Response to Reviewers

Dear Seismica Editors,

Thank you for the opportunity to submit a revised draft of the manuscript, “Performance of Slab Geometry Constraints on Rapid Geodetic Slip Models, Tsunami Amplitude, and Inundation Estimates in Cascadia". We appreciate the time and effort that the editors and reviewers put into improving the manuscript and to consider it for publication. The comments and feedback from the reviewers were insightful. We incorporated most of the changes from the reviewers. Also, we believe the revisions reflect a more concise and organized manuscript of our work. A summary of major changes is provided in the paragraph below.

We agree with both reviewers that section **3.4 Influence of Coastline Shape on Tsunami Hazard** seems disconnected with the rest of the sections. This entire section has now been removed. Another significant comment from both reviewers was suggesting more information on why the fault model with two sub-faults and 100-rectangular patches was chosen as the representative GFAST model for the Cascadia subduction zone region. In the methods section, we describe that the two sub-faults provide a rough geometrical approximation and location in reference to the Slab2 model. We inserted details in the method section about the inversion and regularization constraints needing the dislocation patches to be rectangular and uniform in size. We support with our results that the “slab model” with 100 fault patches results in “coarse” finite-fault models in GFAST that are able to resolve heterogenous slip characteristics in a rapid computational manner.

Provided below is a point-by-point response (**in blue**) to the reviewers comments (in black).

Sincerely,

Kevin Kwong

Reviewer A:

I have few comments throughout the manuscript that may make the manuscript a better read and potentially a better reference. I suggest addressing those before the publication. Most of my comments are of editorial nature, but some may need a bit more explanation.

1. I also have one general comment, the issue that really caught my attention right away (hence, may be a question for many potential readers): why was such a rough two-rectangle geometry was chosen for the sub-fault patches in the study. I didn’t find a good explanation of that choice in the text (I may have missed it, admittedly). It appears that the authors have full freedom to choose a geometry that is more conformal to the subduction line, which may have improved the results for the pre-defined fault inversions. It would have been especially relevant for the tsunami warning operations, since they already use the database of sub fault that are similar in sizes to the sub-faults used in this study, but more aligned with the geometry of the subduction. This may be beyond the scope of this manuscript, and may be the subject of future studies, but it may be worth elaborating more about this choice of the pre-defined fault geometry.

Author response: The two-rectangle geometry was chosen as an adequate representation of two fault planes that span within the area of the subduction

megathrust. In Figure 1 (b-i), we show that the two rectangular sub-fault areas roughly align with the geometry and extent of the megathrust portion in reference to the Slab2 model (shown as purple contour lines). In order to have rectangular sub-faults that conform to the curvature of the slab at depth would require variable area sub-faults, which will require changes to the regularization equation within GFAST and would not provide much additional information since the resolution is less at depth.

2. Line 49 and elsewhere: I don't think this use of "*a priori*" is appropriate in this context. The phrase is usually used to relate to something deduced from pure theoretical knowledge. I don't thinks it should be applied to the catalogs of the sub-faults, which is deduced from actual observations (in that case "a posteriori" may need to be used, but also not fully appropriate here). I would either delete this word, or may use just "prior" as in "prior knowledge" in some instances.

Author response: We agree and removed the phrase "a priori".

3. Line 99: "…simulated displacement data modeled after the GNSS network…" — I don't think I understand this. Should this be "at the GNSS network locations"? Please re-phrase the sentence to clarify the meaning.

Author response: We agree, the line is now "simulated displacement data at the GNSS network locations"

4. Line 137: "And finally, we look into tsunami inundation predictions at tide gages…" — this should be either "amplitude predictions at tide gages" or "inundation predictions at areas near tide gages"

Author response: We modified the sentence to "We look into run-up and amplitude predictions at tide gage locations".

5. Line 166: "…waveform data modeled from locations of 434 regionally located GNSS stations…" — should this be "at locations"? Please clarify

Author response: Yes, we changed it to "at locations".

6. Line 200: "We tested how "true" slip from the FakeQuakes normalized under the rectangular fault sub-patch areas…" — I am not sure I fully understand what "normalize" mean in this context. Should it be "approximated by"? Please clarify

Author response: To clarify we changed "normalized" to "distributed".

7. Line 206: "We compared this Mw from the FakeQuakes…" — this should be "We compared this Mw with the FakeQuakes…"

Author response: Done.

8. Line 207: Insert comma to read "…match with the moment from the simulations, so there is no magnitude bias in our tsunami…"

Author response: Done.

9. Line 265: The following may be a better reference here than Titov, 2009:

Titov, V.V., U. Kânoğlu, and C. Synolakis (2016): Development of MOST for real-time tsunami forecasting. J. Waterw. Port Coast. Ocean Eng., 142(6), 03116004, doi: 10.1061/(ASCE)WW.1943-5460.0000357

Author response: Thank you. We replaced the reference.

10. Line 271-272: move "on average" up in the sentence and use commas to read "…and large area, on average, tend to overestimate the moment."

Author response: Done.

11. Lines 272-274: Please rephrase the sentence. I don't think I understand what do the authors want to say here.

Author response: We changed the line "The small magnitude bias shown by directly mapping and averaging of slip onto our discrete fault models provided more confidence to include the eight models into our slip inversions" to "The moment release from the Fakequakes mapped within the areas of our discrete fault models show a small magnitude bias that we feel confident in using the eight models to resolve magnitude and slip." This line was in reference to the analysis explained in "2.2. Discretizing Slip Models".

12. Figure 2. Please explain all symbols in the figure.

Author response: Done.

13. Line 331: "The slab model with 100 rupture patches is the preferred representative…" — what is the criteria for the preference. It is not clear why is it "preferred".

Author response: We removed the word preferred but left in "representative". In this case, it is representative out of the 8 different discretizations. The number of fault patches matches the models based on the CMT approach in which we compare the heterogeneous slip distributions of three earthquake scenarios. The sentence is changed to "The resulting finite-fault model solutions we highlight here comes from the fixed slab and CMT-geometry models, both containing 100 rupture patches."

14. Line 371: I would insert "from figure 3" to be more clear: "Earthquakes A, B and C from figure 3 are translated into…"

Author response: Done.

15. Figure 5,6: Please use the same vertical scale for these figures. It would be so much more illustrative.

Author response: We decided to keep the vertical scale the way it was. The focus is to show the relative amplitude difference between the "Fakequake observation" and "GFAST model" rather than comparing the amplitudes between the three different tsunami scenarios. Also, the amplitudes of Tsunami C are larger than A and B and would seem to be taking up most of the focus if it was on the same vertical scale.

16. Chapter 3.4: This whole chapter is a bit of an "oddball" in the whole manuscript. While it is an interesting scientific observation, it seems to break the flow of the manuscript somewhat, and doesn't really contribute much to the main goal of the study. I am OK with leaving it as is, but would also urge the authors to consider the option of removing it.

Author response: We reviewed chapter 3.4 again and we agree that this section breaks the flow with the rest of the sections and seems out of place with the overall goal of the study. This entire chapter has been removed. The chapter and figure numbering is updated.

17. Line 491: "In the case of the PNW, there is currently no local tsunami warning real-time infrastructure in place that can accurately measure and model a scenario like a magnitude 9+ earthquake and tsunami." — I don't think this sentence reflects the situation correctly. At most it simplifies it too much, so it appears a bit alarmist and self-serving. It is true that the observations and modeling capabilities would be stretched to the limit for providing warning and forecast for local communities in the case of the Cascadia event. But they do exist and the warning will be issued. Even the forecast can be done in time based on just the seismic data, albeit the uncertainty of that forecasts will be very large. In fact even the real-time GNSS data is already a part of the forecast stream, albeit in experimental model. So this sentence would need to be re-phrase to reflect the more detailed situation. It would still benefit from this research.

Author response: This is a fair point. Our intended goal is to showcase the positive impact that a geodetic earthquake inversion algorithm like GFAST can bring to tsunami monitoring. We revised the sentence to "Through this study, we highlight technology such as GFAST to enhance the tsunami warning real-time infrastructure in the Pacific Northwest"

Reviewer B:

1. After reading the manuscript I am left with the feeling of several disconnected strands that haven't been satisfactorily explored and discussed to result in meaningful conclusions. I believe this perception is well reflected in the conclusions of the paper: fault catalogs should be added to GFAST instead of just relying on CMT-inferred geometry, fault catalogs should be used in other subduction zones. The latter is somewhat moot, because that seems the general approach in my understanding of the field: either estimate the geometry from the data, use some a priori geometry that makes sense for the region, or do some combination. As far as I know, this is part of the other rapid characterization tools they cite, and has been commonly done in the literature. It seems that a large number of papers in the 2010s, esp. following the 2011 Tohoku-oki earthquake, were investigating ways to simplify the time consuming aspect of geometry determination. That includes the CMT approach, but among other methods it also included use of rough discretization of known faults (e.g., Allen & Ziv 2011, Wright et al. 2012, Columbelli et al. 2013; Grapenthin et al. 2014, Minson et al. 2014). I point this out, because the discussion about these prior approaches is entirely missing.

Author response: We do reference some of the sources you cited and have added the additional references you listed, which also demonstrates that we are aware that using priori geometry information to constrain source models when available have been done in other studies. Our work here is, in part, a response to Williamson et al. (2020), where this study showed finite-fault models in GFAST for the Cascadia region using information from the CMT solution only. We make further reference in the discussion: "Williamson et al. (2020) showed that modeling large ruptures on a subduction zone using a single planar fault model can lead to improbable fault parameters. Fault geometries derived

from independent CMT solutions can result in models with locations and geometry that are not consistent to already published slab geometry models (e.g. Slab2).”

2. Something this prior work didn't delve into, and I had hoped this paper would, is assessing the level of discretization necessary to both characterize the earthquake properly and get reasonable prediction of the tsunami. Their synthetic data set would have lended itself to this very well, and they do this some, but then just seem to pick the 100 rupture patch discretization with no further justification (lines 331-332) for futher analysis that takes them to the tsunami characterization where the paper takes a rapid, and only modestly motivated turn into local hazard analysis of which the coastline shape investigation seems quite out of place and disconnected.

Author response: We selected the model with 100 rupture patches as the representative fixed geometry model to compare with the models with geometry derived from the CMT solution that is defined with 100 rupture patches as well. Further in our discussion, we concluded: “The slab discretization model with 100 rectangular patches is an appropriate fault catalog to use to model Cascadia subduction zone megathrust events. Example scenarios shown in the study highlight the ability of GFAST finite-fault inversions to resolve discrete slip asperities defined by 100 fault patches across the shallow Cascadia slab region and recreating heterogenous slip distributions that roughly resemble the synthetic models. The prescribed sub-patch size (50 x 50 km) provides adequate spatial slip variability. While GFAST is fairly computationally efficient, incorporating a finer fault grid with more fault patches would potentially cause the inversion to fall behind the real-time (i.e., each iteration takes longer than 1 second), and thus, we find the 100 fault patch model is sufficiently detailed without sacrificing real-time performance.”

3. The section about the coastline shape and its influence on tsunami hazard seems quite disconnected from the other parts of the paper. For one, I have a difficult time understanding what is done, and what the lines in figure 7 represent. I think there's some averaging and then a comparison going on that may be a correlation operation, but I can't be sure. The authors should at least revise the text for clarity. But it also seems that the point of the paper is on the impact of slab geometry on a range of things. But now they add another independent variable - coastline irregularity - whose impact they try to assess while also figuring out how CMT, fault catalog and synthetic earthquake perform. Why? If there's interest in figuring out the impact of coastline shape on tsunami hazard, this should be investigated separately and thoroughly. Their conclusion is much too broad and misleading: "an obvious correlation between coastline straightness and relative tsunami hazard is not concluded." This leaves me surprised and is much too broad for the level of analysis that has been done. At a minimum, I expect that an undulated coastline exposes more coastline to the tsunami, leaving more land area open to inundation. Also, irregular coastline will result in parts of coastline being more / less optimally oriented to the incoming waves, absolutely altering the relative tsunami hazard.

Author response: We decided to remove the section on coast line shape on relative tsunami hazard as we agree with your assessment that the analysis did not fit with the theme of the impact of slab geometry on estimating the earthquake source and tsunami hazard overall.

4. The current discussion has to be reworked. The first two paragraphs don't contribute to the discussion of the results. They are a motivation for the study and should go elsewhere. The remainder of the current discussion is predominantly on the fault geometry. The discussion on all the tsunami analyses need to be included / expanded.

One example, I give above. If the authors want to retain the current section on coastline shape, they should add a detailed discussion on which aspects of this question is covered by their analysis and which aspects are not, how their analysis could be improved or not, etc. What is learned and not learned about tsunami runup / height etc. and how is this affected by the choice of using (I think) only the 100 patch model? Wouldn't that impact the maximum sea floor displacement and thus the tsunami characteristics?

Author response: Discussion on coastline shape is removed. The discussion is focused on the capabilities and improvements in GFAST and how it can be utilized in a tsunami early warning context. Specifically we highlight the benefits of using a geodetic algorithm that generates rapid earthquake source products for NOAA's Tsunami warning system operations.

Our main points with the tsunami inundation are stated in the results section. We stated: "we showed the two different FF modeling approaches within respect to fault geometry to highlight the variability in inundation modeling results." Rather than testing the different levels of fault discretization models, our analysis is focused on comparing the approaches of constructing fault geometry constraints and how it translates to tsunami runup and amplitude results with comparison to the "true" result (i.e. fakequake models). The variability in the inundation modeling results are described. For instance, at some tige gauges, we show high accuracy as described: "at four tide gauge locations (Neah Bay, Westport, Newport, and Florence), the GFAST Slab models are capable of providing spot-on, 98-100%, predictions of the FakeQuake observations."

5. The discussion related to the fault geometry requires expansion - I don't know how the 100 patches are determined, and how transferrable this is to other subduction zones (also, shouldn't they be talking about patch area rather than numbers?). What about a discussion on changing the patch size depending on resolution / different patch shapes; there has been a lot of work on this for the non real-time cases (e.g., Barnhart & Lohman 2010) that could perhaps be picked up here with a discussion on whether this is possible / or what aspects are possible. If you are suggesting to create fault catalogs for known faults prior to a large earthquake, it seems a lot more sophistication can go into the model fault generation that what is currently possible / attempted in real time.

Author Response: Please see Reviewer B, comment #2 on full response on why we selected the model with 100 patches. Also for clarification, our intention was not to state that the optimal number of patches for putting in fault catalogs along major fault segments should be approximately 100. Our discussion is focused on the capabilities of the GFAST algorithm with the implication as we stated that, "We plan to add to GFAST the option to compute rapid geodetic source models using pre-defined global fault databases that contain location points representing a 3D mesh model of the fault. The fault databases address the difficulties in constraining fault parameters using rapid inversion techniques and operations."

Our methodologies in building a global fault catalog for GFAST has not been fully explored yet. Also in regards to patch size, the inversion approach in GFAST is limited to the use of rectangular rupture patches with uniform size dimensions which is explained in the methods section.

6. It might also be worth discussing whether the approach of focusing the analysis on modeled displacements at existing GNSS stations impacts the results? To me it seems that if we want to know what role the slab geometry plays in the inversion, using the densest possible displacement field, perhaps downsampled based on variance, would eliminate any sampling bias due to station locations. This point should certainly be discussed.

Author response: We did not systematically analyze the impact of station density and distribution on our results for this study. This could be a useful focus for a future study. In the methods section we stated that we "created synthetic displacement waveform data modeled at locations of 434 regionally located GNSS stations that encompasses networks considered useful to early warning for the Pacific Northwest such as the NOTA, PANGA, and BARD networks." We utilized the densest network of stations in the near-field range for this study.

Minor:

7. Abstract - requires rewriting for language and scope

Author response: The abstract is rewritten.

8. Figure 1 - Are they showing epicenters or hypocenters? Just surprised to see each "epicenter" plotted neatly in the center of a triangle. I wouldn't expect that if they are projected to the surface, but maybe they are sampled that way.

Author response: Both the triangle mesh and earthquake location are surface projections from the dipping fault plane. So epicenter should still be the appropriate term.

9. Figure 7 - fonts too small

Author response: Figure 7 is removed.

10. line 80 - I find the "FF" abbreviation distracting through the manuscript. Perhaps stick with finite fault, which doesn't really take that much space.

Author response: FF is replaced with finite-fault in all instances.

11. line 102: replace "focused" with "cover" - if they span "entire" Cascadia, I'd argue they are not focused.

Author response: Done.

12. Based on this last comment I would encourage the authors to take a close look at the paper and make sure it is always clear whether the synthetic earthquakes (fakequakes), their slip distributions and their displacements are discussed, or the model fault used to invert the fakequake displacements and the resulting forward modeled displacements. There are a few places in the manuscript where that distinction seems blurred.

Author response: Thank you for the suggestion.

13. Table 1 - include "patch" into headers for length and width, otherwise it seems the overall fault model gets short.

Author response: Done.

14. References:

Allen, R.M. and Ziv, A., 2011. Application of real-time GPS to earthquake early warning. Geophysical Research Letters, 38(16).

Barnhart, W.D. and Lohman, R.B., 2010. Automated fault model discretization for inversions for coseismic slip distributions. Journal of Geophysical Research: Solid Earth, 115(B10).

Colombelli, S., Allen, R.M. and Zollo, A., 2013. Application of real-time GPS to earthquake early warning in subduction and strike-slip environments. Journal of Geophysical Research: Solid Earth, 118(7), pp.3448-3461.

Grapenthin, R., Johanson, I.A. and Allen, R.M., 2014. Operational real-time GPS-enhanced earthquake early warning. Journal of Geophysical Research: Solid Earth, 119(10), pp.7944-7965.

Minson, S.E., Murray, J.R., Langbein, J.O. and Gomberg, J.S., 2014. Real-time inversions for finite fault slip models and rupture geometry based on high-rate GPS data. Journal of Geophysical Research: Solid Earth, 119(4), pp.3201-3231.

Wright, T.J., Houlié, N., Hildyard, M. and Iwabuchi, T., 2012. Real-time, reliable magnitudes for large earthquakes from 1 Hz GPS precise point positioning: The 2011 Tohoku-Oki (Japan) earthquake. Geophysical research letters, 39(12).

Author response: Thank you, we added the references above in the manuscript that weren't already cited.