution estimate of the July 2025 Kamchatka earthquake in Russia. The manuscript is well structured, and well written.

While the authors have provided sufficient analysis results to conclude that the slip during the 2025 earthquake likely released some of the residual slip deficit from the previous 1952 event, I do not believe that sufficient evidence has been included to conclude a near complete strain release, and change in periodicity. I have detailed both my major and minor comments below for the authors' consideration:

We sincerely thank the reviewer for this constructive and thoughtful review.

We recognize that our explanation of dynamic overshoot and the occurrence of normal-faulting earthquakes along the large-slip area may not have been sufficiently clear. As discussed for the 2011 Tohoku-Oki earthquake, low-angle normal faulting along the plate interface suggests the occurrence of overshoot, where the fault slip releases strain excessively and even drives the shear stress into the negative range. Following Reviewer #1's suggestion, we have expanded the discussion on dynamic overshoot so that it can be more easily understood by readers across different disciplines.

We also noticed that part of the methodological description may have caused some misunderstanding. The Potency Density Tensor Inversion (PDTI) does not impose a non-negativity constraint, and therefore is less prone to overestimation than methods that do. In contrast, methods with non-negativity constraints are inherently more likely to produce overestimation, because integrating Gaussian errors with only the positive side retained results in a systematic positive bias compared to integrating a zero-mean Gaussian error symmetrically.

We would also like to express our sincere appreciation for your thoughtful review, especially given that this topic may be somewhat outside your primary field of expertise and the short timeframe for the review. We have addressed your comments as sincerely as possible. We believe that your comments have greatly improved the quality and clarity of our paper. We also sincerely hope that our responses will help clarify our perspective and promote a better understanding of this topic among readers and researchers.

Line 38 - It would be helpful to include the location of Kamchatka Peninsula, as something like - along the eastern coast of Russia - to orient readers unfamiliar with its location.

We have added it as suggested.

Line 47 - Can a reference recurrence period be included for M9 earthquakes, especially if available for Kamchatka Peninsula? Otherwise, it is not possible to ascertain why the 73 years recurrence interval is "anomalously short".

We appreciate the reviewer's suggestion. We consider that defining a reference recurrence period for M9-class earthquakes in Kamchatka is not meaningful at present, because only two historical events (1952 and 2025) have been instrumentally recorded, and the earlier one in 1737 is poorly constrained. The limited number of events and large uncertainties in the timing of past ruptures make it difficult to derive a representative recurrence interval. To address the reviewer's concern, it is more appropriate to describe the

73-year interval as “anomalously short” in a relative sense, compared with the intervals typically inferred for M9-class subduction earthquakes worldwide, rather than to treat it as a deviation from a well-defined local recurrence period.

“*These two events, separated by 73 years of an anomalously short recurrence interval* ***in a relative sense*** *for M9-class earthquakes* ***observed globally (e.g. McCaffrey, 2008)****, provide a rare opportunity to investigate the variability of great earthquake recurrence along a single subduction segment.*” (Modifications are shown in ***bold****.)*

Line 56 - The clause “can be partly attributed to the near-complete release of accumulated strain in the region” is unclear. Do the authors mean that residual strain from the 1952 earthquake was also released in the 2025 event? Can the authors clarify this statement?

We thank the reviewer for the comment. In this sentence, “accumulated strain” refers to the strain accumulated since the 1952 earthquake, including some residual strain that remained unreleased after the 1952 event. The 2025 earthquake appears to have released both the newly accumulated strain since 1952 and a part of the residual strain left from the previous rupture. For clarity, we have revised the text as follows:

“*Our results suggest the occurrence of dynamic overshoot, indicating that the short recurrence interval can be partly attributed to the near-complete release of accumulated strain* ***both before and after the 1952 earthquake.***”

Line 65 - Can the authors add a qualifier to datasets - are they referring to seismic waveforms?

Line 66 - Similar to previous comment, can the authors add some examples of “features” that they are referring to?

We thank the reviewer for the comment. For clarity, we have revised the sentence as follows:

“*Interestingly, teleseismic body-wave analysis, with a formulation that appropriately accounts for Green's function uncertainties, produces detailed coseismic slip distributions well correlated with those from diverse datasets, including* ***near-field strong-motion records, crustal deformation data, and tsunami data****, and is therefore one of the methods that best capture the average features* ***of coseismic slip distribution*** *across* ***different inversion*** *approaches* ***(e.g., seismic, geodetic, and tsunami analyses)*** *(see Fig. 7 in Wong et al. (2024)).*“

Figure 1 - What does “another” refer to in the statement - “For comparison, the epicentre of another mainshock is shown by a grey outlined star.” Do the authors mean “the other mainshock”, implying the 2025 mainshock in (a) and the 1952 mainshock in (b)?

We thank the reviewer for pointing this out and have revised the sentence accordingly.

Line 70 - Can the authors include the reference or url for Seismological Facility for the Advancement of Geoscience (SAGE)?

We have stated this in the Data and Code Availability section.

Line 71 - Can the authors clarify the units of distance as degrees?

In the manuscript, the distance refers to the epicentral distance in degrees.

Line 73 - After subsampling and removal of low SNR waveforms, how many waveforms were left for further analysis?

As stated in the manuscript (Section 2.1), after refining the dataset by removing low-SNR and spatially redundant records, a total of 80 P-wave velocity seismograms were used for the analysis.

To avoid possible misunderstanding, we have revised the corresponding sentence in the manuscript as follows:

"*Vertical-component teleseismic P waves were downloaded from the Seismological Facility for the Advancement of Geoscience (SAGE**; see Data and Code Availability section**), and 80 stations at epicentral distances of 30°–95°* ***(in degrees)*** *were selected* ***and used*** *for analysis.*"

Line 72 - Were the P-wave onsets picked by manually observing each of the 80 station waveforms?

That's correct. We have clarified it by stating that the P-wave onsets were manually picked.

Line 72 - What was the cutoff threshold for signal-to-noise ratio?

We excluded stations where the P-wave onsets could not be identified.

For clarity, we have revised the corresponding sentence in the manuscript as follows:

"*P-wave onsets were* ***manually*** *picked, and stations* ***for which the P-wave onsets could not be reliably identified*** *were excluded.*"

Line 73 - What was the criteria for spatial subsampling, e.g., how was it determined how many samples to keep in the dataset?

We thank the reviewer for the comment. In this study, spatial subsampling was performed qualitatively rather than based on a strict numerical criterion. We empirically evaluated the data quality at each station and selected representative observations by considering both waveform reliability and spatial distribution, ensuring that each region was adequately represented without excessive clustering. To the best of our knowledge, no previous study has used as many as 80 P-wave records in inversion modeling this event. We really attempted to utilize as many high-quality P-wave seismograms as possible in our analysis. We have revised the corresponding part of the manuscript as follows:

"*To minimize bias from uneven station density,* ***we adopted an empirical subsampling approach, preferentially retaining stations with high signal clarity*** *in densely covered regions such as California.*"

Paragraph 111 - In the supplementary material describing PDTI, the authors have highlighted that "Because non-negativity constraints are unadopted, PDTI may yield a slip estimate that exceeds the final slip value due to estimation errors during the rupture process." How have the authors addressed this overestimation in their methods? Could this indicate that their slip range estimates of 9.6-13.6m are over-estimates? This limitation of PDTI is highly relevant to the conclusions of this study. As a result, I would suggest that the authors include this limitation in the main text, and address why they believe that their results are not over-estimates.

We thank the reviewer for the comment. It seems that the intent of the original sentence was not clearly conveyed. As we mentioned in our general response, the non-negativity constraint actually tends to suppress overestimation, rather than cause it. To avoid misunderstanding, we have revised the relevant sentence in the Supplementary Material as follows.

"Because non-negativity constraints are unadopted, PDTI ***allows both positive and negative deviations of slip during the rupture process. As a result, the slip integrated over a certain time window may temporarily exceed the final slip value (e.g.*** *Fig. 3c)."*

Line 113 - Since there are only 9 models, can the authors add more specificity to the number of models in agreement, instead of "most models"?

We thank the reviewer for the comment. We have revised the sentence.

"*The cumulative slip distribution reveals that slip exceeding 6 m is observed in an area of 300 km × 160 km centred around 290 km southwest from the hypocentre in all models, while slip exceeding 9 m is observed,* ***at least partially, wit****hin an area of 175 km × 110 km centred around 325 km southwest in* ***all*** *models (Fig. 3a,b).*"

Figure 3 - There is one large star and three smaller stars in (a) and (b). The caption only describes "The star". Can the authors update the caption to describe the large and the 3 smaller stars?

We thank the reviewer for pointing this out and have revised the sentence accordingly.

Line 131 - Can the authors provide a description for the terms - dynamic overshoot and dynamic stress disturbances? This would make it easier for readers to understand how their observations lead to their conclusion.

We thank the reviewer for the comment. We have expanded the discussion of dynamic overshoot following Reviewer #1's suggestion, also added an explanation of dynamic stress disturbances, clarifying that they are caused by seismic wave propagation.

Section 3.2 - The heading of this section "Repeating M9-class earthquake" does not appear to convey the content of the section. Suggest improving the heading to be more relevant to the text, such as "Tsunami observations from the 1952 and 2025 earthquakes".

We thank the reviewer for the helpful suggestion. However, we believe that the current heading, "Repeating M9-class earthquake," appropriately reflects the broader focus of this section. In this section, we discuss not only tsunami observations but also the co-seismic slip distribution of the two M9-class events that ruptured nearly the same segment along the Kamchatka subduction zone. The tsunami data are presented as one line of evidence supporting the repeating nature of these M9-class earthquakes, rather than as the main topic itself. For this reason, we have retained the original heading.

Line 161 - Can the authors add more information about how the maximum slip deficit was determined to be

6m, or provide a relevant reference?

We thank the reviewer for the comment. The method for estimating the maximum slip deficit (~6 m) is already described in the Introduction, where it is explained as being derived from the plate convergence rate and the interseismic time interval. For clarity, we have added a brief explanation here as well.

*"This raises the issue of the slip budget, because these two earthquakes occurred only 73 years apart and the slip deficit accumulated on the plate interface is at most about 6 m**, based on the plate convergence rate and the interseismic period.**"*

Line 171 - "Instead" is unnecessary, since the previous statement supports the following statement.

We have deleted it as suggested.

Line 179 - Can the authors include locations of the Nankai Trough and Parkfield, e.g., Japan and California, US?

We have added it as suggested.

Line 184 - Can the authors include the fault name and region for their example?

We thank the reviewer for the comment. The fault name was not specified in the original reference, so we have added the regional name in the revised manuscript to clarify the location.

Paragraph 174 - In my opinion, the authors have not provided sufficient evidence to support the claims in this paragraph, and especially their statement that "These findings present an antithesis to conventional periodic and quasi-periodic occurrences of large earthquakes". The recurrence intervals for earthquakes are "average recurrence intervals". In other words, over sufficiently long periods of time, e.g., 10,000 years, it is expected that 50 earthquakes would occur if their average recurrence interval is 200 years. This recurrence interval does not necessitate that consecutive earthquakes may not occur at an interval much smaller or much larger than 200 years. While the short recurrence interval of the Kamchatka earthquake is sure to have significant consequences to earthquake risk, e.g., the impacts to people, and public policy, it does not by itself support a change in the periodicity of large earthquakes in the region.

We appreciate the reviewer's suggestion. However, we argue that the conventional view of "periodic (or quasi-periodic)" occurrence of large earthquakes should be reconsidered. Our discussion does not refer only to the "average" recurrence interval; rather, we emphasize that dynamic processes such as overshoot or partial strain release can cause significant temporal variability in recurrence, beyond what is implied by a simple periodic model (e.g. Shimazaki and Nakata, 1980). For clarity, we have revised the text and added a reference to Shimazaki and Nakata (1980), which discusses deviations from ideal periodicity in earthquake recurrence.

"*These findings* ***suggest that the*** *conventional* ***view of*** *periodic or quasi-periodic occurrence of large earthquakes* ***(e.g. Shimazaki and Nakata, 1980) should be reconsidered.***"

Line 191 - While the authors have provided evidence that the slip in the 2025 event must have released some of the residual deficit from the 1952 event, it is not clear how they have concluded that the 2025 event released most of the accumulated strain. Isn’t it possible that there is additional accumulated strain from the 1737 and 1952 events that remains unreleased?; Line 209 - The authors have not included results in the text to indicate negative stress values. As a result, this statement, including the clause - almost complete release, cannot be validated.; Line 211 - Due to support lacking for almost complete release of accumulated strain, the conclusion that the next M9 event should occur on a scale longer than 73 years is not well supported.

We appreciate the reviewer’s suggestion. However, we believe there might be a misunderstanding regarding the evidence for “dynamic overshoot”. As shown in Fig. 3 and Fig. 4 of the manuscript, low-angle normal-faulting aftershocks occurred immediately after the mainshock within the main large-slip area along the plate interface. These events correspond to reverse-polarity faulting relative to the megathrust motion, implying that shear stress locally dropped below zero after the main rupture. This is a well-recognized indicator of dynamic overshoot, as discussed for the 2011 Tohoku-Oki earthquake (Ide et al., 2011). Therefore, we consider that our data provide clear observational evidence for stress reversal (i.e., overshoot) and near-complete strain release within the large-slip region. We have revised the relevant text in the Discussion section following Reviewer #1’s suggestions.

Line 214 - Can the authors include references for the stress release in Tohoku, Sumatra and Maule earthquakes?

We thank the reviewer for the helpful comment. We carefully checked the literature but could not find explicit studies. To reflect this, we have slightly revised the sentence.

“*Notably, such complete stress release is uncommon even among M9-class earthquakes;* ***to the best of our knowledge****, it* ***has been*** *reported for the 2011 Tohoku-Oki earthquake, but not for the 2004 Sumatra and the 2010 Maule earthquakes.*”