REVIEWER A

Main comment:

The only weakness that I see in the manuscript is the lack of a near-field velocity model for this kind of modeling. You mention adding low-velocity layers, but that is still not realistic. This could be a significant factor. I suggest adding a paragraph explaining the implications of not having a near-field velocity model and what is potentially lost by not including it. Additionally, providing details about the velocity model used would give readers a better understanding of the progress made when low-velocity layers were incorporated.

We're not sure whether you meant "near-field" in relation to the source or stations. Either way, the velocity model we chose (Yue et al., 2014) was developed using several datasets in the Mentawai region and, in our opinion, could be considered "near-field". We originally misspoke in the text when mentioning that the velocity model does not have a high enough resolution to capture the accretionary wedge, and we removed this sentence. Yue et al. (2014) developed two velocity models— one for the source and one for the receiver side. The velocity model we used was their source velocity model, which does have a lower velocity in the top 8 km to account for the wedge. Figure S8 in the supplement shows a comparison between the Yue et al. (2014) original velocity model and the softer velocity models we tested.

Perhaps you meant it is not realistic to use a 1D velocity model as opposed to a 3D model. We have added a discussion paragraph (lines 583–601 of the revised manuscript) discussing the potential limitations from velocity model selection.

Minor comments:

Line 62: From my knowledge, the term "tsunami efficiency" was first introduced by Miyoshi (1950), Kajiura (1963, 1970), and more recently by Riquelme and Fuentes (2021). I recommend mentioning them since there is a specialized definition for that term.

Thank you for bringing that to our attention. We believe the Miyoshi article you referenced was the 1954 one titled "Efficiency of the tsunami." We have added that reference, along with the Kajiura (1970) and Riquelme and Fuentes (2021) references on lines 63–64 of the revised manuscript. We were unable to find the Kajiura 1963 reference you mentioned.

Background: It's worth mentioning the work of Sallares et al. (2021) because the velocity model is crucial in these cases as it defines the elastic properties of the crust and subsequently influences all calculations thereafter, especially in the near field and high-frequency related calculations. They correlated many observations from the Nicaragua events with other previous studies but fitted the velocity model (elastic properties) with a finite fault.

Thank you, we added a reference to Sallares et al. (2021) in the new discussion paragraph on velocity models on line 587 of the revised manuscript.

Background: The slowness parameter mentioned there is also related to the centroid delay, which is evident, but Duputel et al. (2013) define tsunami earthquakes based on the triangle of the moment function, which also indicates tsunami earthquake behavior. This might aid in characterizing tsunami earthquakes in real-time at some point.

Thank you, we added a reference for Duputel et al. (2013) (line 169 of the revised manuscript). It is true that tsunami earthquakes tend to have anomalous centroid time-delay values, but based on Figure 3 from their study, it does not appear that tsunami earthquake anomalies are that distinguishable from other anomalous events.

Figure 9 is excellent and explains well the differences in frequency behavior among regular earthquakes of the same size.

Thank you!

There is also a paper by Madariaga et al. (2019) that explains how the frequency decays in the near field. Additionally, there are other papers by Shuo Ma on the deformation of the wedge in the case of earthquakes with inelastic properties. Perhaps you could find some useful information there to enhance the manuscript.

Thank you, we found the paper by Madariaga et al. to be quite informative, although we did not find it to be relevant to our study. It focuses on the shape of low frequencies of displacement spectra in the near-field, but our focus was on finding a model to capture the high-frequency decay of ground motion. We did add an additional reference for a Shuo Ma study (Du et al., 2021) for inelastic wedge deformation and tsunami earthquakes (line 121 of the revised manuscript).

REVIEWER B

Main comments:

I would like to see the authors extrapolate on their results and potential uses for this work. Are we now able to create synthetic data to model TsE's in other subduction zones. Could this aid in modeling or tsunami hazard studies?

Thank you for your comment. With this study we estimated the rupture parameters needed to simulate the 2010 Mentawai tsunami earthquake, as well as a range of rupture parameters we recommend using to simulate tsunami earthquakes in other subduction zones. Such resulting tsunami earthquake simulations could then be used to help develop a tsunami early warning algorithm capable of discriminating tsunami earthquakes quickly enough to issue a useful warning. We have added a discussion section on starting on line 685 elaborating on this.

Make sure you are including relevant and recent studies to back up your work. This paper cites many of the appropriate and relevant studies on this subject but misses several more recent publications that are exceptionally relevant to the study of TsEs and the 2010 Mentawai TsE, specifically. Missing references of notable studies by Bilek (2010), Kanamori et al. (2010), Geersen (2019), Ming et al., (2021), Qiu and Barbot (2022), Meng et al. (2022), (Felix, 2022), and Kuncoro et al. (2023).

Thank you for bringing these references to our attention. We were unable to find the references Kanamori et al. (2010) and Ming et al. (2021), but we have added the other suggested references.

The Mentawai TsE is a unique event when compared to other TsE's and furthermore occurred in a notably complex subduction zone (Kanamori et al., 2010; Greerer 2019; Kuncoro et al., 2023). Does using the 2010 Mentawai TsE bias these parameters to a region like Mentawai or do you expect these parameters to be usable more generally for other TsE events or tsunamigenic earthquakes?

You're right, the Mentawai tsunami earthquake occurred in a complex region, and it is valid to question whether parameters derived for this event would be reasonable to simulate tsunami earthquakes in differing subduction zones. However, regardless of the tectonic region, all tsunami earthquakes have exhibited a slow rupture velocity, long rise time, and low stress drop. The parameters we propose modifying help capture these unique characteristics of tsunami earthquakes, regardless of the tectonic setting. This is discussed in the section on starting on line 685 of the revised manuscript.

After going through the paper a few times, it is still unclear to me if these parameters are for TsEs in general or Mentawai/Indonesia specifically. Is the focus of choosing your parameters to be more representative of "typical" tsunami earthquakes? Can you elaborate on this?

As addressed by a previous comment, we estimated the rupture parameters needed to simulate the 2010 Mentawai tsunami earthquake, as well as a range of rupture parameters we recommend using to simulate tsunami earthquakes in other subduction zones. [See lines 737–741 of the original manuscript: "Using *FakeQuakes* forward modeling, we constrain ideal rupture parameter configurations, which successfully recreate the near-field ensemble of ground motions for the 2010 Mentawai TsE. Our intent is for this configuration (Figure 7; equations 7–8) to be used to generate TsE scenarios in other regions around the world to aid in the assessment of tsunami early warning systems in the case of an unexpected tsunami earthquake."]. We recognize that tsunami earthquakes can occur in substantially different subduction zone environments. Nevertheless, they all exhibit unique characteristics (slow rupture velocity, long rise time, and low stress drop). With our results, one should be able to simulate a tsunami earthquake in a different region and still observe these key features of a tsunami earthquake. We have added a discussion section starting on line 685 elaborating on this.

Some information is repeated in the background and introduction sections, I suggest combining them.

Thank you for the suggestion. We agree there was a bit of repeated information. We prefer to keep a separate introduction and background, but we have revised the sections to make them more concise and avoid including unnecessary repeated text.

There is a lot of mention of real-time application (section 5.3 "Real Time Analysis"). I suggest saying *near* real time instead. Your study is really a good feasibility study for modeling and testing the method proposed by Sahakian (2019) for this and other regions. But my understanding with using GNSS station data is that there is a problem with real-time application with converting phase and range GNSS data to displacements (Ruhl et al. 2018). It would have been interesting for you to touch on the analysis of how long these algorithms run in combination with the time it takes for all data to arrive to exemplify the feasibility for real-time or near real-time applications.

Thank you for bringing up your concern; however, processing GNSS data in real time is done routinely and does not have limitations for real-time applications (e.g., Geng et al., 2019; Bertiger et al., 2020; Melgar et al., 2020; Melbourne et al., 2021). Several geodetic earthquake early warning algorithms have been developed which rely on real-time information from GNSS data (e.g., Crowell et al., 2016; Lin et al., 2021).

Minor comments:

Line 29-30 : "…generating exceptionally large seafloor displacements" I would add that it is exceptionally large compared to what one would expect from the seismic moment / magnitude. These are shallow *and*long rupturing events.

We have revised this sentence to include “for their magnitude” on line 60 of the revised manuscript.

Line 59: “…magnitude M7-8…” often see Mw 6.8-8.0 in the literature. Please check or add a reference here.

We were providing an approximate magnitude range. We have clarified this on line 61 of the revised manuscript.

Line ??: "The rupture parameters of rise time..." Large section between 80 and 85 is missing line numbers?- No need to repeat word "parameter" in "... high frequency stress parameter." I would also add a reference here in this section.

There was a formatting error which caused some of the lines to not be included in the numbering (which has now been fixed). We removed the sentence starting with “The rupture parameters…”, but we added more references in this section.

Line 83-86: Run-on sentence here, I would split this sentence up

We have revised and split the sentence on lines 97–100 of the revised manuscript.

Line 73 and 87- 90: For moment magnitude I would suggest using "Mw" or appropriate magnitude type for all cases in the text. For Sanriku, for example, the magnitude you list is tsunami magnitude (Mt). Please also add a reference for your magnitude as these Mws can differ from publication to publication. For Example, Polet and Kanamori (2022) cite 1992 Nicaragua TSE as Mw 7.7, the 1994 & 2006Java TsE as Mw7.8 and 7.7 respectively, the 1996 Peru TsE as Mw 7.5 and do not include the 1947 Gisborne and Tolga Bay events. Geersen (2019) lists the 1947 Poverty Bay and Tokomaru TsEs as Mw 7.1 and 7.0.

Thank you for providing those references. We used the moment magnitude naming convention of a bold M (**M**) as recommended by Tom Hanks for each of our magnitudes. The **M**8.0 we listed for Sanriku is the moment magnitude as suggested by Tanioka and Seno (2001), whereas they indicate the tsunami magnitude as M_t 8.6. We have removed this section though and instead just refer to the global map of tsunami earthquakes (Figure 1).

Line 100: “since the development of modern seismic instruments” I suggested adding what you mean / approximately what year? 1990?

Thank you, by “modern seismic instruments” we meant the widespread development of digital broadband seismic networks in the 90s. We have clarified this in the caption for Figure 1. We were unable to find a direct reference for this, but it likely stems from recent technological advancements (first broadband instruments developed in the late 70’s and early 80’s), formation of IRIS organization and deployment of more instruments), and research and funding increases following the 1994 Northridge and 1995 Kobe earthquakes.

Line 110: Make sure all references are up to date account for most recent publications: Geersen (2019) TsE specific study on structural similarities of topography and trench sediment thickness. Kuncoro et al (2023) focus on the accretionary wedge of West Sumatra and the Mentawai TsE.

Thank you, we have added the Geersen (2019) reference. As this section focuses on erosional settings, we do not find Kunocoro et al. (2023) relevant here, but we have included the reference elsewhere in the paper.

Line 116 : "1/10th" need to reference this- I suggest Bilek and Lay (1999)

Thank you for the suggestion, but we decided to remove this sentence.

Line 119: Equation 1- I don't think defining this equation is necessary. If you're not referencing this equation later on in the text I suggest incorporating it within the text like you do on line 368-369.

Thank you for the suggestion, but we removed this equation from the text.

Line 120-123: "i.e., more velocity weakening and unstable…" The wording here is a little unclear and confusing. I suggest re-wording for clarity. A positive b-a corresponds to velocity weakening and unstable slip. Also is this reference up to date?

We intended for this sentence to convey that increasing b–a (or making it more positive), makes the material more velocity weakening and unstable, thus allowing for coseismic slip. We have clarified this on lines 121–124 of the revised manuscript. We find the use of these references suitable because the claim that large-volume prisms increase tsunami height and tsunami efficiency by increasing the rate-and-state parameter is a direct result from Lotto et al. (2017)'s study. Our intent with including the Scholz (1988) reference was to provide the seminal reference for the friction laws, thus we do not find it necessary to provide a more recent reference. However, we realized that the seminal references should include Dieterich (1979) and Ruina (1983), so we have added those references.

Line 129: "on occasion" I suggest removing "on occasion" for clarity and conciseness.

We have made this revision.

Line 136: I suggest removing "still"

We have made this revision.

Line 129 – 148: The information presented here is solid, but out of date. Notable missing references in this section: (Bilek, 2010), (Kanamori et al., 2010), (Geersen, 2019), and (Felix, 2022)

Thank you for bringing these references to our attention. We were unable to find the references Kanamori et al. (2010), but we have added the other suggested references.

Line 153: "… so they do not register as a threat" - I would clarify here, that a magnitude 7-8 event is still a major event, do you mean tsunami potential?

We have revised this to clarify that a tsunami warning system would underestimate the tsunami hazard from a tsunami earthquake compared to a typical tsunami. See lines 79–80 of the revised manuscript.

Line 165: = "the slowness parameter" or "tsunami earthquake discriminant"

Thank you for the suggestion, but we removed this sentence during revisions.

Line 167: "determinable in *near* real time." Although routinely calculated at the Pacific Tsunami Warning Center, it is my understanding that is not determined in real-time due to the requirement of focal mechanisms.

Thank you for the suggestion, but we removed this sentence during revisions.

Line 168: “false TsE detections”

Thank you for the suggestion, but we removed this sentence during revisions.

I’m not sure Weinstein and Okal (2005) had false detections, though they did not have robust results using stations less than 30 distance degrees away, which was addressed in Ebeling and Okal (2012).

Thank you for the suggestion, but we removed this sentence during revisions.

Line 174: “do not stabilize quickly enough” *if inundation is less than ~15-20 minutes as was the case for Mentawai, (though not for an event like 2006 Java TsE where there were ~ 40 minutes to inundation).

We have clarified that this is an issue for near shore ruptures on line 167 of the revised manuscript.

Line 182: “a s” extra space between a and s.

We have made this revision.

Line 182-186: run-on sentence, a little difficult to read.

Thank you for the suggestion, but we removed this sentence during revisions.

Line 194 – 198: I suggest adding a citation here

We have included additional references to this section.

Line 203: “V_{rupts}” V_{rupt} or V_{rupts} ?

We have changed all accounts of “V_{rupts}” to be “V_{rupt}”.

Line 224: “In this way…” not sure what you mean by “In this way…”

We have revised this sentence on line 212 of the revised manuscript.

Line 231: “stations” move “stations” - “…HR-GNSS *stations* operated by ….”

We have made this revision on line 218 of the revised manuscript.

Line 241: “fakequakes” For people looking to use fakequakes, I might note as you introduce it here that fakequakes is part of the MudPy package on GitHub.

We have added this clarification on line 232 of the revised manuscript.

Line 258: “FakeQuakes” This instance is not italicized.

We have made all references of “FakeQuakes” italicized.

Line 259-261: “This approach has been…” I think I know what you mean here, but the wording is confusing. Please revise.

We have revised this sentence on lines 250–251 of the revised manuscript.

Line 266: "..best resolves all observational data." Compared to other models that you tested? Please clarify.

By this phrase, we meant that their inversion was developed using all available data, rather than a subset of the available datatypes like other studies had used. However, this is redundant with the first part of the sentence ("…their inversion used the most comprehensive dataset"), so we have removed that phrase.

Line 268-269: You mention "as opposed to.. 6-12 m… 13-20 m…" Where do these numbers for peak slip and hypocenter come from? Please reference.

These ranges for peak slip and hypocentral depth came from the references I provided in the first sentence ("Various slip inversion models exist for the 2010 Mentawai TsE (e.g., Hill et al., 2012; Newman et al., 2011; Satake et al., 2013; USGS, 2018; Yue et al., 2014; Zhang et al., 2015"). We have moved these references to be within the parentheses with the number ranges for clarity (See lines 257–260 of the revised manuscript).

Line 323-324 : "… filter of the low/high frequency data…." Do you mean "on" instead of "of"?

Yes, we have made these corrections.

Line 327-328: "…generated using typical megathrust parameters…" Why not use TsE parameters since that is the focus of your study? Or do you mean to highlight the difference of TsEs with typical megathrust earthquake parameters by using them for your simulations?

Yes, we used typical parameters for this figure to highlight the poor fit when using such parameters to simulate a tsunami earthquake. We have clarified this on lines 320–321 of the revised manuscript.

Line 338: add parentheses around 2017

I think this was actually referencing line 438, but we have made this revision on line 402 of the revised manuscript.

Line 338-347: This is interesting. Could this stem from excessive long and slow rupture? Do you see anything similar for other earthquakes or those used in Sahakian et al. (2019)?

Yes, this likely stems from excessive long and slow rupture. Rupture of the shallow domain (especially with the presence of a defined wedge) tends to result in longer-duration events that are biased towards lower-frequency energy. Fukao and Kanjo (1980) identified a shallow zone within the Japan trench (Zone I) where low- and very low-frequency earthquakes were observed, which they also associate with tsunami earthquakes. This low-frequency ringing was not notably observed for earthquakes analyzed in Sahakian et al. (2019). Other than the Mentawai tsunami earthquake, the events in their study were more typical megathrust events.

Line 359: Equation- I cannot find this equation in Melgar and Hayes (2017).

You're right, the equation is not explicitly written out in Melgar and Hayes (2017). We took this equation from the *FakeQuakes* source code (https://github.com/dmelgarm/MudPy), which was written by the primary author of the Melgar and Hayes study. We have revised this sentence to say "We use the Melgar and Hayes (2017) scaling relation..." on line 355 of the revised manuscript.

Line 361: “evident from Figure 1 of Melgar and Hayes (2017)” I would avoid referencing figures in other papers. And here do you mean in Figure 1b where the TsE’s are labeled? I think you can cite this finding from Melgar and Hayes (2017) without having to reference the figure.

Yes, we were referring to their Figure 1b. We have made this revision on lines 350–352 of the revised manuscript.

Line 369: By “this value” do you mean rupture propagation speed or Vs?

We were referring to the rupture propagation speed. We have made this clarification on line 359 of the revised manuscript.

Line 373: “… velocity profile does not exist in the Mentawai, Indonesia region…” Would the study by Kuncoro et al. (2023) be helpful here?

We have removed these sentences as a response to a comment from another reviewer. We originally misspoke in the text when mentioning that the velocity model does not have a high enough resolution to capture the accretionary wedge. One of the Yue et al. (2014) velocity models was developed for the source (i.e. wedge) side, which is the model we used. We do not believe the velocity model used in the Kuncoro et al. (2023) study would add any value over our current model. They did not develop their own model, but adopted the velocity model from Collings et al. (2012). The shallow portion of the Collings et al. velocity model for the wedge is unrealistically high, where velocities are more on part with rigid crust material rather than weak, compliant sediments. The Yue et al. (2014) model used in our study was modified from the Collings et al. model to better model the observed tsunami amplitudes.

Line 378: “better method to be to modify..” remove “to be”

We have made this correction.

Line 388 and 486: SSFs not SSF’s ?

We have changed all accounts of “SSF’s” to be “SSF”.

Line 389: “In this study” This phrase is used repetitively in this section.

We have removed the repeated use of this phrase.

Line 392: I suggest re-wording this first sentence, the parenthesis with the comment and reference does not flow well.

We have revised this sentence on line 371 of the revised manuscript.

Line 406: “10 Hz” Is 10 Hz the Nyquist frequency for this data?

The Nyquist frequency for this data is 50 Hz. We did not consider data at frequencies above 10 Hz because all of our strong motion records exhibit unusual behavior above 10 Hz (see Figure 1 below). This artifact appears to be a result of data processing or instrument limitation, but as the data were processed and provided to us by a collaborator at the Agency for Meteorology, Climatology, and Geophysics of Indonesia (BMKG), we do not know the exact source. However, we only consider frequencies below 10 Hz. We mentioned this in the original submission of the manuscript on lines 405–406— "The upper bound of 10 Hz is a limitation of seismic data processing prior to our acquisition of the data."

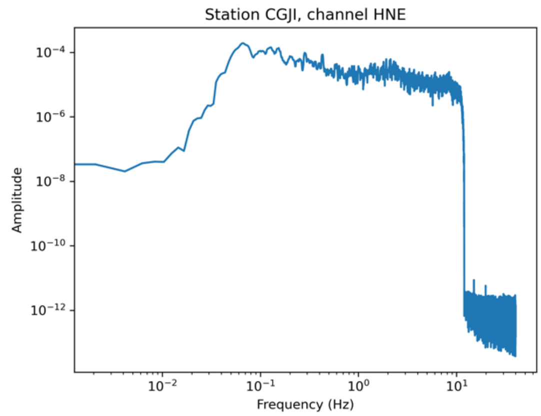

Figure 1. Example Fourier amplitude spectrum for station CGJI. Data at frequencies >= 10 Hz appear to be compromised, so we set 10 Hz as an upper limit to our calculations.

Line 407-416: This section reads more like an abstract and doesn't seem to fit here. I think it is unnecessary to say what you expect. You can include this in your discussion or conclusions when your results do/don't match what you anticipate.

We agree, and we have removed this section from the revised manuscript.

Line 426, 444 and 493: "Fig." First instances of abbreviating Figure to Fig. Please pick one and be consistent throughout.

We have changed all "Fig." abbreviations to the full word "Figure".

Line 441: "rietime" replace rietime with risetime

We have made this correction.

Line 452: "V_{rupts}" V_{rept}s not V_{rupts}? If you want to make it plural, the 's' should not be in the subscript, otherwise you are changing the variable name.

Thank you for pointing that out. As mentioned in a previous comment, we have changed all accounts of "V_{rupts}" to be "V_{rupt}".

Line 463-464: "simulated data have more high frequency energy than observed" This is very interesting. Often times with seismic data the higher frequency content is lost in synthetic data. What do you contribute this to? Do you see a similar result in other simulated data for non-TsE events?

It is unclear to us what you mean by higher frequency content is often lost in synthetic data. Do you mean that there is a computational resource limitation when simulating data deterministically at higher frequencies (e.g., Rodgers et al., 2020)? If so, this is a semistochastic approach, which does not suffer from the same computational limitations. Although, it is not physics-based out to this higher frequencies. (Graves and Pitarka, 2010, 2015). Tsunami earthquakes are unusually deficient in high-frequency energy. Thus, using a "typical" stress drop value results in simulations with larger simulated high-frequency ground motions than observed. The high-frequency simulated ground motions behave as expected for typical (i.e., non-TsE) events (e.g., Goldberg and Melgar, 2020; Lin et al., 2023).

Line 487: "can vary" "can vary *by* orders of magnitude"

We have made this correction.

Line 503: What are the expected IM residual trends?

This was a reference to lines 407–416 in the original manuscript, where we described the expected behavior of the intensity measure residuals, which sounded more like an abstract. As we agreed to remove that section, we revised this sentence to say "We find that individually varying the rise time, V_{rupt}, and stress parameter expectedly controls the rupture kinematics and resulting ground motions…" on lines 462–463 of the revised manuscript.

Line 511: "for this study" many instances of "for/in this study" throughout the paper, I suggest being more direct with your language.

Thank you for bringing this to our attention. We have removed repeated instances of "for this study".

Line 515: "We assume…" do you mean "We use" ?

Yes, we have made this correction.

Line 525: Make sure you define all variables and symbols used ()

We looked over the text, and all variables and symbols are already defined for this equation. We noticed that "j" was not defined for the next equation, so we added that in the text on line 488 of the revised manuscript.

Line 538: "mostly likely" "*most likely"

We have made this correction.

Line 535 -540: This feels like it belongs in the figure description, not the body of the text. Some of this is duplicated in the figure description.

Thank you, we have removed the redundant text from this section and left it in the caption (now Figure 8).

576: "surved" Do you mean served?

We actually meant "surveyed". We have made this correction on line 529 of the revised manuscript.

636: "non-insignificant" I suggest removing the double negative here and just say "significant."

We have made this correction.

Line 661: "PGA as a proxy (M_{PGD})" I'm slightly confused here. Didn't Sahakian et al. (2019) use PGA as a proxy for radiated energy and the PGD as the proxy for seismic moment? Please clarify what you mean here.

Line 661 actually reads "PGA as a proxy (M_{PGA})", and yes you are correct. PGA was used as a proxy for radiated energy, and the $\mathbf{M}_{PGA}$ parameter they define is essentially the magnitude of shaking. PGD is a proxy for seismic moment, and $\mathbf{M}_{PGD}$ should be similar to the moment magnitude. We have clarified this on line 623–624 of the revised manuscript.

Line 692: "Fig." Figure

We have changed all instances of "Fig." to Figure.

Line 717: "calcualted" calculated

We have made this correction.

Line 720: "Figures 9-10" Figure 10 is missing.

Thank you for pointing this out. This was a typo and should have said "Figure 7" (now Figure 8). This correction was made on line 682 of the revised manuscript.

Line 726: "typically aseismic" I would disagree here. Do you mean that TsEs occur out of the classic "seismogenic zone" of subduction zones?

Thank you, yes we meant that the tsunami earthquakes occur outside of the traditional "seismogenic zone", but this zone (Domain A in the figure below; Lay et al., 2012) can be considered aseismic. We have revised this sentence to say "updip of the traditionally aseismic region" on line 706 of the revised manuscript.

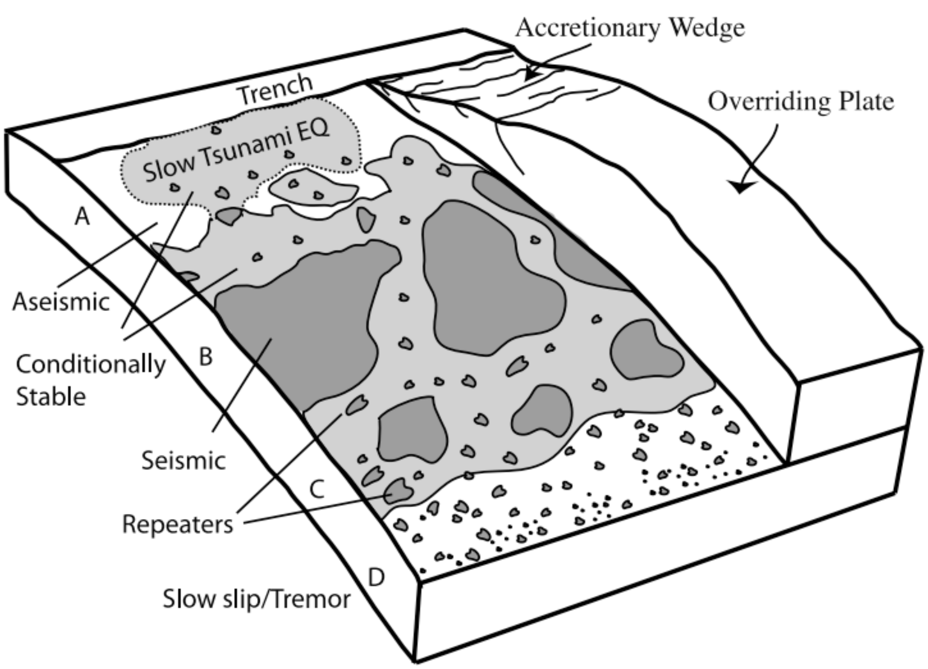

Figure 2. Figure 15 from Lay et al. (2012) illustrating the domains of a typical subducting megathrust.

Line 734: “aimed to” Could remove “aimed to”. Since your study did was successful.

Thank you, we have made this revision.

Line 741-745: “We are aware… physical properties of the subduction zone.” This feels unnecessary. My understanding is that you are justifying your work, but this doesn’t fit in the conclusions and distracts from the conclusions. You’ve done great and interesting work, but your conclusions seems to shy away from that.

Thank you, we appreciate your support of the work we’ve presented. We included this section to be fully transparent about the limitations of this work, but we agree that it distracts from the conclusions. We’ve moved this section to a new discussion paragraph (*5.3 Extrapolation to other regions*) starting on line 685 of the revised manuscript.

Line 767: “2023” Was this zenodo dataset published 2022 or 2023? Different year on this line than in the references (line 900). It is also referenced with the year 2023 in the supplementary introduction.

Thank you for pointing this out. We have not actually published this dataset, yet, but it is saved as a draft in Zenodo with a DOI. We felt it best to wait to publish the results in case reviewer comments required re-running simulations or analyses. We have modified the reference to list the year 2024.

Line 770: “tsuquakes” I suggest adding a ReadMe file or some description to the tsuquakes Github repository. Or perhaps, highlighting which (if not all) scripts were used for this study.

We have cleaned up the Github repository by removing unused scripts and added a ReadMe file.

Line 773: “matched filter technique implemented” You mention the supplementary information includes analysis on the matched filter technique implemented. This appears to be missing?

Thank you, we were referring to the supplemental figures S3 and S4. That was not made clear in the figure captions, so we have revised those.

Line 810: Fadugba et al. (2023) When referencing a pre-print article make sure to check and see if it has been published before final revisions of this paper. As of March 11, 2024 it is still a pre-print.

Thank you for the reminder. The article was published recently, so we have updated the reference.

Figure and table comments:

Table 1: Not all journals require this but it could be helpful to define acronyms/variables used in table and figure descriptions.

We have added definitions for the acronyms/variables in the table description.

Figure 1: I suggest making (1b) larger if you can. It’s a little difficult to see with overlapping stations. For example, it appears the SLBU and PPSI are both labels for the same seismic station. GNSS station PKRT is labeled but not mentioned in the paper. In the description you could say before and after “since the development of modern seismic instruments” rather than repeating this twice.

Thank you for your comment. We have split this figure, where Figure 1 is now just the global map of tsunami earthquakes and Figure 2 is the data map of Mentawai. We have also included an inset on Figure 2 which zooms in on the GNSS stations.

Figure 2: Please define the yellow star and what is the difference between b, c, and d.

The yellow star is the event hypocenter. Panels b-d are simulated slip patterns that were generated using the finite fault inversion in (a) as an average slip model. They are similar, but not exactly the same as each other because they are stochastic variations. We have made these clarifications in the figure caption.

Figure 3: Your labels for 2(a-c) and 2(d-f) appear to be reversed. Or the station labels are on the wrong figures. Please check this.

You are correct. We have made correction in the figure caption.

Figure 4: What do the diamond shapes stand for?

The diamonds are the outliers of the boxplots. We have clarified this in the figure captions.

Figure 5: Could figure 4 and 5 be combined?

We do not think it would make sense to combine Figures 4 and 5 without also combining Figure 6, because they each illustrate results from the same analyses but using a different rupture parameter. As the resulting figure would be too large if all three figures were combined, we have chosen to leave Figures 4 and 5 as separate figures.

Figure 6: Could look more clear if you make the lines thinner.

Thank you, we made the lines thinner, and we also removed the edge color of the filled boxes to help the standard parameter boxes to stand out.

Figure 7: Just want to confirm that 7a and 7b are meant to share the SSF axis? (right vertical axis for 7a and left vertical axis for 7b)

Yes, they share the same axis. We have revised this figure (now Figure 8) to make that clearer.

Figure 8: Again, what are the diamond shapes?

The diamonds are the outliers of the boxplots. We have clarified this in the figure captions.

Figure 9: This figure is difficult to read with all the overlapping boxes. For all the shapes for the different earthquakes it would be helpful to have darker outlines of the shapes. Additionally, you include these 15 observed events, but don't include much information on these events in the text or supplement. Are any of these events overlapping with those used in Sahakian et al. (2019)? And is there anything unique about the Nepal 2015 and Ibaraki 2011 events? Why do they also have negative M_{PGA}-M_{PGD}?

We agree, the figure was not very clear with the overlapping boxes. We have modified the figure to make the boxplots from the individual parameters more distinguishable. This figure is now Figure 10 in the revised manuscript. The Mpga-Mpgd values for the observed events were all taken from Sahakian et al. (2019), and we revised the figure caption to specify this.

Regarding the Nepal 2015 and Ibaraki 2011 events, it is not immediately clear why they have somewhat negative Mpga-Mpgd values. The USGS event catalog shows maximum estimated PGA to be 0.2g for the Ibaraki event, which is significantly lower than the Ecuador 2016 and Nepal 2015 events of similar magnitude (maximum PGA estimated to be 0.5g). The Ibaraki hypocenter was also significantly deeper. It is difficult to find much information about the Ibaraki event though as most articles focus on the Tohoku event that occurred just shortly before, so it is not clear whether those ground motions were expected for the magnitude and depth. The Nepal event recorded reasonable PGAs that are consistent with ground motion model estimates (Hough et al., 2016); however, it was recorded on only a handful of stations (Sahakian et al., 2019). It's possible that Mpga was more difficult to constrain for this event due to the minimal data, so a smaller magnitude was needed to statistically support the null hypothesis. Regardless, the Mentawai TsE is still an outlier when compared with those events.

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