

REVIEW ROUND 1

We want to thank the editor and 2 reviewers for their comments. Below, we respond to each of the comments and how we treated them individually.

We revised the paper substantially, and changed our conclusions after finding some unit errors in our calculations that we found when treating the comments made by the reviewers, and because of including additional DART data requested by the reviewers. While we include a track changes document, as requested, the changes are numerous, so it might not be of much use.

Having corrected the errors in our calculations, included additional data, and repeated the unit source analysis, we now find that:

- 1) The Sand Point event itself was on a single strike slip fault remains (same as before)*
- 2) This main shock creates positive stress change on a narrow part of the shallow megathrust (same as before)*
- 3) Unit source based tsunami modeling searching for the best tsunami source was expanded to include shallower parts of the megathrust and suggests several reasonable sources, the best fitting one falls into the region of positive Coulomb stress change from the main shock. (revised)*
- 4) The unit source requires that shallower megathrust slip (~1.7-6.8 m of slip between 2.5-10 km depth) to be released ~ 8 minutes after the mainshock (new)*
- 5) We find no seismic and geodetic evidence for megathrust slip / earthquake (same as before)*
- 6) such a “tsunami earthquake” like event, we suggest that the megathrust slip was released as very slow and shallow slip (new)*
- 7) Modeling of the megathrust slip predicts seismic phases (e.g., second Rayleigh wave) that are not observed. These disappear for slow ruptures between 50-150 s, common to many tsunami earthquakes. (new)*
- 8) Small predicted geodetic deformation can disappear if the slip is more concentrated in the updip region of the unit source model (new).*
- 9) Early aftershocks are missing in the region of predicted megathrust slip (new)*
- 10) This all leads us to suggest that the Sand Point earthquake triggered delayed and very slow and focussed megathrust slip that evolved similar to a classic tsunami earthquake. (new)*

This continues to distinguish our work from prior work by Bai et al, who suggest a complex 3 fault geometry, none on the megathrust; and Santallanes et al (2025), who suggest co-seismic deeper megathrust slip, based on joint inversion of geodetic, seismic and tsunami data and published since our submission came back for revision. Our work presents an alternative model that has been tested in all ways feasible given the observations (in some places using methods of exclusion), and shows that shallow slow megathrust slip can be triggered within minutes after the mainshock.

Overall, we find our paper has improved significantly, the conclusions are robust. We hope you will agree and look forward to your responses.

The comments are in black, normal font. Our responses are in red italics.

In addition to the pertinent arguments put forward by the 2 reviewers, I think that the paper could be significantly improved in its form by improving the quality of the figures. In particular, many of the points discussed are presented in different figures, which makes it difficult to see the spatio-temporal links that are being discussed. There is a discussion in the paper about the aftershocks locations with respect to Coulomb Stress changes, and also possible link with shallow slow slip. This is an essential point for the tsunami generation, however the data/models are shown on different figures which makes it difficult to understand and relate. To make the discussion clearer, it would be interesting to show together the aftershocks (all of them) with Coulomb stress change, and also the location of shallow slip source which is put forward as causing the strong delayed tsunami signal: how does this source location compare with the earthquake slip distribution, aftershocks locations. I think the relations between these processes, need to be further shown (in figures) and discussed (if relevant).

While our interpretation changed with new modeling, we've updated the figures and included more information for better cross-referencing.

Other comments:

Why using only two DART buoys data (46402 and 46409): the map on Figure 2 shows other stations, in particular 464014: are there no data available at these other sites ?

We added buoy 464014, esp. for the determination of the unit sources, which changed the potential slip model for that analysis some. Including other DART buoys in that inversion / search does not make much sense because of the low signal not noise ratio. Figures and related text have been updated accordingly.

On Figure 2:

- some inset time series do not have vertical (amplitude) or horizontal (time) axis label and units. It is not clear either why sometimes only the east (magenta) is shown.

(Now included in Figure 1) We updated the caption for this figure to make our design choices clearer. In making the figure it was clear that having displacement and time labels for all GNSS traces resulted in obscuring much of the other data and making the figure unnecessarily busy. The magenta-only traces are shown to demonstrate the surface wave in GNSS, which has been previously interpreted as S wave in PGD analyses, suggesting much larger megathrust slip.

(Note that all new figure numbers are decremented by 1)

On figures 4, 7, 8, 11: the maps do not have consistent lat-long limits, and are difficult to compare. Adding some consistent tectonic features from Fig. 1 would significantly help to the comparison and Figure comparison: in particular, adding the location of the Trench, and the recent event slip contours and/or CMT locations.

We added features and labels.

Figure 10: zoom on the region of interest, because at this scale is it difficult to see the location of the earthquakes with respect to the Coulomb stress change. You could add the aftershocks (From Fig. 3A) on this plot.

We've done all this.

Fig. 11: where is the slip model and where are the aftershocks located with respect to this proposed source ?

Figure 9 shows all that information now.

Reviewer A:

This paper explores the potential drivers of the larger-than-expected tsunami associated with the 2020 Mw7.6 Sand Point Alaska earthquake. This event was part of an intriguing sequence of earthquakes in the vicinity of the 'Shumagin Gap'. What sets the Sand Point event apart is that it was a dominantly strike-slip event, inferred to have occurred within the subducting Pacific plate. It was then surprising that such an event (with minimal vertical motions on the fault plane) could generate a tsunami larger than the somewhat larger (Mw7.8 Simeonof) thrust faulting event. In this paper the authors systematically explore the various potential aspects of the event that could lead to the larger tsunami. They undertake additional seismicity studies and geodetic analyses (GPS and InSAR) to explore possible displacements that could generate the tsunami.

This very detailed analysis of additional data are the strong point of the paper. The data sets they have generated are potentially of use to a wide range of other studies and it would be great to see them made available to the community. Unfortunately, as they discuss, none of the "easy" explanations for the tsunami seem to work. Indications are that the earthquake occurred on a fairly steeply dipping plane, minimizing earthquake driven seafloor displacements as the main cause. Similar to what others before them have found, the earthquake mechanism as defined by the seismological data does a good job of matching geodetic and other seismological data. Similarly the stress change modeling here is basically comparable to other previously published results.

They then conclude the paper with a more speculative part that ascribes the tsunami possibly to additional 'after slip' or triggered slip on the interface that for un-defined reasons is not seen seismically or geodetically (or at least that is how I read their discussion). I have no problem with that as a speculation in the discussion - basically they are saying "we can't explain what happened, so we suggest there could be other 'hard to see' events that occurred and made the tsunami..." But it is not shown.

This gets me to the heart of my concern for this paper. The title of the paper is declarative and says they do have a solution for the tsunami generation and it was late megathrust slip. They suggest this in their discussion, but do not analyze these smaller events in the same detail as the rest of the seismicity. To that end the title is not appropriate - even switching "Could explain" for "explains" in the title would help. However as 80+% of the paper is a detailed analysis of the other events etc.,

and they never really address the substantial misfits between their 2-fault model and observations, I really think the title and paper set-up are very misleading. I think that to be a good representation of the actual work conducted, the direction and tone of the paper needs to change. At present I see it attempting to hard to sell something that isn't demonstrated (one could use stronger terms for this!).

Although maybe not a catchy, I think this paper needs to be re-directed and focus on the detailed issues with all models of tsunami generation for this event - including the late triggered megathrust slip the authors prefer.

Recommendation: Resubmit for Review

We thank the reviewer for their assessment of the paper. The actionable item we take from their review is to change the title to include "could," which we have not done based on our new findings, which we consider substantially more robust to justify this claim.

As this review will be published alongside the paper, we do want to address in multiple instances the reviewer makes vague suggestions that our work and conclusions are misleading (perhaps even implying that we are intentionally misleading), but does not provide any support for these claims (other than the title). Seismica guidance for reviewers states:

"Clearly explain and support your judgements so that editors and authors may understand the basis of your comments and provide reference to additional published works, where appropriate."

Some examples:

"[...] and they never really address the substantial misfits between their 2-fault model and observations, I really think the title and paper set-up are very misleading."

(we had done this in L410-418 of the original submission, and we reject the 2 fault model for its misfit especially to the InSAR data, but also discuss other fits. The reviewer does not provide any specifics on what else we should address)

or:

"I think that to be a good representation of the actual work conducted, the direction and tone of the paper needs to change. At present I see it attempting to hard to sell something that isn't demonstrated (one could use stronger terms for this!)."

The reviewer has not provided specific examples of what we should change (other than the title). We did consider the analysis that we performed very comprehensive: multiple finite fault inversions to test different geometry hypotheses, long-period seismicity analysis to test landslide or slow earthquake hypotheses, earthquake relocations, grid search & tsunami modeling, stress change modeling, and exclusions of other reasonable sources. We'd gladly consider productive guidance on other angles that we may have not yet explored, but the reviewer does not provide this. If there is constructive guidance,

please provide specifics. Our conclusions were clearly presented as inference from observations, a hypothesis that invites future testing if there were additional data becoming available.

We have, however, productively reviewed our models, found and fixed our mistakes as laid out above, and adapted our interpretations accordingly.

Other Specific Responses to Reviewer A:

They then conclude the paper with a more speculative part that ascribes the tsunami possibly to additional 'after slip' or triggered slip on the interface that for un-defined reasons is not seen seismically or geodetically (or at least that is how I read their discussion).

It is unclear to us what the reviewer means with "un-defined reasons." Neither seismic nor geodetic data resolve a distinct secondary signal that could be attributed to additional slip release, but tsunami data cannot otherwise be fit with reasonable model assumptions. A model that includes megathrust slip resolves slip in the approximate region of the unit sources, but it ends up being too early. This is what prior work and ours has been struggling with to explain the tsunami and results in the model Bai et al. developed to release large slip slowly on a fault oriented in a way not to create a seismic or GNSS signal (and meanwhile the Santallanes et al., 2025 paper that also suggests megathrust slip, albeit much deeper and differently timed than ours). Our work shows that megathrust slip included in the co-seismic inversion can worsen the InSAR fit, and simply trades off in location with slip maximum on the strike slip fault. As there seems to be no instrumental evidence for the process that created the tsunami, we need to search for processes that remain undetectable. This was alluded to in, for instance, lines 505-506.

We followed a process of exclusion and arrived at the current conclusions. Our work is reproducible and, if additional evidence comes to light, can be refuted or revised, as is the nature of science.

I have no problem with that as a speculation in the discussion - basically they are saying "we can't explain what happened, so we suggest there could be other 'hard to see' events that occurred and made the tsunami..." But it is not shown.

That is correct - it was inferred based on method of exclusion, and we "suggest" because we cannot "show" as we described in the paper. We show more now, but we're not showing everything. Inverse problems are famously non-unique, showing (which seems to be implied as proving here) remains impossible, so we do our best to suggest reasonable theory.

This gets me to the heart of my concern for this paper. The title of the paper is declarative and says they do have a solution for the tsunami generation and it was late megathrust slip. They suggest this in their discussion, but do not analyze these smaller events in the same detail as the rest of the seismicity.

Our conclusions have changed (see responses above), and we analyze our hypothesized slip in as great a detail as possible. We hope the reviewer concurs, otherwise we look forward to detailed suggestions as to what we should attempt in addition.

To that end the title is not appropriate - even switching "Could explain" for "explains" in the title would help. However as 80+% of the paper is a detailed analysis of the other events etc., and they never really address the substantial misfits between their 2-fault model and observations, I really think the title and paper set-up are very misleading.

We hope the reviewer agrees that the revised paper addresses this concern.

Although maybe not a catchy, I think this paper needs to be re-directed and focus on the detailed issues with all models of tsunami generation for this event - including the late triggered megathrust slip the authors prefer.

We hope the reviewer agrees that the revised paper addresses this concern.

Recommendation: Resubmit for Review

Reviewer B:

Grapenthin et al. provide a detailed analysis of the 2020 Mw7.6 Sand Point strike-slip earthquake. One of the most puzzling points of this event is the causal relationship between the strike-slip mechanism and the large tsunami, as strike-slip events usually do not generate large tsunamis. I think this manuscript makes a big contribution to examining this causality. First, the authors try to model fault slip and then examine whether the modelled buoy data fit the observations. The authors then start from the buoy data, invert the slip on the megathrust, and examine the possibility of fault slip on the megathrust.

I really enjoyed reading this manuscript. I appreciate the efforts the authors have made to thoughtfully examine the relationship between the strike-slip event and the tsunamis, by using the most complete geophysical data sets available for this event.

We thank the reviewer for their evaluation of the paper and constructive feedback.

Minor comments:

Line 43: neither reproduces -> neither model reproduces

Changed.

Line 146: with linear trend -> with a linear trend

Changed.

Line 385: between 2 to 15 -> between 2 and 15

Changed.

Figure 5: add tick labels to the colorbars in (d). They are covered by InSAR phases.

(now figure 4) Fixed.

Recommendation: Revisions Required

REVIEW ROUND 2

We thank the editor and the reviewer for their comments. Below, we respond to each of the comments and how we treated them individually in red italics. The manuscript has gained clarity and focus, and all components are more integrated now. We hope these changes will make the paper suitable for publication in Seismica.

*For all authors,
Ronni Grapenthin*

Reviewer C:

Grapenthin et al. reassess the source parameters of the 2020 Sand Point earthquake that ruptured the subducting Pacific Plate in the Aleutian subduction zone. Specifically, they attempt to understand the enigmatically large tsunami that cannot be explained by oblique strike-slip faulting in the mainshock. They conclude that strike-slip faulting plus megathrust slip that fit available geodetic and teleseismic observations cannot produce the observed tsunami. From this point, the manuscript pivots significantly to a very different type of analysis, systematically searching for megathrust sources that could account for the observed tsunami. They argue that shallow megathrust slip, delayed 100s of seconds from the mainshock and producing no surface displacement signal, was responsible for the observed tsunami.

I think the analyses are generally sound, with some specific comments detailed below. However, I will reiterate a comment I have made about other similar papers invoking thrust faulting to explain the tsunami: this “invisible” slip had to have occurred rapidly enough to generate a tsunami, yet there is no other evidence for it in the way of aftershocks (or seismic/geodetic data). In contrast, most other tsunami earthquakes I am aware of (including in the Aleutian subduction zone) have generated substantial thrust faulting aftershock activity. To use the authors’ own language on p. 3: the chief problem with the hypothesis of megathrust slip is a current lack of evidence.

We certainly agree that it would be more satisfying to point to the piece of data that irrefutably shows what happened, we can’t for this earthquake. It’s not for a lack of trying. We hold that our investigation into this earthquake and tsunami is the most complete attempted so far (both in data used and methods applied). To lay out our workflow:

- 1) try inversions first (first simple strike-slip, then with megathrust) neither fit the tsunami data,*
- 2) check on the submarine landslide - cannot find anything in the data*
- 3) Try the shallow megathrust unit slip models to find a location of slip that would explain the tsunami*

- 4) *Compare that to the stress change predicted by the strike-slip-only model*
- 5) *Unit source slip location and Coulomb stress increase fall in the same location, down-dip end is bounded by two M5.7 earthquakes in the region of large Coulomb stress change gradient.*
- 6) *Why cannot we see this large slip in the seismic record?*
 - a) *Experiment with rise time of the tsunami model*
 - b) *Experiment with slow rupture and synthetic waveform modeling*
- 7) *6a and b demonstrate that a slow rupture could occur hidden from the data record.*

We show that our model explains the data and is physically plausible. We hope that near trench ocean bottom observations will soon fill this unsatisfying observational gap.

My other critique of this paper as written is that it seems like two separate studies loosely glued together. One is the series of fault slip inversions based on geodetic and seismic observations. The other is the systematic search through megathrust sources that could account for the tsunami. I think these could be better integrated.

We have modified the last paragraph of the introduction to better lay out the journey this paper is taking. We also integrated some sections of text from the discussion into the results section, organizing them in a more logical way to follow the train of thought laid out in our response above. Further, we moved some of the landslide text from the supplement into the main text. We hope the reorganization of text (with only minor rephrasing to allow for flow of the text) makes this more obvious.

Line numbers were not included on the manuscript I received, so in my comments below I indicate the corresponding page number.

We apologize for omitting the line numbers in the submission. They are included in the revision.

Major Comments

p. 3: To reiterate my point above, the issue with invoking megathrust slip is identical to the one brought up for landslides; there is no other evidence for its occurrence. I have seen nothing convincing in the mainshock observations or post-earthquake seismicity that requires megathrust slip.

See our comment above. As opposed to prior work, in our paper we are trying to tackle this issue as comprehensively as possible. Since both the tsunami model and the Coulomb stress change point to the shallow megathrust location, and we cannot find evidence for a submarine

landslide, this model is the most plausible to us, especially since the lack of seismic phases is also explainable through the required slow rupture.

p. 4: I do not agree with the authors' conclusion that land-based observations are insensitive to megathrust slip close to the trench. I am aware of several $M_w \sim 7$ -7.5 shallow megathrust earthquakes that have been constrained by teleseismic data (and high-rate GNSS). Perhaps this is instead an indication that megathrust slip is not the solution to the tsunami.

This is a fair point, our phrasing was imprecise. Certainly, there are circumstances where shallow megathrust slip can be resolved (there is a question of whether it is resolved close to the trench because it really is there, or because of one-sided observations / methodological constraints). We changed this to refer specifically to this event.

p. 12: It seems to me that stress changes should just be directly calculated on the plate interface. I would think that calculating stress changes in a volume, then interpolating them onto a surface involves unnecessary extra calculations and potentially could lead to errors in areas of high stress change gradients.

We have clarified this in the revised manuscript now. The stress changes are indeed calculated with respect to a receiver fault, which is the planar approximation of Slab2 in the Sandpoint-Simeonof-Chignik area. The depth slices and slice-resolution are the computational limits of the software we use. Stress-changes on each slice represent the values calculated for the approximate Slab2 geometry in that section. The receiver fault cutting through the slices will have the same values at 1-km-depth intervals. The interpolations are made to match the Slab2 gridding which is smaller in value than the 1-km-depth gridding obtained from coulomb, and for the non-planar parts of Slab2 away from the Sandpoint-Simeonof-Chignik region and the deeper segments.

p. 14: I think a little more explanation is required for the difference between neglecting rupture propagation timing (assumption of instantaneous seafloor displacements) and varying the rupture offset. I would guess that this just changes the arrival time of the same tsunami waveform, but some clarity here would be welcome. Also, how fast does the earthquake need to occur for the assumption of instantaneous displacement to be valid? I am not a tsunami modeler, but this would affect my interpretations of what subsequent deformation (aftershocks, etc.) I might expect to see.

We thank the reviewer for this comment. A uniform rupture-time offset primarily shifts the tsunami waveform in time, whereas finite rupture propagation assigns different initiation times to different parts of the source and can therefore also affect waveform phase, amplitude, and directivity.

The assumption of instantaneous seafloor displacement is appropriate when the characteristic rupture time is much shorter than the characteristic tsunami propagation time. In our study area, on the continental shelf near Sand Point, water depths of 0.1-0.2 km correspond to tsunami speeds, $\sqrt{g \cdot \text{Depth}}$, of about 0.03-0.04 km/s. For a ~100 km source, a rupture propagating at 1.5-3 km/s would take about 30-70 s, during which the tsunami would travel only about 1-3 km. Thus, the tsunami propagates only a small fraction of the source-region dimension while the rupture is occurring, and the water column responds primarily to the final coseismic deformation rather than to the detailed temporal evolution of the rupture.

For the thrust-fault scenario, the source region is considerably smaller and is represented by only two or three subfaults. Therefore, the effect of including finite rupture propagation is expected to be negligible as well.

Fig. 3: The authors state that the Sand Point earthquake ruptured within the subducting slab, as other authors have argued. However, a substantial fraction of their moment release comes from above the plate interface, based on my sketch on the slip distribution in Fig. 3. I wonder if constraining the slip to be deeper would have an impact on the rest of their results.

We don't agree that "a substantial fraction" of the moment release is above the plate interface. The bulk is certainly below (see Figure 3). Given that most of the observations are relatively far away from the shallow slip, we believe the fit to the data will not substantially change. Figure 6 provides evidence for this, allowing slip to be placed directly on the interface, which reduces the amount of shallow slip - effectively doing what the reviewer suggests, with relatively minor impact on the fit (see discussion).

p. 24: Up to this point, I still do not clearly see the purpose of the stress change calculation. By the end of the manuscript, it becomes apparent why the calculation is relevant to the tsunami generation, but I wish this were clearer up front. Part of it might be fixed by addressing my general comment about the manuscript reading like two separate studies.

We hope the reorganization helps to show that this is an integral part of the paper.

p. 26: The locations of the Sand Point aftershocks in the first few 2-3 months that I have seen indicate very little upper-plate activity. I am not doubting the occurrence of upper-plate aftershocks, but I strongly doubt that the causative fault of the Sand Point event crossed the megathrust.

As shown in Fig. 2d, more than 1000 earthquakes occurred in the overriding plate within the first 100 days, in contrast to almost 0 events before the Sand Point M7.6 event. A separate study using machine learning techniques to detect aftershocks (Jie et al., 2026, SRL) also confirms this temporal change. Therefore, we believe the numerous events in the overriding plate were triggered by the Sand Point M7.6 main shock, but we do not claim that these happened on the same fault or fault system, but more likely on different faults.

Detailed Comments

p. 2: I might add that the Simeonof earthquake ruptured to the west and the Chignik earthquake ruptured to the east. Fig. 1 shows the geographic relationships between events well, so not a lot of text is needed to make this point.

We added the geographic relations, but, actually, both Simeonof (hypocenter and bulk of the slip) and Chignik ruptured to the east of the Sand Point earthquake. It's the 2023 and 2025 events that ruptured to the west.

p. 4: Somewhere in the Introduction it would be good to describe what the mechanisms of the Sand Point aftershocks were. The authors spend some time indicating their locations and interpreted fault planes, but there is information in the distribution of aftershock mechanisms from pure strike-slip to normal faulting (no thrust faulting that I can recall, though).

We added one sentence:

"Most of the large ($M_w \geq 4.8$) aftershocks of these three major events share similar focal mechanisms with the main shocks, with two exceptions: an $M_w 5.4$ event ruptured a vertical or horizontal fault and an $M_w 4.9$ normal-fault earthquake occurred nearly 14 and 32 hours after the 2020 $M_w 7.8$ main shock, respectively."

p. 4: I know that these abbreviations are commonly used, but I think it would be appropriate to spell out "InSAR" and "sps" (if not "GNSS" as well) at their first use. "seismic" can be removed after "teleseismic". I would remove "preferred" and just call the first model "strike-slip-only".

Done.

Fig. 1: Please label "Shumagin" and "Semidi" segments. I have been repeatedly told by reviewers to use the Tape and Lomax (2022) aftershock zones for historical events, so I will pass that advice on here. I would label the axes for all the high-rate GNSS time series.

The segments have been labeled. We do not label all the axes for the high rate GNSS (we've tried), because the figure ends up very cluttered. We chose not to use the aftershock regions

from Tape and Lomax. As the authors of that paper stated in the title of a subheading in the discussion section: “An Aftershock Region Is Not the Same as the Coseismic Rupture Area”. Our goal in Figure 1 was to provide representations of coseismic rupture areas for past earthquakes. For the Simeonof, Chignik, and 1938 earthquakes, we used coseismic slip models (Xiao et al., 2021, Elliott et al., 2022, and Freymueller et al., 2021, respectively). While the 1964 outline is based on aftershocks, we feel it provides a better overall fit to the coseismic models for that event (e.g. Suito and Freymueller, 2009) than the Tape and Lomax aftershock zone.

p. 6: How long are the daily GNSS time series data gaps?

There are only two stations with notable data gaps, AC21 and AC41. Both of these resulted from telemetry failures, with some data lost as the time between communications failure and manual download was too long. We rephrased the text because the fact that there is an ongoing postseismic relaxation is more important than the gaps, but we do highlight the two gaps.

p. 7: Please spell out “SNR” at first use.

Done.

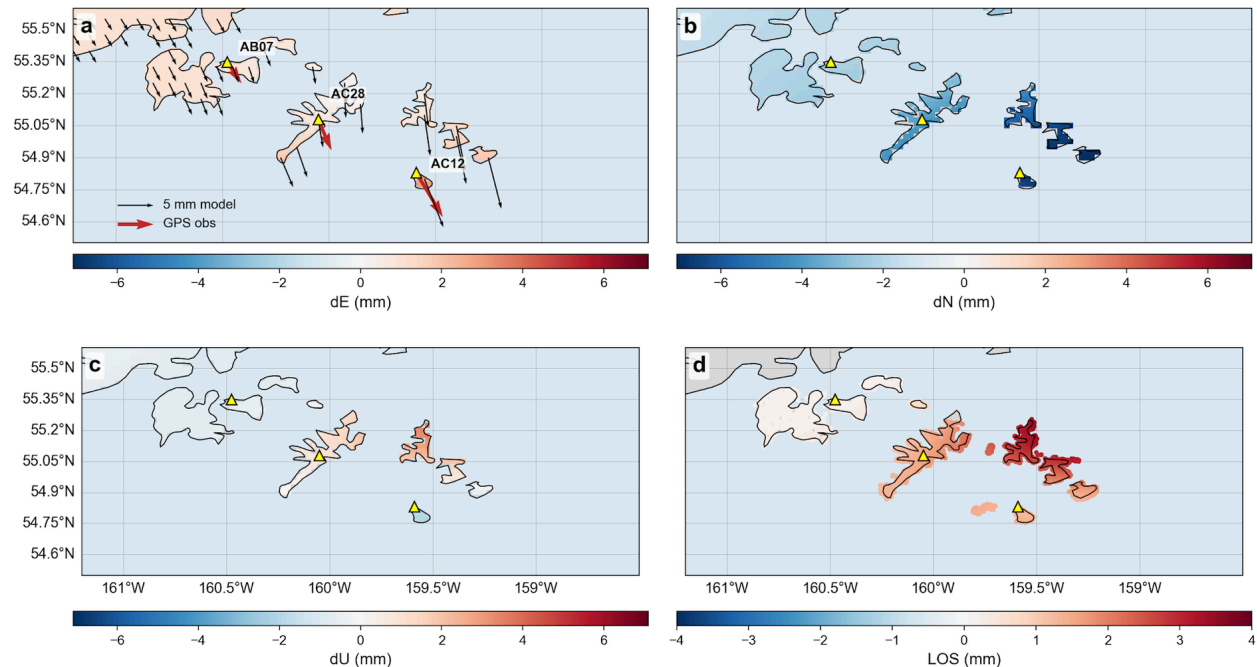
p. 8: Did the authors consider using the near-zero ascending displacement across the islands as a constraint in their inversion?

We did not use the ascending InSAR data in the inversion because no stable interferogram could be obtained from the available ascending SAR acquisitions. In addition, to avoid introducing postseismic deformation into the coseismic inversion, we did not use longer temporal baseline interferograms. We have provided further explanations regarding this issue in Lines 202–206 of the revised manuscript.

How much postseismic would be expected in more than 24 days?

At GPS site AB13, the closest site to the Sand Point event, the total displacement from 2020-10-04 until the time of the Sand Point earthquake was only ~1-2 mm, close to the noise level.

Separately from this work, we have been developing a postseismic deformation model for the Simeonof earthquake which is currently being prepared for submission. The figure below shows the three components on a grid over the islands (a–c), with model vs. observed GPS vectors at the three island continuous sites, plus the line-of-sight projection (d). The displacement is largest at the eastern islands (~5 mm), small near Sand Point (~2 mm). This is close to the noise level for InSAR, too.



Is a “uniform sampling algorithm” simply sampling the displacements on a grid? What is the spacing between InSAR displacement observations after sampling?

The “uniform sampling algorithm” refers to selecting unwrapped InSAR observations at a predefined spatial interval rather than simply resampling the data onto a regular grid. Due to spatial variations in the signal-to-noise ratio of InSAR fringes, the final observation points are not strictly grid-distributed. After sampling, the spacing between neighboring InSAR observations is generally less than 1 km, ensuring adequate coverage of the coseismic deformation field.

p. 8: Is the low end of the filter (0.001 Hz) relevant for suppressing high-frequency noise, as the sentence suggests?

We thank the reviewer for pointing out this potential ambiguity. The frequency range of 0.001–5 Hz represents the band-pass filter applied to the raw waveforms. The high-frequency cutoff (5 Hz), rather than the low-frequency cutoff (0.001 Hz), is used to suppress high-frequency noise. To avoid potential confusion, the original sentence has been revised as follows: “Raw waveforms are band-filtered between 0.001 and 5 Hz to retain the frequency range of interest and suppress high-frequency noise above 5 Hz.”

p. 9: Please include the window for the moving average filter so I do not have to calculate it from what periods are removed.

*We have revised the sentence as follows: A **3-hour** moving average filter was applied to DART buoy records to remove low-frequency variations associated with tides and isolate the short-period tsunami signal.*

p. 10: I would remove “optimal” since debate remains about details of the Sand Point faulting geometry.

Removed.

Just to be clear, the “misfits” are the values determined after the distributed slip inversion? The inversion setup section seems like a strange place to talk about those. What is a “relocation geometry”? (As the authors might be able to tell, I find this section convoluted.)

This section is reworded: “To determine the strike-slip fault geometry, we tested three different 3D fault geometry parameterizations (USGS/GCMT, Jiang et al. (2022), and our own relocation) with the full dataset described above. Slip inversions with these geometries yield the respective misfits presented in Table 1.”

p. 12: How are the Sand Point earthquake fault parameters associated with Slab2? I did not see that explicitly laid out prior to this section.

The wording in the original manuscript was confusing and therefore, this has been written. The revised line (now in section 3.3) explains that it is reasonable that the stress changes due to the Sand Point event calculated for a Chignik-like receiver fault are interpolated over the Slab2 interface because the Chignik fault is a flat approximation of Slab2 in the study region.

p. 15: Do I understand correctly that the hypocenter is a parameter in the inversion? And why would this be similar to the GCMT depth, especially considering this appears to be a nearly unilateral rupture? Also, please add “moment” before “release”.

Yes, the hypocenter is a parameter in the inversion. We note the similarity to the GCMT depth to point out that our depth solution is not far from other published depths. Since we talk about the modeled slip in Figures 3a,b, we did not change this to “moment release.”

p. 15: In a few places, the authors invoke things like “site effects” or other declarative interpretations of misfits without demonstration or reference. I would encourage at least including a citation for these interpretations.

It is not clear to us why these vertical waveforms are the way they are. AC40 is in soil (a thick driven pipe) at the Port Heiden airport

(https://www.unavco.org/data/gps-gnss/lib/images/station_images/AC40.jpg). AC42 is in a rock outcrop (https://www.unavco.org/data/gps-gnss/lib/images/station_images/AC42.jpg). It seems that the largest displacements come later, after the seismic waves should have passed. This deviates from the other sites. We've observed similarly odd behavior at other sites for the Chignik earthquake (e.g., AB13, which is near a steep cliff, Parameswaran et al., 2023). So, we've slightly modified the text: "we attribute to site effects" -> "we assume is due to site effects." The other location already was sufficiently qualified "perhaps due to site effects."

Parameswaran, R. M., R. Grapenthin, M. E. West, and A. Fozkos (2023). Interchangeable Use of GNSS and Seismic Data for Rapid Earthquake Characterization: 2021 Chignik, Alaska, Earthquake, Seismol. Res. Lett. 94, 1367–1378, doi: 10.1785/0220220357.

p. 19: I think the authors should continue saying a rupture of the "slab interface" instead of the slab.

Done.

p. 19: I am curious about the tradeoffs between strike-slip and thrust-faulting slip, and any uncertainties the authors could provide here

We are uncertain what the reviewer is asking. We note in the manuscript that there is a clear tradeoff between the two slip interfaces, as the inversion moved slip onto the slab interface model where previously there was higher slip on the strike-slip fault. We have not performed a Bayesian Inversion or applied any other method that would let us formally assess the uncertainty, so we cannot provide detail.

Fig. 6: I would put the slip distributions on the same color scale, since I initially thought the megathrust had more slip.

We contemplated this, but it would de-emphasize some features of the megathrust model, for instance the compactness of one very high slip patch, and some trench and deeper artifacts that some might take interest in. We emphasized the use of different color scales in the figure caption.

Also, I was very confused by the authors shifting the megathrust slip eastward in 6c. I am not sure that is the best way to show overlapping slip distributions.

We've changed the way we illustrate this, treating the megathrust slip more like an inset, which should make this clearer.

p. 27: Where is the “F2” and “F3” terminology coming from? Why is it being introduced here? This stress change discussion seems like a not-particularly-relevant tangent.

These are the faults that Bai et al. (2023) invoked in their model for the tsunami. The original text described the fault details in the following sentence. We have rephrased this now, to make the connection clear.

p. 28: I appreciate the details in the supplement, but I think it is important to state the expected signals from a submarine landslide in the main text of the manuscript.

We moved the previous Text S2 into the main text.

p. 29: One of these supplementary figures probably needs to make it into the main manuscript.

Since the slip from Figure S6 is included in Figure 9, and the tsunami predictions are in figure 10, we opted not to add another figure to the main text.