Author's Overall Rebuttal

I thank the AE and two reviewers for comments. Responses to them are in **blue**; no new analyses were carried out, the changes are quite minor mostly amounting to clarifying sentences, captions, and stylistic issues. Figures were modified to add bounding boxes, tick marks, and increase spacing between panels as requested.

Associate editor comments

L13: I realize that you are the sold author of this manuscript but consider using we.

As per discussion with AE and Editor left as "I" throughout.

L25: A word is missing: breaks? moves along?

Added text to clarify

L30: Consider rewording.

Reworded

FIG1: Consider adding a bounding box to the figure and also some tick marks for improved readability.

Done

L58: tsunami hazard

Modified as requested

FIG2: Consider adding a bounding box to the map or thickening the line that may already be there to the same width as the lat/lon tick marks.

Done

FIG2: Presumably pink triangles are real tide gauges and yellow triangles are virtual tide gauges. Consider mentioning this explicitly in the figure caption.

Added clarifying text

FIG2 Caption: s

Fixed typo

L49: shown

Added

FIG3: Please see my comments on Figure 1 and consider doing the same here. Also, move the two figures a bit further apart so the "Fault width (km)" label isn't so close to the left panel.

Done

L186: . For

Modified text to be clearer

L188: Kind of awkward. Maybe break into two sentences?

Modified sentence

FIG5 Caption: .

Fixed typo

FIG6 Nice illustration but please include a bounding box and tick marks if possible.

Done

FIG7 Caption: Does red represent east to west or west to east or the model with the maximum difference? Why not show all 3 curves like in Figure 6. Finally, please add bounding boxes and tick marks to each plot (if possible).

Added WE,EW, bi, according to which direction it represents and added bounding box and ticks

L195: Do any stations show no amplitude difference?

Yes! These are shown in Figures 8 and 9 and that is discussed when describing those figures

L213: Perhaps I missed it but I don't think you define this acronym anywhere.

Added definition

L218: Please reword for grammar and clarity.

Re-worded

L227: sources

Added in

L232: Consider breaking into two sentences.

Done

FIG 10: See previous comments regarding bounding boxes and ticks marks. Also, why are the subplot so small? Consider making them larger for easier viewing, so that you can align vertical axis labels, and so that tide gauge labels don't go off the page or overlap the model output, etc.

Done

FIG10 Caption: Modify caption since you're showing many more locations that just Hilo.

Done

L273: e

Fixed typo

L293: Please define what it is for clarity.

Modified wording

L295: just

Fixed typo

L306: this

Changed

Reviewer 1 Comments

The author investigates how time-dependent earthquake rupture affects tsunami waves recorded in the far-field. Generic tsunami modeling often neglects the rupture duration, and thus rupture directivity, instead assumes an instantaneous source. While this may be valid for small to moderate-sized earthquakes, the author argues that tsunami amplitudes and arrival times significantly change when rupture kinematics are taken into account for large (>Mw 9.0) earthquakes. Melgar uses synthetic earthquake scenarios along the Alaskan subduction zone, predominantly with homogeneous slip, and observes up to 30% larger tsunami amplitudes at distant sites, with rupture directivity playing an important role for the tsunami radiation pattern. The results affect probabilistic tsunami hazard analysis,
where most stations considered experience a higher probability of exceedance for a given return period and tsunami amplitude when rupture kinematics are taken into account compared to a static source.

The manuscript is well-prepared and presents useful information clearly and appropriately. Minor remarks regarding some of the figures are noted at the end of the document. I have listed several comments below, not as major concerns, but as suggestions to improve clarity and fairness in the descriptions. I hope these are helpful for the author for the revision of the manuscript, which I find suited for the audience of Seismica.

Major Comments

1. I'd like the author to be more specific about distances when speaking about increased tsunami amplitudes in the far-field. When should we start worrying that current static approaches are not valid anymore for the given magnitudes? Or in other words, for the near-field, when do effects of rupture kinematics break down and a static approach is acceptable?

For example, a similar classification as done by the PTWC for tsunami warning might help, see e.g., Hirshorn et al. (2020).

Major comments #1, #2, and #3 are interrelated so I am responding to them jointly below.

2. The manuscript emphasizes that kinematic effects grow with moment magnitude, but the
transition from negligible to significant remains qualitative. Is there a threshold value (e.g.,
>Mw 8.5) or a scaling curve that quantifies this effect across magnitude bin? This could also
inform exactly when the added complexity of kinematic modeling becomes essential.

Major comments #1, #2, and #3 are interrelated so I am responding to them jointly below.

3. The author argues that a ~30% increase in runtime is justifiable, yet this may be non-trivial for operational PTHA workflows. Could the author recommend strategies for selectively incorporating kinematics (e.g., only for ruptures >Mw 8.5 or >1000 km in length)? This could
help guide modelers who need to prioritize resources.

Addressing major comments #1, #2, and #3, I agree with the spirit of your comment, that we should be more explicit about when kinematic effects become operationally important. In the revised manuscript I added a practical paragraph that frames the issue using PTWC-style distance bands (local ≤100 km, regional ≤1000 km, teletsunami >1000 km; citing Hirshorn et al.) to guide where rupture duration and directivity most strongly affect far-field amplitudes. I also make explicit that, while kinematic runs can be ~30% slower in CPU wall time, this cost is not a sufficient reason to neglect an effect that meaningfully shapes hazard. At the same time, because there is a continuum of rupture sizes, lengths, speeds, and source-to-site geometries, and bathymetry can focus/defocus energy, there is no single sharp threshold. I therefore offer loose, actionable guidance: if compute permits, include kinematics universally; if resources are constrained, prioritize kinematics for great events (≈Mw ≥ 8.5) and teletsunami distances. I close by noting that this trade-off should ease over time as computational resources improve and GPU-enabled tsunami codes continue to mature, making routine inclusion of kinematics increasingly feasible.

See new final paragraph in the last section of the discussion.

4. This study reminds me of the recent work by Sementsov et al. (2025), who show that in 10 out of 16 real tsunami earthquakes, the energy of the tsunami excited by the kinematic source is greater than that excited by the static source and that the kinematic source causes a spatial redistribution of tsunami amplitudes and a notable amplification of the high-frequency component in the time-series of tsunami height. How do your results relate to their study?

Great find, I was not aware of this paper. I added discussion of it to the introduction:

"Meanwhile Sementsov et al. (2025) found by numerical analysis of 16 large events (1992–2021), that, at distances of a few source lengths from the event, 10/16 earthquakes showed greater tsunami energy for kinematic than static sources (up to ~9%)"

And to the discussion:

"Ultimately the importance of this effect will become clearer as more events are modeled and observed carefully. Sementsov et al. (2025) already found, from a retrospective analysis of large events, that there is a modest increase in amplitude when considering rupture kinematics. New open ocean observations for large events, such as those from satellite altimetry, for example for the recent 400 km long M 8.8 Kamchatka earthquake (Ruiz-Angulo et al., 2025), or from fiber optics (Taha et al., 2021) will help to elucidate this."

Minor Comments

Line 21: Tsunamis are large surges of sea water caused by undersea earthquakes. Even though this is in the non-technical summary, other sources can generate tsunami, too. Suggest to add "commonly" (or rephrase).

Modified as requested.

Lines 36f: the 2004 M9.2 Sumatra, Indonesia and the 2011 M9.0 Tohoku, Japan, earthquakes It would make sense to introduce the moment magnitude here, otherwise "M" may be too ambiguous.

As this is a seismology journal I feel reasonably confident readers will understand magnitude scales

Line 37f: far-reaching effects

Suggest to replace effects with “impacts”.

Modified as requested

Line 37: sobering
While this is true and may be worded this way in a news article, I would refrain from using such strong emotional wording here.

The reviewer and I will disagree on this. We are talking about events that kill people by the tens of thousands. Nothing wrong with that being sobering.

Line 44: maximum credible tsunami
Introduce the abbreviation MCE here? See comment for Line 213.

Yes, thank you, added in.

Line 54: By way of illustration
Wordy?

Yes, removed

Line 67-70: …Indonesia (Sørensen et al., 2012), the Mediterranean (Horspool et al., 2014)...
It seems the references of Sørensen et al. (2012) → Mediterranean and Horspool et al. (2014) →Indonesia are switched. I recommend carefully checking these lines again.

Corrected

Line 71f: we typically distinguish between near-field sources which are close to the site of interest and far-field sources which are further afield, sometimes an entire ocean basin away Could the author maybe introduce distance ranges here? See moderate comment 1.

I added the distinction from the Hirshorn et al paper in this sentence.

Line 88: ahzard
Typo.

Fixed

Lines 99ff: Indeed, a numerical study by Williamson et al. (2019) found that in the near field there is less than a 1% difference in tsunami amplitudes resulting from considering instantaneous vs. time-dependent crustal deformation associated with the earthquake. For which magnitudes was this conclusion reached? Please add (and see moderate comment 3).

Added magnitude range

Line 105: special class labeled "tsunami earthquakes"
This sentence would benefit from a reference to Kanamori (1972).

Added

Lines 108-112: The second instance where rupture speed can play a significant role was identified by Williamson et al. (2019) and has to do with far-field tsunamis. There, the modeling showed that while the impacts in the near-field were negligible, in the far-field there was enough time, given the long propagation distances, for the differences to amount to meaningful variations in amplitude, especially for long, high magnitude, unilateral ruptures where the source process can take many minutes.

I feel the flow of the sentences here (in particular the 2nd sentence) could be improved for better readability.

Reworded

Line 121f: I will conclude that it cannot, and that ignoring rupture kinematics can have a measurable impact in estimated far-field tsunami amplitude that cannot be ignored.

Please add here that this conclusion is valid for large (Mw > 9.0) earthquake ruptures (it may very well differ for moderate-sized events).

Added magnitude ranges

Lines 125f: near-field effects Williamson et al. (2019)
Suggest to insert a comma between “effects” and “Williamson”.

Done

Lines 152f: spline interpolation
Which type of spline interpolation is applied?

Added “cubic”

Line 162f: (Clawpack) suite (Mandli et al., 2016; Clawpack Development Team, 2024b), to simulate tsunami generation, propagation, and inundation

While GeoClaw is able to model inundation, this study does not do any inundation modeling, so mentioning this here may be misleading. Also, make sure to unify the cited versions of GeoClaw (see comment for Line 336).

Removed “inundation

Line 166: solves the depth-averaged
Recommend to write “2d depth-averaged”.

Modified as requested

Line 177: Each tsunami model was run for a propagation time of 14 hours.

Could you add more details about the computational runtime of each model?

Added wall-time details.

Line 185f: To first order what can be seen is that the tsunami roughly increases in amplitude in the direction of rupture.

I recommend not writing “roughly”. Either support this argument with an approximate value/percentage or rephrase the sentence.

Re-worded

Line 213: MCE
This abbreviation has not been defined yet. See comment for Line 44.

Noted and introduced definition above

Line 231f: Overall, what can be observed in Figure 10 is that allowing for more realistic heterogeneous slip has a major impact, this has been clearly articulated already by Davies and Griffin (2018) and Melgar et al. (2019)

There are actually also older papers, which share the same finding. See e.g., Geist (2002) and Goda et al.(2014).

Added references

Line 272: Overall, yes
I recommend avoiding words like “yes”.

Modified as requested

Line 336: archived in Zenodo (Clawpack Development Team, 2024a)
The Clawpack Development Team is cited twice (see Line 162 and Lines 361ff):
Clawpack Development Team. Clawpack v5.11.0, 2024a.
Clawpack Development Team. Clawpack software, 2024b. http://www.clawpack.org. doi: https://doi.org/10.5281/zenodo.13376470. Version 5.11.0.

Cleaned up references

Figure 1:
Would it be possible to add tick marks to this figure, either at the probabilities of 1, 0.1, 0.01 or at those values from the example in the text (Lines 54-57)?

Done

This also applies to the semi-log/log-log plots in Figures 3, 12, 13, 14.

Done

Figure 5: Differences between the time-dependent kinematic ruptures and the static one are shown as well. Please rephrase this sentence (see moderate comment 4).

Rephrased

Figure 7: For some fo the sites the maximum difference is with the east to west rupture, for others with the west to east rupture. Please add labels to the panels and explain which panel belongs to which scenario. In lines 195f. you write sometimes that is the east to west, others the west to east, it would be nice to know which is which. Also correct typo “fo”.

Corrected typos and added “EW” or “WE” according to which rupture is represented by the red line

Figure 8:
Could the author add to the figure description that the part of the fault assumed to have ruptured is colored in pink?

Done

Figure 9:
I assume the part of the fault, which ruptures here, is shown in blue? Please add this to the figure description as well.

Done

Figure 11B:
I am not sure I understand what happened to the violin plot for a distance of 450 km. Could you please explain why it’s so narrow compared to the other violin plots?

Great question, I dug into this a bit, it’s the luck of the stochastic draw so to speak, there just were not a lot of sources in that specific bin making it look awkward

Figure 12:
The figure description is rather short. As a suggestion, maybe add another sentence on how exactly the tapered magnitude frequency distribution is constructed.

Done. Expanded the caption.

References

- *Geist, E. L. (2002). Complex earthquake rupture and local tsunamis. Journal of Geophysical Research: Solid Earth,107(B5), ESE 2-1-ESE 2-15. https://doi.org/10.1029/2000JB000139*
- *Goda, K., Mai, P. M., Yasuda, T., & Mori, N. (2014). Sensitivity of tsunami wave profiles and inundation simulations to earthquake slip and fault geometry for the 2011 Tohoku earthquake. Earth, Planets and Space, 66(1), 105. https://doi.org/10.1186/1880-5981-66-105*
- *Hirshorn, B., Weinstein, S., Wang, D., Koyanagi, K., Becker, N., & McCreery, C. (2020). Earthquake Source Parameters, Rapid Estimates for Tsunami Forecasts and Warnings. In R. A. Meyers (Ed.), Encyclopedia of Complexity and Systems Science (pp. 1–35). Springer. https://doi.org/10.1007/978-3-642-27737-5_160-2*
- *Horspool, N., Pranantyo, I., Griffin, J., Latief, H., Natawidjaja, D. H., Kongko, W., Cipta, A., Bustaman, B.,Anugrah, S. D., & Thio, H. K. (2014). A probabilistic tsunami hazard assessment for Indonesia. Natural Hazards and Earth System Sciences, 14(11), 3105–3122. https://doi.org/10.5194/nhess-14-3105-2014*
- *Kanamori, H. (1972). Mechanism of tsunami earthquakes. Physics of the Earth and Planetary Interiors, 6(5), 346–359. https://doi.org/10.1016/0031-9201(72)90058-1*
- *Sementsov, K. A., Baba, T., Kolesov, S. V., Tanioka, Y., & Nosov, M. A. (2025). The effect of earthquake fault rupture kinematics on tsunami generation: A numerical study of real events. Geophysical Journal International, 240(2), 920–941. https://doi.org/10.1093/gji/ggae413*
- *Sørensen, M. B., Spada, M., Babeyko, A., Wiemer, S., & Grünthal, G. (2012). Probabilistic tsunami hazard in the Mediterranean Sea. Journal of Geophysical Research: Solid Earth, 117(1).https://doi.org/10.1029/2010JB008169*

Reviewer 2 comments

The author conducted tsunami numerical experiments to investigate the effect of kinematic rupture propagation on far-field tsunami amplitudes around the Pacific Ocean using the earthquakes from the Alaska subduction zone. The author found out the rupture directivity rotates the tsunami radiation pattern. The resulting computed far-field tsunami amplitudes from the kinematic deformation initial condition could be over 30% larger than those from the static deformation initial condition.

The effect of kinematic rupture on far-field tsunami amplitudes is an interesting topic. However, a crucial factor of this study is the tsunami numerical model's capability to model tsunami generation from kinematic seafloor deformation. González et al. (2011) and Arcos and LeVeque (2015) don't validate GeoClaw's kinematic seafloor deformation initial condition. Hence, the author needs to validate GeoClaw's capability to model tsunami generation from kinematic rupture (Saito and Furumura, 2009).

Also, the author didn't conduct a sufficient literature review about essential factors of earthquake, seafloor deformation, and tsunami generation. There are more important factors increasing tsunami amplitude than the kinematics seafloor deformation. For example, lower rigidity near trench (Bilek and Lay, 1999), the horizontal deformation over the steep bathymetry (Tanioka and Satake, 1996), and the accretionary wedge near the trench (Tanioka and Seno, 2001). These factors could increase tsunami amplitude more than the kinematic rupture propagation along the trench.

The author tried to create realistic earthquake source model, but ignore the basic characteristics of earthquakes. There should be major slip region around the hypocenter. Therefore, the location of hypocenters ,Hwe and Hew, cannot be at the west and east ends of the fault model.

Also, the major slip region doesn't exist around the hypocenter for the heterogeneous stochastic slip M9.37 model, FIG10(B).

I recommend rejection for this paper because GeoClaw's kinematic seafloor deformation initial condition has not been validated, and the effects of varying rigidity, horizontal deformation, and accretionary wedge on rupture and tsunami generation have not been considered or discussed. In addition, the fault models used in this study don't capture basic earthquake characteristics.

The above review seems to indicate a response to the following 4 issues is necessary

1. Assumptions in kinematic source generation in GeoClaw
2. Completeness of the literature and relative importance of other amplifiers, especially on heterogenous Earth structure (rigidity) at the source
3. Source realism with respect to the hypocenter and slip distribution around the hypocenter.

Issue #1:

If the reviewer is referring to the horizontal advection of sloping topography and how that introduces an "extra" source term then he or she is correct that this is important. I do in fact include this and myself have carried research on this issue (see Melgar & Bock, 2015). Please see expanded methods where I reference the original finding of

Tanioka & Satake (1996) and my implementation for including advected sloping bathymetry.

If the reviewer is referencing the postulate of Song (2008) who hypothesized that horizontal momentum contributes significantly to tsunamigenesis then we refer him ro her to the work of Lotto et al. (2017) who tested Song's "horizontal impulse" hypothesis with full-physics Earth–ocean simulations and parallel shallow-water runs. They fond that while horizontal seafloor motion over a slope does transfer substantial horizontal momentum to the ocean, almost all of it leaves as ocean acoustic waves, contributing negligibly to the tsunami's initial depth-averaged velocity. It is not a meaningful source term and thus I do not include it here.

Lotto, G. C., Nava, G., & Dunham, E. M. (2017). Should tsunami simulations include a nonzero initial horizontal velocity?. Earth, Planets and Space, 69(1), 117.

<u>Issue #2:</u>

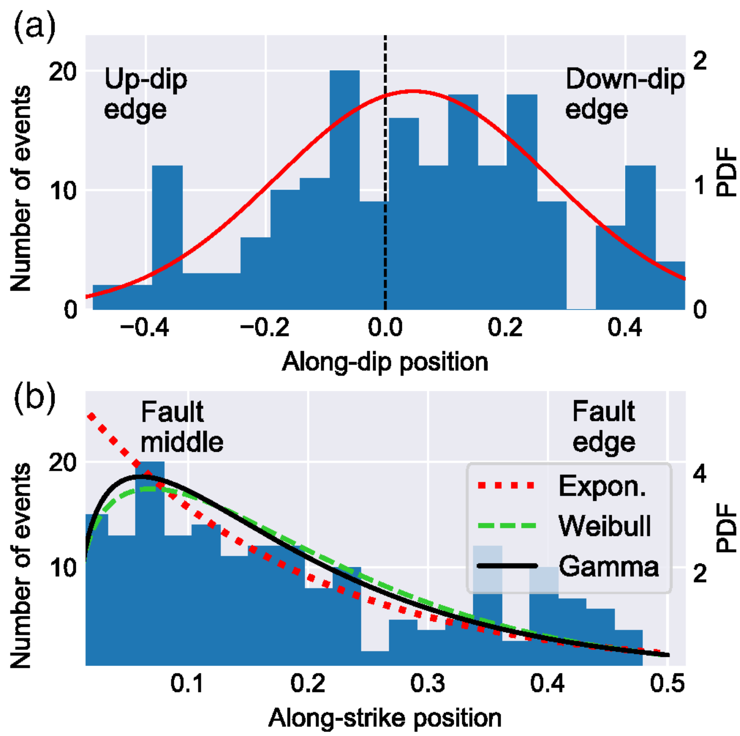
The reviewer is of course correct that depth-varying rigidity can have a profound effect on tsunamigenesis. However we note that we are trying to isolate the effect of one variable at a time, in this case rupture kinematics, so adding extra confounding variables makes it difficult to isolate/separate the effects. To acknowledge the importance of this I have added the following passage to the discussion:

The computed sources do not consider depth-varying rigidity Bilek & Lay (1999). This is an important extra variable that can contribute to increased complexity in tsunamigenesis. If slip extends to shallow low-rigidity materials, coseismic deformation can increase significantly with the attending increase in the resulting tsunami amplitudes (Saito & Furumura, 2009). In these materials rupture slows down even more (Riquelme et al., 2021) approaching tsunami propagation speeds making the kinematic effect even more pronounced. Further, higher-order mechanics such as plasticity can augment vertical coseismic deformation even more (Wilson & Ma; 2021). Full consideration of these complexities is becoming increasingly necessary.

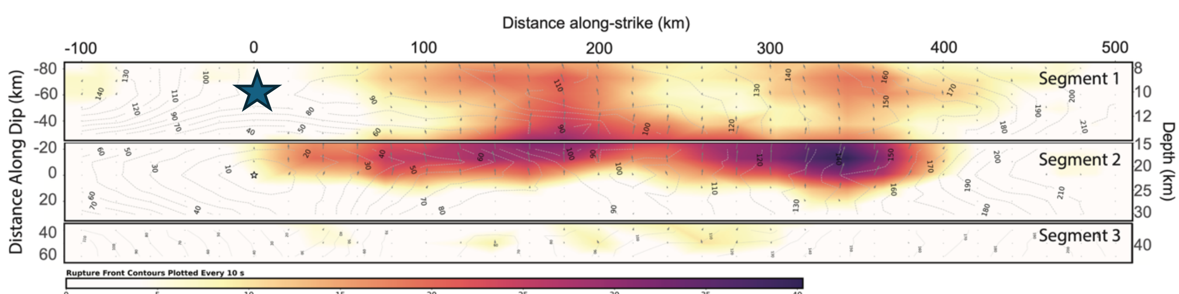
<u>Issue #3</u>

I am unaware of literature showing that slip at the hypocenter has to be very high so I cannot address this part of the critique.

As for the issue of rupture starting at the edges of the source region being unrealistic; this has been addressed by Melgar & Hayes (2019). There, we carried out a systematic review of slip models for large earthquakes worldwide and found that, yes, nucleation at the edges is less likely, but it still occurs, here is the relevant figure from that paper:



Panel (a) shows the up or down-dip positions of hypocenters, 0 is the middle of the fault and +/-0.5 is the up/down-dip edge. More relevant to the reviewer's comment, panel (b) shows the same for the along-strike position, 0 is the middle, once more, and 0.5 is either edge. Note that events nucleating right at the fault edges, while rarer, still occur. Look no further than the recent M8.8 Kamchatka earthquake whose hypocenter (star symbol) is at the extreme edge of the event:



See: Melgar, D., & Hayes, G. P. (2019). The correlation lengths and hypocentral positions of great earthquakes. *Bulletin of the Seismological Society of America*, *109*(6), 2582-2593.

Reference

- Saito, T. and Furumura, T. (2009). Three-dimensional tsunami generation simulation due to sea-bottom deformation and its interpretation based on the linear theory. Geophysical Journal International, 178, 877-888.

- Tanioka, Y., and Satake, K. (1996). Tsunami generation by horizontal displacement of ocean bottom. Geophysical Research Letters, 23(8), 861-864.
- Tanioka, Y., and Seno, T. (2001). Detailed analysis of tsunami waveforms generated by the 1946 Aleutian tsunami earthquake. Natural Hazards and Earth System Sciences, 1, 171-175.
- Bilek, S. L. and Lay, T. (1999) Rigidity variations with depth along interplate megathrust faults in subduction zones. Nature, 400, 443-446.

Reviewer 3 comments

The manuscript submitted by Diego Melgar presents a compelling and well-described study on the impact of rupture kinematics, specially rupture directivity and duration, on far-field tsunami hazard assessment. The author uses a large suite of synthetic earthquake scenarios along the Alaska Subduction Zone to demonstrate that the commonly-used assumption of instantaneous rupture may lead to underestimation of tsunami amplitudes at distant coastal sites, particularly for mega-earthquake events with magnitudes greater than 9. The presented work is technically correct, mythologically rigorous, and highly practical to hazard mitigation efforts. The findings have direct implications for PTHA frameworks and coastal hazards planning.

I have several concerns regarding the model grid resolution and the energy conservation in hydrodynamic modeling that probably need more clarification. My overall recommendation is acceptance with minor revisions, contingent on the points outlined below.

For earthquake of equal magnitude, the total energy transferred from the earthquake to the ocean should, in principle, be consistent between static and kinematic ruptures. In Figure 5, the WE-static comparison shows a widespread decrease in tsunami amplitudes with only narrow bins of increase. In contract, the EW-static comparison shows a more pronounced increase. While these directional effects are qualitatively reasonable, the manuscript could benefit from addressing:

1) Is the total tsunami energy conserved across the static and kinematic simulations?

Yes it is. The total final coseismic deformation is the same in both kinematic and static cases and since the tsunami potential energy, E, available to be used for wave propagation is given by the closed surface integral of the intial sea surface disturbance η over the two spatial dimensions:

$$E = \frac{\rho g}{2} \oiint \eta^2 dx dy$$

Where ρ and g are seawater density and gravity, then that energy is the same in both cases. I've added this sentence in Section 3.1:

"This a purely geometric effect, the total tsunami potential energy available for wave propagation is controlled by the final coseismic deformation (Nosov et al., 2014; Melgar et al., 2019) is the same in both instantaneous and kinematic cases"

2) Can the observed amplitude differences be also attributed to numerical artifacts due to coarse grid resolutions?

Worth considering and I perceive this as unlikely. Both the static and kinematic cases used identical AMR settings (same base grid, refinement ratios, flagging/regions, and regridding cadence), so any numerical artifact would affect them equally. GeoClaw's conservative coarse–fine synchronization further minimizes resolution bias; the amplitude differences therefore most liekly reflect physics, not numerics.

The manuscript states that 3 AMR levels of grids were used in the model simulations, with the coarsest grid at 10-arc-min resolution. Please clarify:

1) where and how wee these AMR levels applied spatially?

Here I clarify both the *forced* and the *dynamic* use of AMR. At the source, I *required* refinement to the finest level (Level 3) over a polygon that encloses the initial seafloor-deformation footprint for at least the rupture duration T_{rup}(with a small post-rupture buffer). This is implemented in GeoClaw via time-dependent "regions" that enforce a minimum/maximum level within user-specified boxes and time windows; such regions are expressly designed to require refinement before a wave arrives and can vary with time.

Away from the source, refinement is *dynamic* and follows GeoClaw's standard AMR workflow: cells are flagged based on tsunami indicators (principally sea-surface elevation relative to still water, which is non-zero only in the wave), flagged cells are clustered into rectangular patches, and finer patches are initialized by copying or interpolation from coarser levels. Regridding is performed every few time steps with a 2–3 cell buffer around fine patches so the mesh "moves" with the wave while limiting unnecessary refinement. At each level, the code advances (in time) the coarser grid, interpolates ghost-cell data to finer grids, advances finer grids with smaller time steps according to the refinement ratios, and then averages/refluxes to maintain conservation across coarse–fine interfaces; this integration strategy is applied recursively through all levels.

The relevant paragraph in the methods now reads:

“GeoClaw incorporates adaptive mesh refinement (AMR), which dynamically increases computational resolution in regions of interest such as along inundation zones, details of the implementation are in LeVeque et al. (2011). Near the tsunami source I force the highest level of refinement to be employed for at least the duration of the rupture. Three AMR levels are used, the coarsest grid has 10 arcmin resolution, the intermediate grid 2 arcmin to match the ETOPO2 data…”

2) Does the 10-arc-min grid converge evolve dynamically with the tsunami wave propagation?

No. The 10-arc-min grid is the *fixed* coarsest background mesh. What evolves is the AMR hierarchy: refined patches (Levels 2–3) are created/destroyed as the wave propagates via flag–cluster–regrid every few time steps, and we enforce Level-3 at the source during the rupture window.

3) Could the kinematic rupture direction influence the grid resolution, and if so, does it affect energy conservation?

Not in the implementation I have here because, as noted above, I force AMR level 3 at the source for the duration of rupture.

More information is needed regarding the virtual tide gauges used to valuate tsunami amplitudes. Are these gauges located offshore, nearshore, or in harbors? What is the range of the water depth at these virtual gauge locations?

Tide gauges are placed 1 pixel offshore in the finest grid (level 3), I then use Green’s law to homogenize their amplitudes to a common depth of 5 m. I added this small note to the methods:

“For every model output is collected at "virtual" tide gauges (locations are in Fig. 2), each gauge is placed 1 pixel offshore and its amplitude corrected in post-processing using Green's law to a common depth of 5 m”

The manuscript discusses the implication of rupture kinematics for future PTHA and deterministic scenario development. It will be valuable to provide some thoughts on how the rupture onset time and directivity could be implemented into real-time tsunami source characterization, especially in the context of early warning systems.

It is not the purpose of this manuscript to discuss early warning/forecasting so I’ve deliberately refrained from commenting extensively on that. I’ve thoughts but that is such a different implementation problem that I’ve simply added this tot eh final paragraph for now:

“In particular, for tsunami warning as rapid, more accurate, forecasts become desirable it will be important to consider the effect at teletsunami distances.”