

Reviewer A:

Tsunami hazard assessment is important for coastal zone, and thus a well-predefined inundation limit zone and maximum inundation depth prediction are crucial for both long-term mitigation strategy making and during the timeliness early warning. Santallanes et al. have done a thorough analysis through comparing inundation limits predicted by homogenous and heterogenous slip models in the CSZ. As reported by previous studies and also this study, not surprised, the heterogenous models assure a larger, and thus a greater inundation limit further inland. They conclude that previous standards used for tsunami evacuation are still valid since their predicts have 20-60% larger estimates. These analysis and results are useful and important for next generation of tsunami hazard assessments in the CSZ. Below are some questions to be addressed before the publication.

Why 200 models? Is it enough to capture the heterogeneous of the rupture in regarding the large seismogenic zone at Cascadian? :::{.Danger} Response: We use 200 models because it was computationally more feasible to run for the science question we were asking. If given more time, we would run a much larger ensemble. These 200 models thus only capture a small subset of possible heterogeneous ruptures of the large seismogenic zone of the CSZ. Lorito et al. (2025) suggests the required number may be in the amount of tens of thousands of synthetic events, which would be compounded by the need to inundation modelling. :: How do the 2004 Sumatra and 2011 event mirror what a future look like in CSZ? In what sense?

Response: In the context of the manuscript, a future CSZ rupture would mirror those events in the amounts of casualties. DOGAMI forecasts at least, 20,000 casualties in Oregon alone.

Further inland by how much?

TThese?

(Eungard et al.,2018).

Response: Inundation further inland should be treated in a more probabilistic sense, as we do in our study. The work being referenced is a deterministic scenario, which is useful for some purposes (i.e. evacuation). A better defined quantity of how much further inland is being worked on by the co-authors of this study, as it will cover the entire CSZ adjacent coast with a 30 m resolution. All other typos fixed. Thanks!

Imprecisions in probabilistic assessments of tsunami hazards? Should inaccurate better than imprecision? llikewise?

Response: These mistakes have been fixed. Thanks!

The coupling in Alaska was not well constrained along both dip and strike due to lack of data constraints.

Response: The following sentence has been added in: “Although, some of these subduction are not well constrained due to the lack of data.”

“Currently perfectly pinned” refer to 100% locked? If so, the high coupling is vague to reflect 100% locked.

Response: This sentence has been changed.

A high coupled area is not necessary to rupture with large slip, which highly dependent on the slip memories over a seismic cycle or many seismic cycles.

Response: Current sentence reflects this sentiment, and is a leader into the next paragraph, which discusses this issue.

“While we cannot explore the ASCE 7-22 models. We can infer what their models look like from conference proceedings and documents.” this looks like a sentence? Response: While switched to “Although,” in order to keep two short sentences ahead of longer sentences. It keeps it easier to read.

“The 372 models they use”—they used?

Response: this has been changed.

Figure 2: it is confusing, very large areas are uplifted and subsided by large amplitudes, which do not make sense. Either it is a plotting issue or something else. Simply plot the vertical without the bathymetrical and topographical effect should work better, and for safety, overlay the vertical displacements contours. The same applies to Figure 3 as well.

Response: All changes implemented.

Figure 3c: why there all the slip occurs deeper than a clear depth contour (30 km)? why not supposed to be shallower than this contour? the same questions for (d). It is uncommon to have a cut of line that one sides has very large slip; while the shallow depths have zeros slip. This feature could be hardly seen from slip models with $M_w > 7$ earthquake in the database (Hayes, 2017; Ye et al., 2016)

Response: The slip files utilize the GeoClaw okada deformation code, which can sometimes lead to deformation deeper than 30 km. The fix for this problem was to clip along the CASIE21 slab polygon. However, it still permits deformation past certain boundaries. For tsunami initial conditions, this does not really matter. Also of note, contours were not added to this plot because they pose visual issues. Those persons suffering from poor vision could not see the contours at any resolution.

What does total coseismic deformation mean?

Response: In this case, vertical + horizontal.

When compare the XL1 model between homogenous and heterogenous, the difference is not that much, see figure 5. These subtle differences then are attributed to both the source complexity and coseismic subsidence. Assuming the coseismic effects on land height changes, then the difference should be small as reflected in figure 5.

When discussion the correlation between the high coupled area with the large tsunami inundated points, except the slip amount, the horizontal topography should also contribute. In addition to this, the local nearshore bathymetrical feature also matters on tsunami excitation.

Is there some demonstrations to show the entire sites that are fully below mean high water?

Response: It depends on what you refer to sites. In the case of Crescent City, it sometimes happens that the FakeQuake rupture has the entirety of the domain below mean high water. The other areas of Newport and Ocean Shores suffer from this feature much less so. However, it could be the luck of the draw for the stochastic generated events. A more physics based model (i.e., RSQSim) might not be so aggressive with the subsidence.

Not so clear why the middle New port area has the minimum subsidence effect from all the rupture scenarios while the other twos have significant subsidence.

Response: As with the above response, it may be a function of the amount of stochastically generated events. But also, Newport has a greater topography change. North Newport rises in elevation much faster than South Newport, and both have more topographic change than either Ocean Shores and Crescent City.

“Due to the source complexity of these scenarios, it was common for the two out of the three sites farthest from the main rupture asperity to have inundation depths of 0-3 m while the other site local to the main asperity had inundation depths of 10-20 m. This behavior was seen in the 2011 Tohoku-Oki tsunami wherein local tsunami runup was > 40 m for places near the main rupture asperity (Mori et al., 2011, 2022)

” these sentence mention the high inundation depths are correlated with main asperities, which include the shallow trench asperities as well. But it was argued that the shallow ones do not determine the final inundation depths alone.

Response: This has been made more clear with the reviewer feedback. Thanks!

Recommendation: Revisions Required

Reviewer E:

Comparisons of tsunami inundation between homogeneous and heterogeneous earthquake sources at select sites for the Cascadia Subduction Zone

Sean R. Santallanes et al. (submitted to Seismica)

Steven J. Gibbons (reviewer – anonymity waived)

This is a well-executed and well-presented study of tsunami inundation along 3 stretches of coastline in the Pacific North West when subjected to different scenarios of Cascadia subduction earthquakes. The main conclusion is that the definition of the slip distribution affects the impact (inundation) at first order and that the spatial variation of slip cannot be ignored in Probabilistic Tsunami Hazard Analysis (PTHA). The subject is within the scope of Seismica and I recommend acceptance following minor improvements and corrections. It has been understood for many years now (as well-referenced in the manuscript) that spatially heterogeneous slip can increase tsunami intensity relative to homogeneous slip models. This has often applied to far-field sources; here the sources are very close to the coastlines being inundated so the effect is likely even more pronounced. This may be worth commenting.

On page 4, we read “homogeneous or quasi-homogeneous slip distributions” without explaining what quasi-homogeneous means. I see in the next section this being elaborated with “with slip now having along-dip smoothly varying slip and considering splay faults and other complexities”. Is this what the “quasi-homogeneous” in the previous paragraph meant? But all of that seems like quite a lot of complexity to me. I don’t insist on any change here, but it might be helpful to the reader to provide a hint or two as to what “quasi-homogeneous” involves up-front.

Response: This change has been done to the specifications of the reviewer.

Figure 2 caption: L1 should be XXL1? Can you clarify what we are looking at in this Figure? Is L1/XXL1 one specific earthquake hypothesis or does it encompass a range of different scenarios of a similar magnitude? If so, which scenario is being displayed here?

Response: In Figure 2, it is the L1 scenario from the PMEL catalog. This has been made more clear in the caption.

Figure 3 displays far more information squeezed into a far smaller plot

(I wouldn't mind Figure 3 being expanded to fill two rows with more air and larger panels!) Figure 3 shows both the slip and the vertical deformation (which is often non-intuitive!). Would it be possible to show the corresponding slip distribution for Figure 2? (This is "homogeneous", yes?) I think this would help us to understand the differences between the homogeneous and heterogeneous slips. It is not obvious to somebody new to this work that the vertical deformation displayed in Figure 2 comes from a homogeneous – or quasi-homogeneous(?) – slip model. I imagine that the slip model displayed here already has a lot of geometric complexity compared with simpler multi-rectangular fault zone representations of earthquakes that other readers may be familiar with.

I would appreciate the references to the different slip models in the caption to Figure 3. I know they appear in the text elsewhere but it doesn't take much space and many readers use the figure captions as concise summaries of information!

Response: I will ask about the possibility of making this a page length plot along the vertical axis. This figure does pose some visual concerns being limited to the textwidth of the horizontal axis. The slip distribution cannot be added to Figure 2. The t-shirt models provided did not include the slip. It is not clear if these deformation models provided to us were based off the rectangular subfaults or triangular subfaults, so an inversion of it would not be in good practice without that information.

Page 9: Manning coefficient of 0.025 (which is held constant).

To me "held constant" means something that should be varying with time that is constrained not to change. I think what is meant here is that it is specified as a constant over the full spatial domain. I would use the term "uniform over the spatial domain" or similar. I think the final sentence here "however, realistically varying Manning coefficients requires knowledge of the land use footprint of the sites, which does not exist at this current time." could be rephrased. Not existing at a given time is a (literally) existential term that I don't think applies to something that could be obtained with a little more work and the implication is that if only we had a land-use map we would be able to apply a realistic spatially varying friction map. Even with a detailed land-use map, very arbitrary decisions need to be made about how to vary the Manning coefficient and the land use often changes at a spatial scale similar to that for the inundation modelling so I don't think this is trivial or a great shortcoming of the paper. I would probably write something like "however, realistically modelling the spatial variation of the friction would require a detailed land-use map and a pre-specified mapping of the land-use to a Manning coefficient ... " – up to you, but this opens up a Pandora's box of assumptions. ("Is the reduced inundation here because I made a bad choice about the Manning coefficient for this particular cell?")

I see that "constant" and "varying" are used throughout to describe the Manning friction coefficient. This does suggest to me varying in time (or e.g. within a simulation). I think "[spatially] uniform" and "[spatially] non-uniform" would remove any doubt.

Response: Thanks for the feedback. I have tried to incorporate it as best as I can. Yes, I will mention the limited test of simulations as well.

I now see that document of the supplementary material does indeed examine the behaviour with non-uniform Manning coefficient, something that contradicts the paragraph on Page 9. So page 9 should be revised to say something like "In the main study we have applied a uniform Manning coefficient, but we have done a limit set of simulations using spatially non-uniform friction. These are described in the supplement."

Response: Thanks for the feedback. I have removed the supplement per the points below. I did amend the paragraph on page 9 to remove any doubt about the Manning coefficient being uniform over the entire spatial domain.

Page 10: "less than" à "less than or equal to" (I know you say this to emphasize the fact that it might be less than, but it is more correct to say less than or equal to the total number of events).

Response: This has been changed. Thanks!

Figure 7 (and 8 and 9).

The black line is not very visible against the dark grey background. Would it be possible to increase the contrast? (different color line or paler background?)

Response: I have fixed this issue. The implemented change is a line style of alternating black and white dashed lines, which should be more apprenent than the solid, thick black line.

I notice that the (c) panel in all figures shows the 50% percentile for the L-scenarios and panel (d) contains the 95%-percentile. So these side-by-side plots aren't really comparing like with like which is usually the case with side-by-side panels (where typically one thing is varied). Here, two things are varied: (a) the source scenario and (b) the percentile. So it looks like the difference between the two scenario families is even greater than it need be. I understand that you are probably showing an "average case" of inundation versus "an almost worst case" inundation scenario – but it would probably help to make this explicit in the caption so that readers (who are often as pedantic as me!) don't ask the same question.

Response: I thought about this pretty hard, and I ultimately decided to change the 95th percentile to the 50th percentile. This change is done to make the comparison more logical. The caption now also contains more information about the types of scenarios.

In Figure 6, I see violin plots showing the range of inundation heights for different ensemble members of classes “L” and “XL” and single values for scenarios L1 and XL1. This raises my confusion again as to whether these letters represent single scenarios or ensembles. “L” *must* be an ensemble of different slip models (otherwise what is being displayed in the violin plots). But L1 is a single scenario? On page 3, I just understand L1 to represent “L” but with splay faults. So I would assume that for each L-scenario, there is also an L1-scenario with the same slip model but with splay faults? This stuff is probably obvious to you who have worked with this for years but it isn’t to me and almost certainly not to the average Seismica reader.

Response: I will make it clearer in the paper. L1 and XL1 in the figure are the average inundation depth for all inundated cells from the respective models. The violin plots show the the distribution of these values for the ensembles, and the lines are showing the average values from the L1 and XL1 scenarios. L-class ruptures are those ensembles who fall into the magnitude bin prescribed by Witter et al. (2011). The same is true of the XL-class ruptures.

In Figure 7 at around -124.095E and 46.565N there is a patch that is inundated for the L-scenarios (panel c) and yet not inundated for the XL-scenarios (panel d) – despite the surrounding flow depths being far greater in panel (d). Why and how is this? Is it to do with co-seismic slip (i.e. this region is being raised in the scenarios to the right? Or is it a plotting error (e.g. NaN values that are not colored?).

Response: The average deformation has these cells being below mean high water for the XL-class ruptures (i.e., 0 m), so they are shaded as being part of the water. I experimented with using the average deformation from the L-class ruptures, but then this does produce a plotting error. Instead, I used the non-deformed topography of the L1 and XL1 scenarios. This changes the Jaccard index slightly (more pixels to account for), but the meaning of the results are unchanged. This change is also made clear.

Figure 11. I spent a long time looking for triangles in these plots before realizing that the white lines were lots of tiny triangles. The average reader may have better eyesight than I do. Is there no ASCE inundation limit for Ocean Shores? The white “line” dominates the black line greatly. I do not know if this is the intention. (I am fully aware of the difficulty of displaying multiple limits and inundation metrics on the same plots – I have done this in several papers myself and I am almost never satisfied with the final product.)

Response: The data itself are points. Plotting them as lines was tried, but the final product is even less visually accessible. I have tried making them bigger, but the same issue still arises. Worse yet, some of the points overlap with each other, so making a distinct line product is difficult. I have made the black lines, which are the evacuation lines, bigger to compensate, but I must admit this is an imperfect solution. The white triangles were changed to goldenrod circles to aid with contrast issues.

Page 21: “however, we maintain that it must be constant throughout our simulations at 0.025” (again confusion between constant throughout a simulation - implying a time-dependent variable – and spatially non-uniform). The formulation of this sentence suggests “We believe that it has to be this way” rather than “We made a pragmatic decision to make it so”. Again, no mention of the supplement in which the opposite seems to be done!

Response: The supplement has been removed, and this made clear that it is spatially uniform.

In the “Data and code availability” section it says “Code available on request” (which always makes the reader’s heart sink!). Can you specify what code we are talking about here? In Section 2.2, it sounds as though we are using geoclaw – which is of course open software. So it wouldn’t hurt to provide the URL and version number of the geoclaw used. Would it be possible to put the source specifications in setrun.py files on a repo somewhere (figshare etc.?) It would be useful to the community to understand the setup. (How is non-uniform Manning implemented?)

Response: Yes, I have added these fixes. I have gone the way of doing “Code available on request” for papers due to pre-prints. The necessary code set ups are now on github and DOI with Zenodo.

I miss a reference to Davies (2019) – DOI 10.1093/gji/ggz260 which I think is quite a pivotal paper in demonstrating the increased impact of tsunamis from heterogeneous slip models.

Response: I had not read Gareth’s paper until this past boreal summer (2025). It has been added in to the paper. Thanks!

Supplement:

2 hours of model time? It was 4 hours in the main paper.

Put the (~5 m) equivalence at the first occurrence of “arcsec” not the second.

In Figure S1, could you provide a map of how the Manning coefficient is distributed? The differences between the inundation do not really make sense without one.

I see that we have a binary contrast between inundation and not inundation. In DOI 10.3389/feart.2021.757618

we ran inundation calculations at multiple (uniform) values of Manning and found that while the inundation extent changed “a bit”, the most dramatic change was in the velocities and momentum flux. These metrics are not considered here, and that is ok. But it means that the friction representation may change the estimated impact of a tsunami far more than the inundation depth alone does.

I would not be against simply removing the supplement and doing a more thorough study of Manning friction in a new paper. The main paper does not mention the supplement and implies that what is in the supplement can’t be done. So I think for the sake of consistency, the easiest remedy is to remove it now and then do a first class study of this in the next manuscript?

Response: Yes, you are right. It might be best to approach this topic as its own standalone paper. However, given the timing of doing the spatial domain varying Manning coefficients and my lack of funding to do so, this might never be touched by me again. Newport is the easiest case study to do because the land-use can be estimated as a function of height above mean high water, which is an imperfect assumption but functional given the lack of digital twin for the area at the time this was performed. With the variations, 2 hours was chosen as the model run time because of the prevalence for numerical instabilities if performed in GeoClaw at the same duration of the parent manuscript. TUNAMI may be a more preferential model to use for these cases. HySEA lacks the feature. And, COMCOT has not tried this, but will be unsuited for the amount of simulations required for PTHA.

Minor notes:

Page 3 (top): Tthese (typo)

Page 5: antincipated (typo)

Page 5: There exist(s) ... hypotheses ...

Page 21: typo – space before “One of ...”