Dear Dr. Wenbin Xu:

Thank you for the opportunity to respond to these 2 reviews, we have found they improve the manuscript significantly. Please note that as per USGS policy we also secured an internal review from Dr. Benjamin Brooks and our revised manuscript includes the response to his comments. For completeness and transparency those comments are included here as well. Overall, the changes to the manuscript are minor and the main findings of the manuscript have taken place.no alteration of the main findings of the manuscript has taken place.

We are confident you will find these modifications sufficient.

Sean Santellanes

Reviewer D:

General Comments: The manuscript presents an in-depth investigation of the 2020 Sand Point earthquake, focusing on the anomalously large tsunami generated by a predominantly strike-slip event. The hypothesis of concurrent slow reverse slip providing the tsunami generation mechanism is intriguing and well-supported by the combined use of seismic, GNSS, and tsunami data. The manuscript is generally well-structured, following a clear sequence from introduction to methodology, results, discussion, and conclusion. This logical progression helps readers understand the study's purpose, execution, and findings. I have only minor comments.

Specific Comments:

1. **Line 100:** Change "blue" to "dark blue" for clarity and consistency with figure descriptions.
    1. *This has been fixed.*
2. **Line 226:** The authors have adopted a zeroth order Tikhonov approach to invert slip distribution, as opposed to using Laplacian regularization. This method, which penalizes the Euclidean norm (or squared L2 norm) of the parameter vector, could ignore the relationships or connections between neighboring patches. As clearly seen from the slip tile view in Figure 6, there are sharp variations in slip distributions and no zero boundary constraint. I am wondering whether this is physically plausible.
    1. *Inversion S2 is carried out on two faulting surfaces simultaneously, one is a planar source (the strike slip fault) and another a curved source (the megathrust), this latter curved surface is discretized into triangular subfaults rather than the more common rectangular one because rectangles cannot be used to approximate an arbitrary 3D surface. In the inversion code, Laplacian regularization is only implemented for planar faults and not for 3D surfaces with triangular discretization. The triangular sources have different sizes and thus different distances between them so the traditional 1,1,-4,1,1 stencil used to approximate the Laplacian for a rectangular fault cannot be used here. In theory it is possible to design a Laplacian stencil for such an irregular mesh but we have not*

implemented this in the code. On a more philosophical note, we would argue that enforcing a second derivative constraint isn't necessarily more "physical" than the Tikhonov regularization because one doesn't know the inter-fault correlation lengths a priori.

3. **Line 495:** "White star shows the nucleation point for the megathrust." The nucleation point is almost invisible in Figure 8a. I would recommend using a darker color for better visibility.
    1. *This has been changed to a blue star for visibility.*
4. **Figure and Table Alignment:** I would prefer all the tables and figures to be more neatly aligned in the manuscript before publication. For example, Table 1 should be center-aligned with unnecessary spaces reduced; Figure 3c could be stretched to make it more visible and vertically aligned with Figures 3a and 3b. Also, the font size for titles (e.g., "Inversion Name") in Figure 7 is abnormally large and not consistent with the rest of the manuscript.
    1. *This has been fixed.*

Reviewer E:

This paper studies the 2020 Mw7.6 Sand Point, Alaska, earthquake. The basic idea is that it is infeasible that a strike-slip earthquake generated significant tsunamis. Therefore, the authors propose that there could be a thrust earthquake in order to explain the tsunami, but the thrust event was so slow that it was not really observable from tele-seismic waves. My evaluation is that the paper has a point and provides evidence to some extent. It is worth a publication. Overall, I think the paper is somewhat lengthy and some contents could be removed. For instant, the authors described about 7 source models in details, which unnecessary and also confusing. For example, people don't expect the USGS-NEIC model to predict tsunami waves because it is derived from seismic data. Thus, the modeling results of the USGS-NEIC model could be put in the supplementary materials. I think the static finite-fault model, the initial elevation model, and your preferred model (K3) are necessary, and other models can be moved to the supplementary materials (the authors choice). There are many places where the statements are not accurate and not carefully considered. Refer to the following comments.

We appreciate the reviewer's thoughtful consideration of the presentation of the manuscript. Upon reflection, we have decided that USGS model will stay in the main text because we believe it is critical to the story of the strange behavior of the earthquake. However, we have decided to move models K1 & K2 to the supplement, in recognition of the reviewer's concerns for readability. Please note these changes in lines 456-513.

Line 170: unexpected paragraph here

This has been fixed.

Line 209: 210: non-linear -> nonlinear, non-linearity -> nonlinearity

This has been fixed.

Line 214: To remove the tidal components you only have to apply a high-pass filter. I assume the limit of 5 min is to remove the seismic waves, which does not seem to be necessary because these waves are not included in your inversions. Does the limit of 5 min change the latter tsunami waves?

A band-pass filter is needed even in the absence of seismic waves. Infra-gravity waves in the open ocean are common and were present during the time-period of this tsunami event. They inhabit a spectrum (30 s – 300 s) that overlaps with the tsunami spectrum from 120 s – 300 s (see Rawat et al., 2014). Therefore, it becomes necessary to filter them out lest you include atmospheric effects in the inversion. In fact, any time one uses water level data, they must use a band-pass filter, especially in areas where infra-gravity waves are abundantly produced, which includes the Aleutians, Gulf of Alaska, and Cascadia. This correction applies for the whole of the Pacific, as infra-gravity waves can propagate across the open ocean (Webb et al. (1991)). If one inverts water level data without accounting for infra-gravity waves, the resulting inversion could be contaminated with atmospheric noise.

Rawat, A., Ardhuin, F., Ballu, V., Crawford, W., Corela, C., & Aucan, J. (2014). Infragravity waves across the oceans: Following Infra-Gravity Wave Bursts. Geophysical Research Letters, 41(22), 7957–7963. https://doi.org/10.1002/2014GL061604

Webb, S. C., Zhang, X., & Crawford, W. (1991). Infragravity waves in the deep ocean. Journal of Geophysical Research: Oceans, 96(C2), 2723–2736. https://doi.org/10.1029/90JC02212

Figure 2: show all the stations in this map are.

We have added the Sand Point (SAND) station to the map. Given the smaller map region, other stations are not included. We remind the reviewer that all station locations are available in Figure 1.

Figure 3: AA' should be marked on the two maps.

Thank you for noticing this omission. The letters have been added.

Section 2.2, around Line 235: This is confusing. You relaxed the magnitude constraint yet obtained worse fits for tsunami waves. How is that? Here you didn't show the GNSS stations or the GNSS fitting. But from Figure 3a and b, it is seen that the GNSS fitting at AC12 is also worse.

As with S1a, we used the 0^{th} order Tikhonov regularization and L-curve criterion to choose the proper model. Model S1b is presented to show that increasing the magnitude of the strike-slip rupture cannot recreate the tsunami signal and that GNSS fits are best for lower magnitudes.

Line 265: I didn't find the supplementary materials.

The USGS ScienceBase link stopped working, for which I apologize. We have included a new temporary link in the revised manuscript for reviewers to access. USGS protocol is to make the ScienceBase link publicly available once the corresponding manuscript DOI is available (i.e., during type setting, and before the manuscript is published). Therefore, we will include the public link prior to manuscript publication.

Line 260: How is the weighting determined for the coastal and DART stations? Is it the same as used in the finite-fault inversion?

Thank you for noticing this omission. We have added the following text:

"The three DART stations and coastal sea level station SAND are given the same weight while station KING is allotted half of their weight, since higher weighting of KING led to numerically unstable inversions."

All Figures: All the plots of tsunami waves should use the same x/y limits for easy comparison. Figure 3c/4c: the data at 46403 are apparently not the same. You have to make sure you are using the same recorded data throughout the text.

Thank you for pointing out our plotting error with station 46403. It has been rectified.

The tsunami waveforms have been split into two groupings, deep ocean (DARTs) and shallow ocean (coastal/tide gauges). The amplitudes are vastly different for these two groupings, due to the amplitude amplification process as waves go from deep to shallow waters. Thus, we use y limits of 0->0.5 m for coastal/tide gauges, and 0->0.03 for deep ocean (DART). Similarly, the different stations observe the tsunami at very different times, necessitating different limits for the x-axis. For example, the tsunami arrives at the local coastal stations within 2 hours but does not arrive in Hawai'i until hours later. We have opted to retain the x-axis limits to focus on the tsunami signal rather than the pre- and post- tsunami quiessence.

Line 288: Tsunami wave is not a solitary lump. It has a trough from the subsidence in the source area.

The reviewer is correct that the observed tsunami waveform is not a solitary lump. We intended to describe the modeled tsunami wave as approximately a solitary lump. The text has been revised for clarity as follows:

"…megathrust rupture can usually be conceptualized as a solitary gaussian lump; DART station 46402's observed signal exemplifies this typical character (Figure 4c)."

Line 288: How did you rule out the possibility that the two crests are due to wave propagation effects (e.g., wave dispersion)?

Initial spectrograms of the DART observed signals did not show frequency-dependent propagation speed. This makes sense because the source/station distances are short; dispersion is usually noticeable at much longer distances much greater than the fault length (Kirby et al., 2013). GeoClaw does not calculate wave dispersion at these distances (Clawpack Development Team, 2024).

Clawpack Development Team (2024), Clawpack Version 5.10.0, http://www.clawpack.org, doi: 10.5281/zenodo.10896214

Kirby, J. T., Shi, F., Tehranirad, B., Harris, J. C., & Grilli, S. T. (2013). Dispersive tsunami waves in the ocean: Model equations and sensitivity to dispersion and Coriolis effects. Ocean Modelling, 62, 39-55.

Line 312: I don't think discussions of such details make sense, because the inversion has very low resolution and large trade-off. Let's assume that you use a smaller source area in the inversion, or simply force the initial elevation of water in the black circle to be zero, can you still fit the tsunami waves? Basically, the inversion of initial elevation leads to many short-scale features, and most of them are artificial and won't affect the data fits.

We respectfully disagree with this comment. The region inside the black circle only arises with the use of water level data. None of the other inversions that include seismic and GNSS data can replicate it. If the area inside the circle is set to 0, it becomes very difficult to fit King Cove's first tsunami arrivals. Likewise, if one mutes everything that is not part of the proposed submarine landslide signal, you get the following figures.

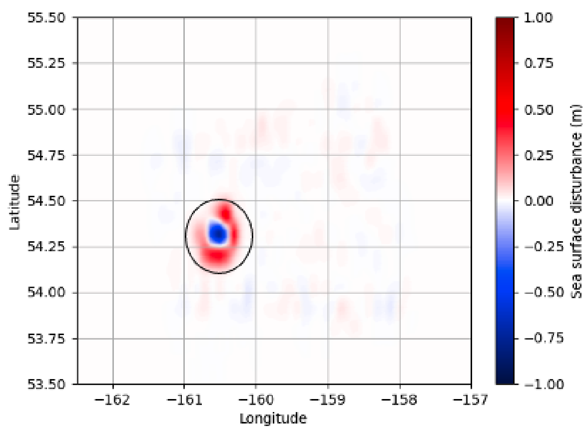

Figure R2.1 The proposed submarine landslide area. An ovular null space shuttle filter has been applied to extract the signal from the H0 model. We utilize the null space shuttle method as described by Deal & Nolet (1996).

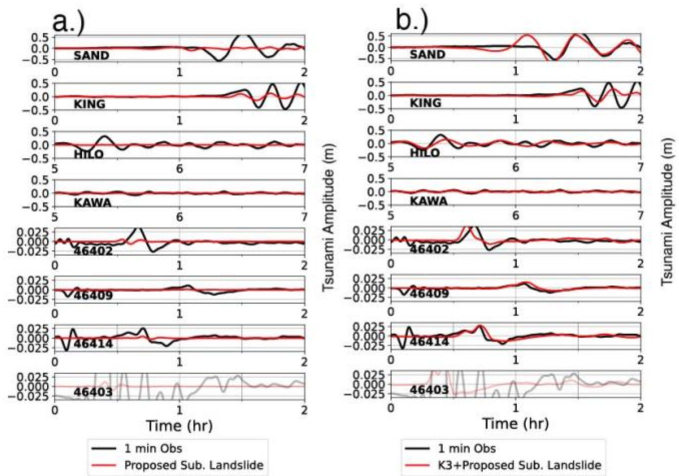

Figure R2.2 a.) Tsunami waveforms for the proposed submarine landslide area after the application of a nullspace shuttle filter. The tsunami waveforms for the observations (black) and the model synthetics (red). b.) The observed tsunami waveforms (black) compared to synthetic tsunami waveforms (red) resulting from the combination of kinematic model K3 and a submarine landslide signal obtained from model H0.

Deal, M. M., & Nolet, G. (1996). Nullspace shuttles. Geophysical Journal International, 124(2), 372-380.

Line 505: allowing a very long rise time will also cause propagation effects of tsunami waves during the earthquake rupture. Is this considered in your inversion? How?

WThe GeoClaw software used to model the tsunami has the option to initiate the tsunami over a span of 10 minutes, accounting for long duration earthquake sources and propagation effects. Due to the long source times, this option has been used for every single model. Added in line 164:

“We use GeoClaw’s kinematic rupture feature for all models for faithful comparisons amongst the different kinematic parameters considered.”

Line 533: It's always very convenient to add some landslide source when tsunami data are not well predicted. However, it's never easy to prove a real landslide actually occurs. I suggest move this part to the discussion section.

We agree that this is a more speculative part of the paper – submarine landslides are difficult to confirm. However, given the propensity of this area of the Aleutians to host submarine landslides, we elect to keep this section as a way to indicate potential areas of future investigation. Comparisons of Bécel et al. (2017) and Barcheck et al. (2020) show that landslides have occurred in the vicinity of ALEUT-5 active seismic lines — that area is close to our proposed submarine landslide — over the span of 4 years. Whether any additional landslides occurred after any of the 2020-2023 Aleutian sequence earthquakes is not yet known. To confirm the submarine landslide would require extensive dating of the recent landslides in this region.

Grace Barcheck, Geoffrey A. Abers, Aubreya N. Adams, Anne Bécel, John Collins, James B. Gaherty, Peter J. Haeussler, Zongshan Li, Ginevra Moore, Evans Onyango, Emily Roland, Daniel E. Sampson, Susan Y. Schwartz, Anne F. Sheehan, Donna J. Shillington, Patrick J. Shore, Spahr Webb, Douglas A. Wiens, Lindsay L. Worthington; The Alaska Amphibious Community Seismic Experiment. Seismological Research Letters 2020;; 91 (6): 3054–3063. doi: https://doi.org/10.1785/0220200189

Reviewer Benjamin Brooks (USGS):

Would help to define that megathrust refers to the subduction zone fault interface, otherwise it feels like dives into jargon.

Fixed.

A bit sensational, how about just “contrast”

Text modified as suggested.

Correct spelling is “Semidi”; this should be changed in text and also in figures

Fixed.

A reference should be given here that encompasses all 3 events.

We have added reference to Brooks, B. A., Goldberg, D., DeSanto, J., Ericksen, T. L., Webb, S. C., Nooner, S. L., et al. (2023). Rapid shallow megathrust afterslip from the 2021 M8.2 Chignik, Alaska earthquake revealed by seafloor geodesy. Science Advances, 9(17), eadf9299. https://doi.org/10.1126/sciadv.adf9299

Should mention what the Simeonof mechanism is when Simeonof if first introduced

Text modified as suggested.

Awkward with the quote. Can you paraphrase in your own words and reference the NEIC event page?

Rephrased and added in reference as follows:

"…has the hypocentral depth at 40 km, yet it strongly prefers slip at shallower depths that extends through the top of the slab (*https://earthquake.usgs.gov/earthquakes/eventpage/us6000c9hg/executive , last accessed July 17, 2024)*."