Response letter, Glehman et al., “Partial ruptures governed by the complex interplay between geodetic slip deficit, rigidity, and pore fluid pressure in 3D Cascadia dynamic rupture simulations”, Seismica to handling editor Harold Tobin

We thank the Handling Editor Harold Tobin, the anonymous Reviewer A and Reviewer Diego Melgar for their constructive comments that helped improve our manuscript. In this response we address additional comments from the editor Harold Tobin.

As in the previous letter, we address all comments point by point in the following. All line numbers refer to the updated manuscript and are highlighted in blue.

We hope the revised manuscript will be well received,

Jonatan Glehman and Alice-Agnes Gabriel on behalf of all co-authors: Thomas Ulrich, Marlon Ramos, Yihe Huang and Eric Lindsey

Comment:

Figure 2: In Figure 2 and in the text, you state that you used a plate convergence velocity of 40 mm/yr. The real-world Cascadia plate convergence, according to MORVEL, varies by ~20% from south to north (from <34 mm/yr to >41 mm/yr). Does the assumption of a single value make a difference to your SDMs that would alter the results in significant ways?

Response:
Thank you for this comment. We acknowledge that the plate convergence rate in Cascadia indeed varies along strike, with MORVEL estimates ranging from less than 34 mm/yr in the south to over 41 mm/yr in the north. We added the following to lines 722-727: “In our SDMs, we adopt a representative value of convergence rate of 40~mm/yr to simplify the modeling framework and maintain consistency across the margin. Notably, a single, relatively high convergence rate provides a conservative basis for assessing slip potential. Incorporating a lower convergence rate in the southern segment would further reduce the likelihood of full-margin ruptures by decreasing the available slip budget in that region. Therefore, while beyond the scope of this study, we expect that spatially variable convergence rates may reinforce our conclusion that full-margin rupture is unlikely under current loading conditions.”

Comment

In lines 479-480: “…sustained dynamic rupture occurs only in combination with very high Pf , i.e., γ = 0.96 to 0.97. For lower pore fluid pressure, dynamic rupture propagation cannot be sustained.” I have a question about this. How dependent is this finding on your assumption of dynamic coefficient of friction? It seems intuitively like Pf and mu(d) could trade-off in terms of fault strength and ease of rupture propagation. If, for example the mu(d) was set to much less than 0.1, would self-sustaining rupture be more likely? I may not be grasping how the trade-offs work in the dynamic model however, so if this question doesn't make sense, no problem. But if it does, it may be worth addressing this point briefly either in that section or in Discussion.

Response

Thank you for this thoughtful question. We agree that there is a trade-off between pore fluid pressure (P_f) and the dynamic coefficient of friction (μ_d) in controlling dynamic fault strength and coseismic frictional strength drop which may affect potential for self-sustained rupture. In our models, we used a dynamic friction coefficient of $\mu_d = 0.1$ to maintain consistency with Ramos et al. (2021), from whom we also adopted the scaling factors (SFs) for recurrence interval calculations. This choice is further supported by Madden et al. (2022), who used similar values in comparable dynamic rupture simulations.

However, future work using rate-and-state friction will be able to account for more severe velocity weakening as observed in laboratory studies. We are adding to lines 629-633:

Specifically, while beyond the scope of this study, rate-and-state friction-based simulations may account for creeping, velocity-strengthening rate-and-state friction behavior in central Cascadia. In addition, the rate-and-state friction framework can capture severe coseismic velocity-weakening observed in both laboratory experiments and theoretical studies \citep{Noda2009Earthquake, DiToro2011Fault}, and lower dynamic friction may trade off with pore fluid pressure in governing dynamic fault strength and thus influencing the potential for margin-wide rupture."

Minor comments

Comment

Lines 50-51: "a large portion" ... instead, maybe say "nearly all" or "the vast majority"?

Response

Thank you for this suggestion. We change the phrase "a large portion" in lines 50-51 to "nearly all".

Comment

Line 54 -55: it would be worth citing Brothers et al., 2024 here, as that is a pretty major new contribution. https://doi.org/10.1130/GES02713.1

Response

Thank you for this suggestion. We added the suggested reference to lines Line 54 -55.

Comment

Line 72: Delete all commas in this sentence and add a comma after "decades."

Response

Thank you for this suggestion. We removed the commas and added a comma after "decades" in line 72.

Comment

Line 174: change to “allows incorporation of complex …”

Response

Thank you for your suggestion. We changed the phrase “allows to incorporate” to “allows incorporation of complex...” in line 174.

Comment

Line 463: Change “In distinction,” to “Distinctively,”

Response

Thank you for this suggestion. We changed “In distinction” to “Distinctively” in like 463.

Comment

Line 648: Delete parentheses around “Fig. S13.”

Response

Thank you for pointing that out. We removed the parentheses around “Fig. S13.” in line 648.

Comment

Lines 670 – 671: The reference citations need editing to put Name, year in parentheses, in all cases.

Response

Thank you for pointing that out. We updated all cited references: “...(Krenz et al., 2021) but can account for shallow velocity-strengthening behavior (Kaneko et al.,2008) and models accounting for off-fault plasticity and/or splay faulting (Ma, 2023; Biemiller et al., 2023).

Comment

Line 731: “compare our physics-based model predictions **with**, following Ramos …”

Response

Thank you for pointing that out. We changed the word “to” to “with” in line 731.

Comment

Lines 747-748: “without the requirement for a very high slip deficit rate. Included but not limited to slip or velocity strengthening friction and higher Pf . change to ”very high slip deficit rate, including but not limited to …”

Response

Thank you for your suggestion. We modified the sentence: “ without the requirement for a very high slip deficit rate. Included but not limited to slip or velocity strengthening friction and higher P_f to: “without the requirement for a very high slip deficit rate, including but not limited to slip or velocity strengthening friction and higher P_f .

Comment

Figures:
Is the map of figure 1-A properly scaled, such that the 500 km scale bar applies in both latitudinal and longitudinal dimensions? It looks distorted to my eye, but I may be wrong.

Response

Thank you for your observation. The 500 km scale bar is intended to apply to both latitudinal and longitudinal directions. For our modeling, we use a custom projection based on a spherical Earth approximation (see “Data and Code Availability”). However, we acknowledge that this projection is not exact, and the perceived distortion in the figure may result from this simplification.

Figure 1B – “exemplary” is the wrong word here – use “Example of” (also in line 384).
“interface is not intersecting…” change to “does not intersect…

Response

Thank you for your suggestion. We change the word “exemplary” to “Example of” in Figure 1b caption and also in line 384.

We again thank both reviewers and the handling editor for the detailed and constructive comments.

Response letter, Glehman et al., “Partial ruptures governed by the complex interplay between geodetic slip deficit, rigidity, and pore fluid pressure in 3D Cascadia dynamic rupture simulations”, Seismica

We thank the Handling Editor Harold Tobin, the anonymous Reviewer A and Reviewer B Diego Melgar for their constructive comments that helped improve our manuscript. In response to their comments we have added clarification, additional references and discussion points throughout the manuscript. We have added a new Discussion section focusing on implications for the 1700 A.D. megathrust rupture and a new supplementary Figure analysing ground motions. We also updated the Zenodo repository with more information.

In the following we address all comments point-by-point. All line numbers refer to the updated manuscript and highlighted in blue. New cited references are highlighted in yellow in the track-changes version.

We hope the revised manuscript will be well received,

Jonatan Glehman and Alice-Agnes Gabriel on behalf of all co-authors: Thomas Ulrich, Marlon Ramos, Yihe Huang and Eric Lindsey

Reviewer A:

I reviewed the Seismica manuscript “Partial ruptures governed by the complex interplay between geodetic slip deficit, rigidity, and pore fluid pressure in 3D Cascadia dynamic rupture simulations” by Jonatan Glehman, Alice Gabriel, Thomas Ulrich, Marlon Ramos, Yihe Huang, and Eric Lindsey

Usually I would identify myself, but because Seismica posts manuscript reviews publicly, my employer requires that I remain anonymous as an author of these review comments.

The manuscript implements the dynamic (spontaneous) rupture computational approach and investigates a range of assumptions about the initial stress conditions, pore-fluid pressure, shear modulus structure, etc., to see what their effects are on the resulting simulations of large Cascadia subduction zone earthquakes. My main requests are to explain a few things a little more clearly for the non-expert reader, and to cite more references of related work done awhile ago. In addition, I could see the text being streamlined a little bit, so as to not repeat some of the information as much, but I leave this at the discretion of the authors. Overall I found the manuscript to be pretty well written and easy to understand, and I just recommend minor revisions.

Please also see my many comments sprinkled throughout the annotated manuscript.

Here, with this review writeup, I focus on some of the main writing I’d like to see explained a bit more, or lightly changed.

Response: We thank Reviewer A for the thoughtful comments and detailed suggestions. We address all of the major comments in the following, followed by the annotations in the PDF (minor comments).

Major Comments:

Comment:
Abstract: I recommend rephrasing a few words, mostly to make things clearer for a reader. Please see the annotated manuscript version for detailed requested changes.

Response: Thank you, we have rephrased the abstract within the strict word limit (200 words). Please see our detailed responses to all minor comments below.

Comment: Section 1.3:
The term "slip deficit model" needs to be defined early in the manuscript. Also, sometimes in the manuscript they are called "geodetic slip deficit models". Anyway, I think that I know what an SDM is, because early in my earthquake career I was a geodetic modeler, but I too always want to know what the deficit is calculated relative to. Summary: Please define "slip deficit model". Perhaps a figure could be handy, e.g., for this Cascadia setting.

Response :
Thank you very much for your comment. We added a definition of "geodetic slip deficit models" in line 62-65: "Geodetic slip deficit models (SDMs) map the spatial distribution of slip deficit accumulated within plate boundaries relative to the plate convergence rate derived from geodetic observations such as GPS and Interferometric Synthetic Aperture Radar (InSAR) (e.g., Savage, 1983; Bürgman et al., 2005; McCaffrey et al., 2013; Schmalzle et al., 2014; Li et. al 2018; Johnson, 2024; Pollitz 2025)."
.

Comment:
Section 1.6: There is a lot of important information in this section, including a brief mention of where to find all of the files to make the results reproducible. The latter especially is important, so I'm thinking it might be mentioned elsewhere too?

Response :
Thank you for this comment. It is an important point however, since the paper has a designated section for "Data and code availability" we do not want to be repetitive. We have removed the sentence.

Comment:
Section 2.2: Do all of the models use the same shear modulus structure, including the slip-deficit model? The geometry is important, but I think that the shear modulus structure is important too.

Response :
Thank you for this comment. We have added clarification on the structural models used in the SDMs in line 229-231, as: ""The Gamma and Gaussian SDMs of Schmalze et al. rely on elastic

dislocation block models presented in McCaffrey et al., 2007, and McCaffrey, 2009, while the shallow-coupled model by Lindsey et al. 2021 relies on block model corrections derived by Li et al., 2018."

Comment: Regarding the slab geometry, both are pretty much based on McCrory et al., 2012, in the Cascadia region, so I'm wondering how different Slab 1.0 and Slab 2.0 are – please note the differences in the manuscript, if you haven't already.

Response: We have added a short explanation of the differences between Slab 1.0 and Slab2 in line 222-226 as:
"The main differences between the Slab1.0 and Slab2 geometries are the convexity of the slab in northern Cascadia and the length of the overall subduction zone (i.e., Slab1.0 is steeper and slightly longer than Slab2) . Moreover, the Slab2 model leveraged additional offshore and regional tomographic data to constrain the upper and deeper portions of the megathrust, respectively (Hayes et al., 2018). "
We also corrected the model name from "Slab2.0" to "Slab2" throughout the manuscript.

Comment:
In this section, please also define more rigorously "scaling factors", e.g., with a simple equation. A comment for this section is that I think that the assumption here is that the "slip deficit" is released coseismically? If yes, then maybe mention this here, that other deformation mechanisms, such as aseismic slip, etc., are not accounted for?

Response :
We added to line 233: "assuming it is entirely released co-seismically (complete stress drop)." and added an equation in line 240.

Comment:
Section 2.3:
I must have missed where the rock-density information is coming from. I think of rigidity as being the shear modulus, with the shear modulus involving knowledge of both Vs and the density.

Response :
Thank you for this comment. We added clarification to Section 2.3. The high rigidity profile corresponds to the 1D average of a 3D community velocity model of Cascadia (Stephenson et al., 2017), see also Figure 3's caption. The low-rigidity profile is proposed by Sallarès and Ranero (2019). Both the density and the wave velocities (Vs,Vp) profiles used from these papers to convert them into rigidity using the square root of the density times the shear wave velocity squared:

Lines 261-264: "We construct the low rigidity profile from density and shear wave speeds of Sallares & Ranero, (2019) in all our scenarios as ρVs^2 where ρ and Vs are the density and shear wave speed of the rock, respectively. Exceptions are model 5 and model 7 (Table S1), where we use a higher-rigidity profile based on the densities and shear wave speeds given in Ramos et. al., (2021)."

Comment:
If it's appropriate, please also note (either here or in the discussion section) that some researchers have examined what happens if there's a velocity contrast across a dipping fault, e.g., Ma and Beroza, BSSA, 2008.

Response :
Thank you for this valuable comment. We added to Section 2.3 "We use two distinct 1D depth-dependent elastic material models of the velocity structure, lines 255-256: thus, we do not account for bi-material effects (Ma & Beroza, 2008)."

And we added a comment in Discussion section "4.7 Model limitations", Lines 763-766: "Incorporating a more realistic 3D velocity model in future simulations will allow us to account for bi-material effects on rupture dynamics (Ma & Beroza, 2008) as well as the effects of sedimentary basins (e.g., Olsen, 2000; Pilz et al., 2021; Niu et al., 2025) on ground shaking. Such future work may focus on evaluating the potential of the scenarios presented here in producing realistic ground shaking estimates."

Comment:
Please cite work on subduction zones which has accounted for inelastic yielding, e.g., work by authors in this group, and also work by Ma & Nie, GRL, 2019.

Response :
Thank you very much for the suggestion. We added references to Ma & Nie, GRL, 2019 and Wilson & Ma, GRL, 2021 in section "2.3 Depth-dependent variable rigidity and 1D velocity models" to line 256-257: "Although off-fault plasticity may contribute to seafloor uplift (Ma and Nie 2019, Wilson and Shuo 2021, Ulrich et al., 2022, Wirp et al., 2024)...."

Comment:
Section 2.4:
"Please cite Andrews, 1976 or Ida, 1972 or Day, 1982 for slip-weakening. In addition to the current citations to work using slip-weakening, please also cite some older work, e.g., Guatteri and Spudich, BSSA, 2000."

Response :
Thank you for your comments. We added a reference to Day, 1982 and Guatteri and Spudich, BSSA, 2000 in Section 2.4, in lines: 278-279. We already cite Ida, 1972; Palmer et al., 1973 and Andrews, 1976 and have now added in this section as well. See also our response below.

Comment:
Section 2.6:
For the nucleation method, the best description is written in Harris et al., JGR, 2021 – it's the method we used for many of the SCEC benchmark exercises.

Response :
Thank you for this comment. We added the Harris et al., JGR, 2021 reference to section "2.6 Rupture nucleation" line 366.

Comment:
Section 3:
Empirical scaling relations are mentioned. Please add references to them in the text, before the reader discovers that the Figure 6 caption mentions one scaling relation.

Response :
Thank you very much for this suggestion. We added the proper citation in the text line 386-387: "proposed by Allen and Hayes (2017)."

Comment:
And, speaking of scaling relation, I'm wondering how the fault rupture area is calculated (this in turn relates to how the average slip is calculated).

Response :
Thank you for this comment.. We clarify in the caption of Figure 6 how we compute the rupture area as:
"The rupture area is inferred as the sum of the area of all triangular fault element faces that slip more than 1 cm."

Comment:
Section 3.6:
The term "stress shadow" is mentioned here, but I'm not sure that this stress shadow is the same as the classic stress shadow defined in Harris & Simpson, GRL, 1996 or Harris & Simpson, JGR, 1998 (or Harris, JGR, 1998). In the static stress change community, we define "stress shadow" as where the Coulomb failure stress change is negative, and thereby it is anticipated that future earthquakes might take longer to occur, because the stress needs to build up again.

I'm not sure that this is the same as the "stress shadow" phrase used in this manuscript, and because of this different idea, I'd like to either see the "stress shadow" phrase in this manuscript renamed, or at the very least, well explained. As far as I understand, "stress shadow" used in this manuscript relates to the locked portions of the megathrust effectively pinning the parts of the megathrust up-dip, so that when a large earthquake does occur, the up-dip portions are suddenly released and able to rupture. In contrast, for the traditional stress shadow (i.e., negative Coulomb stress change), the faults (or fault portions, including on the main fault itself) have had their stress decreased, making them farther from failure/slipping, and the shadowed faults/fault portions do not regain the ability to fail/slip again until tectonic strain has reaccumulated, or another nearby earthquake has elevated the stress level via an increase in the Coulomb failure stress (stress triggering).

Response:
Thank you very much for this important comment. The term "stress shadow" that we use is defined as when down-dip asperities partially or entirely immobilize the shallow part of the fault (lines 80-83). We follow previous studies naming for consistency (see Wang and Dixon, 2004; Hetland and Simons, 2010; Almeida et al., 2018; Lindsey et al., 2021). We added a clarification that this term differs from the traditional stress shadow (i.e., negative Coulomb stress change) in

lines 85-86: "This concept is not to be confused with the stress shadow describing a negative Coulomb failure stress change on the fault, as defined by Harris & Simpson 1996, 1998".

Comment:
Section 4.2:
Please add mention that the inferences for how the slip deficit model translates into the initial stress conditions also assumes that the stresses will be released seismically, rather than aseismically, or by inelastic yielding.

Response:
Thank you for this comment. We added this information to line 233 in section 2.2 "Geodetic slip deficit models (SDMs)": "assuming it is entirely released co-seismically (complete stress drop)."

Comment:
Additional reference to cite early in the main text:
Yao and Yang, EPSL, 2023. They used interseismic locking models and dynamic rupture simulations, and took them all of the way to ground motions (at least PGV), and also compared their results with the observations from a big earthquake.

Response:
Thank you for this suggestion. We added to our Introduction lines: 92-93 the following:
"Dynamic rupture scenarios using SDMs can also supplement ground motion analysis and contribute to earthquake hazard and risk assessments (Yao and Yang, 2023)."

Comment:
General question:
After a large Cascadia earthquake, postseismic slip is expected (as well as viscoelastic response). Do the locking models assume that the slip will all be released coseismically?

Response:
Thank you for your question. The locking models we use in this study do not account for heterogeneous or aseismic release of accumulated strain. Locking models alone cannot provide information regarding partial or total release of accumulated strain. For example in this study we assume total slip deficit release but we could assume that it is partial by limiting the magnitude of the slip deficit we use to convert geodetic slip deficit models into initial stresses acting on a fault. We now clarify this better throughout the manuscript and also in response to Reviewer B for example by adding a discussion point in section "4.4 Slip deficit and frictional constraints on dynamic rupture arrest" lines 732-733: "...paleoseismic data may also capture pre- and post-seismic elastic and viscoleastic deformation, potentially overestimating co-seismic subsidence."

Comment:
References:
There were some previous studies of dynamic rupture for subduction zone earthquakes, including for the Japan Trench, for Mexico, and elsewhere.

Please check if some of this older work should be cited too, e.g., work by Takeshi Mikumo and Takashi Miyatake (1970's-1990's), work by Ben Duan, and by others

Response :
Thank you for the suggestion. We have added references to the Introduction acknowledging previous dynamic rupture simulations in other subduction zones beyond Cascadia in lines: 99-102 as:
"Dynamic rupture simulations, often restricted to 2D, have been applied to subduction zones world-wide, including the Tohoku (Ide & Aochi, 2013, Huang et al., 2014, Galvez et al., 2016, Ma, 2023), Nankai (Hok et al., 2011), Sumatra (Ulrich et al., 2022), Nicoya Peninsula (Yao & Wang, 2020), Hellenic Arc (Wirp et al., 2024), Guerrero México (Li & Gabriel, 2024) and Cascadia (Ramos & Huang, 2019; Ramos et al., 2021)."

Comment:
Figures:
All of the figures look very good.

Response:
Thank you.

Comment:
Table:
The only table in the manuscript is in the supplement. I am wondering why it isn't in the main text instead. Or perhaps Seismica has guidelines requiring tables to be in the supplement?

Response:
Thank you very much for this comment. Yes, we are following Seismica guidelines that tables should preferably be put in the Supplementary material.

Minor comments, as annotated in the manuscript:

Comment:
Line 16: Please add "dynamic rupture" or "earthquake" after "Physics based".

Response:
Thank you for this suggestion. We added "dynamic rupture" after "Physics-based" in line 16.

Comment:
Line 16: "I wouldn't use the words "crucial to". Instead, how about replacing the words with "valuable for"?"

Response:
Thank you for this suggestion. We change it to "valuable for".

Comment:
Line 17: Use "but these types of simulations require" instead of "requiring".

Response:
Thank you for the suggestion. We changed this text to “but require” to remain within the 200 words limit of the abstract.

Comment:
Line 20: Change the word “on” to “for”.

Response:
Thank you for the suggestion. We changed the word “on” to “for” in line 21.

Comment:
Line 21: “this might just be me, but sometimes there are confusing uses in seismology of the terms "wide" or "width" and "long" or "length". Some readers might think that "wide" refers to the depth extent, while others might think that "wide" refers to the along-strike extent. Perhaps use another word or phrase? (optional)”.

Response:
Thank you for the suggestion. We added a short explanation (to remain within 200 words) of our definition of margin-wide to lines 21-22.
“ We find that margin-wide rupture, an earthquake that ruptures the entire length of the plate boundary, requires a large slip deficit in the central CSZ.”

Comment:
Line 26: Remove the word “the”.

Response:
Thank you for your suggestion. We removed the word “the” in line 27.

Comment:
Line 27: “?” next to recurrence time scaling factors.

Response:
Thank you for your question. We could add a reference to Section 2.2 here where we define the term recurrence time scaling factors, however, referring to specific sections is not common in the abstract. We have improved the explanation of scaling factors throughout the manuscript.

Comment:
Line 28: Change the word “on” to “for”.

Response:
Thank you for the suggestion. We changed the word “on” to “for” in line 29.

Comment:
Line 28: “I'm not sure what "structural" is referring to here.”

Response:
Thank you for this comment. When using structural, we refer to velocity and rigidity models, and we now better explain this later in the manuscript.

Comment:
Line 33: “The Cascadia subduction zone also affects Canada. The U.S. also includes Alaska, and Alaska's seismic hazard isn't dominated by Cascadia, so maybe rephrase?”

Response:
Thank you for this important comment. We have rephrased the sentence to make it less general and to include Canada in the discussion explicitly. Lines 34-35: “The Cascadia Subduction Zone (CSZ; Fig. 1a) dominates the seismic hazard in most of the northwestern United States and Canada (Petersen et al., 2002).”

Comment:
Line 35: “we don't really know this, there easily could have been M6's or M7's we haven't yet detected in the geologic record of the past few hundred years. Please rephrase.”

Comment:
Line 37: “ditto. We only really know what is has been doing in recent times, aside of that it likely didn't sneak an M9 in without our know about it.”

Response to both comments:
Thank you for this comment. In response to this comment and to a similar comment by Reviewer B, we have added a clarification to lines 39-40 as:
“Since then, the CSZ has been accumulating strain, though the paleoseismic record provides limited constraints on the occurrence of interplate seismic activity, including potential M6+ or M7+ events.”

Comment:
Line 38: "slip deficit" should be defined at its first use, here. I know what it is, because my first geodetic inversion of an active fault region was done eons ago, but other readers may not know,. Also, there are many assumptions which go into a "slip deficit" calculation.”

Response:
Thank you for this comment. We added a definition to lines 41-44: “The CSZ may have accumulated up to 15~m of slip deficit (the difference between the tectonic plate motion and actual slip along a fault) that could be released in future earthquakes (e.g., DeMets et al., 2010), however, slip deficit calculations rely on many assumptions (Wang & Dixon (2004); Almeida et al. (2018); Herman et al. (2018); Lindsey et al. (2021) some of which we discuss further in Sec. 4.1.

Comment:
Line 40: Remove the word “may”.

Response:
Thank you for this suggestion. We removed the word “may” from line 45.

Comment:
Lines 44-45: “hoping I remember what I've been taught, we don't mitigate EQ hazard, instead we mitigate the risk, unless we can figure out how to stop the EQ or the tsunami. Please change "hazard" to "risk".”

Response:
Thank you for pointing it out. We Changed the word “hazards” to “risks” in line 50. See also our reply to Reviewer B below.

Comment:
Line 46: “at the end of the sentence, perhaps add something like "rendering some features of the megathrust challenging to observe" (or similar text).”

Response:
Thank you for your suggestion. We added “rendering some features of the megathrust challenging to observe” to lines 51-52.

Comment:
Line 47: “Please add a caveat about it being among the most comprehensive for a subduction zone (if this is true, e.g., how about the record for Japan?). Or alternatively, could just omit the phrase "and is one of the most comprehensive globally", but keep the references.”

Response:
Thank you for your suggestion. We have removed this phrase from line 52 in the revised manuscript.

Comment:
Lines 50-51: “liquefaction and landslides aren't proxies, instead they're earthquake effects (if they were caused by the earthquakes, although of course landslides may have other causes).”

Response:
Thank you for the clarification. We have rephrased this sentence to better acknowledge that liquefaction and landslides are effects of earthquakes, as are turbidites caused by tsunamis. In this context, we refer to these effects as proxies because they provide indirect evidence of past earthquakes, regardless of their individual mechanisms or impacts. Lines 55-56:

“ .. and other on-land earthquake effects such as liquefaction (Takada and Atwater, 2004), and landslides (Schulz et al., 2012), which can provide indirect evidence of past earthquakes. “

Comment:
Line 56: “hopefully "slip deficit models" have already been defined, but if not yet, please do so here.”

Response :
Thank you for your comment. In addition to our earlier changes mentioned above, we added a definition to what “geodetic slip deficit models” are in line 62-65: “Geodetic slip deficit models (SDMs) map the spatial distribution of slip deficit accumulated within plate boundaries relative to

the plate convergence rate derived from geodetic observations such as GPS and Interferometric Synthetic Aperture Radar (InSAR) (Savage, 1983; Bürgman et al., 2005; McCaffrey et al., 2013; Schmalzle et al., 2014; Li et. al 2018; Johnson, 2024; Pollitz 2025)."

Comment:
Line 57: "slip deficit" has appeared twice in this sentence, which is fine, so it really needed to have been defined earlier."

Response :
Thank you for your comment. See the response to the previous comment.

Comment:
Line 58: "o.k., maybe it's just me, but I'm confused here about "coupling" vs. "locking". Or does one need a physical explanation for why it's happening and the other one doesn't?   Anyway, this is a bit confusing."

Response:
Thank you for your comment. We further clarify what "locking" means and cite additional references. We added to lines 67-69:
"We will use the term 'coupling' in a kinematic sense to describe the ratio of slip deficit to long-term slip rate. This should not be confused with the mechanical concept of 'locking', which implies knowledge of the frictional faulting behavior (Lay and Schwartz, 2004; Wang and Dixon, 2004; Almeida et al., 2018, Lindsey et. al, 2021)."

Comment:
Line 60: Change the word "on" to "of".

Response :
Thank you for the suggestion. We changed the word "on" to "of" in line 70.

Comment:
Line 60: "I'm wondering why this time-scale is mentioned. Ideally we'd hope for a longer time-scale, e.g., since the last big quake, but of course we don't have that info. from geodetic data."

Response :
Thank you for the question. In this context, the time-scale is mentioned to highlight the advantage of having a long dataset spanning decades. However, as the reviewer pointed out, this is still insufficient to fully capture longer-term processes, but it remains the best available dataset for our analysis. We have changed our previous Lines 70-72 to now read:
"From inferences of the temporal and spatial evolution of slip deficit rates, SDMs provide insights into the accumulation of strain that may contribute to future earthquakes. Geodetic data has been measured over several decades though ideally, a longer time scale, extending to the last major event, would offer a more complete assessment."

Comment:
Line 68: "Yes!   Aseismic strain release is very important in this setting. Add another reference too, just to show how important the concept is?"

Response :
Thank you for the suggestion. We added two additional references to lines 78-79 as:
"Assessing earthquake slip distributions relying solely on SDMs may overlook the potential for heterogeneous or aseismic release of accumulated strain (Kanda et al., 2012, Chlieh et al., 2014, Materna et al., 2019). "

Comment:
Line 69: "I'm not sure I'd use the word "Conversely", because the idea posed isn't in parallel with the aseismic slip idea. Instead, isn't it a debate about how the energy release is partitioned, rather than a debate about what strain is currently being stored? (optional)."

Response:
Thank you for the suggestion, we have removed the word 'Conversely,' from line 80 in the revised manuscript. Here, we contrast one end-member scenario where the slip deficit is overestimated with another where the slip rate deficit is underestimated. The reviewer is correct that the actual process likely involves a combination of both.

Comment:
Line 76: "Change "directly" to "be used to."

Response :
Thank you for the suggestion. We changed the word "directly" to "be used to" in line 89.

Comment:
Line 78: "please also cite Ma et al., *Dynamic modeling of the 2004 M_w 6.0 Parkfield, California, earthquake,* JGR, 2008. There are of course also many other, recent papers to cite too, e.g., work by Frantisek Gallovic."

Response :
Thank you for the suggestion. We added "Ma et al., 2008; Gallovič et al., 2019, 2020; Harris et al., 2021; Schliwa et al, 2024" to line 91.

Comment:
Line 80: "please also cite Yao and Yang, EPSL, 2023."

Response :
Thank you for the suggestion. We added "Yao and Yang, 2023" to line 96.

Comment:
Line 81: "were they varying the rigidity or the shear-wave velocity structure?"

Response :
Thank you for your question. Both. We have clarified this sentence in lines 96-98 to:

"have highlighted the importance of 3D variability in initial stresses, frictional behavior, rigidity or shear-wave velocity variations, or effective pore fluid pressure governing megathrust earthquake dynamics as well as the challenges in constraining these initial conditions."

Comment:
Line 95: Add “in subduction zones”.

Response :
Thank you for the suggestion. We added “in subduction zones” to this sentence, now line 115.

Comment:
Line 97: “add a reference here?   (but also please tie this back to subduction zones, because otherwise there are a lot of other references which need to be included).”

Response :
Thank you for the suggestion. We rephrase this text to tie this back to subduction zones and added “Sallares and Ranero, 2019” and moved Ulrich et al., 2022. The modified version in lines 113-117:

“ Off-fault rigidity is a key controlling factor of earthquake kinematics, dynamics, and tsunami genesis (Lay and Bilek, 2007; Lay et al., 2012; Ulrich et al., 2022) in subduction zones. Shallow rigidity reduction can lead to slower rupture propagation, larger slip, longer rupture duration, and energy depletion at high frequencies characteristic of tsunami earthquakes (Sallares & Ranero, 2019).”

Comment:
Line 100: “please add a reference here about the importance of the upper plate (as opposed to the lower plate).”

Response :
Thank you for this suggestion. We added “Prada et al., 2021a” to line 119.

Comment:
Line 101: “I think that this is a knowledge gap not only for subduction zones but also for many crustal faults.”

Response :
Thank you for this comment. We added “that is not limited to subduction zones” to lines 120-121.

Comment:
Line 102: “slip-strengthening has also been used in the shallow portions of crustal faults, e.g., if I remember correctly, Quin, Tectonophysics, 1990.”

Response :
Thank you for the suggestion. However, in this section our focus shifts specifically to subduction zones. We added this reference to the “Methods” part section “2.4 Friction parameters” in line 283:
“Slip-strengthening parameterization can mimic rate-strengthening behaviour on co-seismic time scales (Quin et al., 1990).“

Comment:
Line 118: Remove the words “yet”, “variations”, “with”, and “depth”.

Response :
Thank you for the suggestion. We removed the suggested words from line 137 in the revised manuscript.

Comment:"
Lines 127-128: “I'm not sure if this sentence should be here: “However high Vp/Vs ratios can result from methodology and instrumental limitations such as band limited signal or R.F. phase interference (Mann, 2021)” - I might omit it. We do always know that our inferences of rock properties at depth are imprecise, so I'm wondering if singling out this one case as potentially being erroneous might not be appropriate, given the uncertainty in many of the other assumed parameters we use in dynamic rupture simulations.”

Response :
Thank you for the suggestion. We agree. We removed this sentence from line 146 in the revised manuscript.

Comment:
Line 128: Remove the phrase “In addition”.

Response :
Thank you for the suggestion. We removed the phrase from line 146.

Comment:
Line 131: “different how - I'm not sure what is meant here - please add a word or two.”

Response :
Thank you for the suggestion. We added a clarification to lines 148-149: “using a single high P_f gradient”.

Comment:
Line 138: “is "allow" the right word to use here? maybe instead write "consider"?”

Response :
Thank you for the suggestion. We changed the phrase “allow for” to consider in line 156.

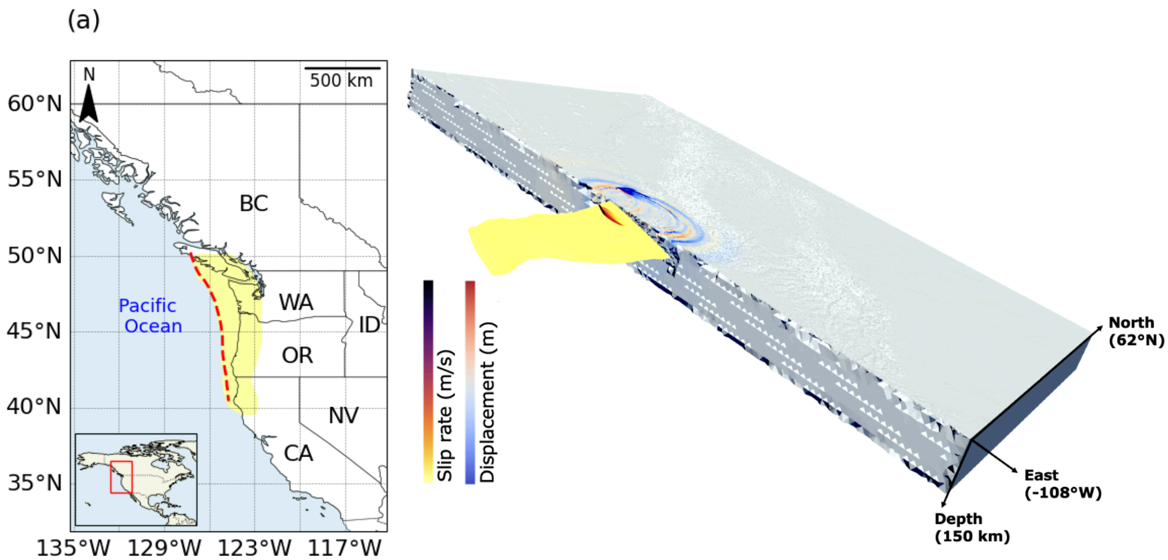
Comment:
Line 143: change "respective" to "resulting"?”

Response :
Thank you for the suggestion. We changed the word “respective” to “resulting” in line 159.

Comment:
Figure 1. “This figure is cool, but a little confusing. Is there more location information to add to it? Also, what's present on the north and south edges of the slab (in real life).”

Response :
Thank you for your suggestion. We revised Figures 1a and 1b: We now include the entire computational domain with a small map at the bottom in Figure 1a, and modified Figure 1b to

show the extent more clearly and increase the brightness. We now also explain in the Figure caption of Fig. 1 that “The meshed megathrust interface is not intersecting the model domain boundaries.”



Rebuttal Letter Figure: Updated Fig.1a and b.

Comment: Figure 1c. “should there be something at the end of the workflow? E.g., results producing the EQ source? (optional).”

Response :
Thank you for your suggestion. We choose to not change Fig. 1c. While the workflow in Figure 1c does not explicitly include post-processed results, it is designed to focus on the key innovations of our methodology (as part of the “Methods” section) specifically, how SDMs are used to constrain initial stresses and run dynamic rupture simulations. The post-processing of multiple simulation outputs is beyond the scope of this figure, as its primary aim is to highlight the workflow for setting up and executing the simulations. We now clarify this better in the Figure caption of 1c as: “This workflow schematic [...], but omits post-processing of simulation outputs.”

Comment:
Line 159: “also cite the Harris et al., SRL, 2011 paper?”

Response :
Thank you for the suggestion. We added “Harris et al., 2011” to line 175.

Comment:
Line 162: “please cite Ramos et al., SRL, 2022 here.”

Response :
Thank you for the suggestion. We added “Ramos et al., 2022” to line 179.

Comment:
Line 189: “does this supercomputer need a reference?   (I'm not sure.)”

Response :
Thank you, it is sufficient to include proper acknowledgements at the end of the paper for this machine.

Comment:
Line 191: Remove the word “current”.

Response :
Thank you for the suggestion. However, we retained the word “current” to emphasize that future observations may provide additional insights to further constrain the degree of coupling in the shallow part of the CSZ.

Comment:
Line 198: “I'm not remembering - are all of these simulations using the same shear-modulus structure?”

Response:
As mentioned in response to the major comment above, we now clarify the structural assumptions of both models in SDMs in line 229-231, as: “The Gamma and Gaussian SDMs of Schmalze et al. rely on elastic dislocation block models presented in McCaffrey et al., 2007, and McCaffrey, 2009, while the shallow-coupled model by Lindsey et al. 2021 relies on block model corrections derived by Li et al., 2018.“

Comment:
Line 216: “I'm glad that "scaling factors" are starting to be introduced here, but I'm still a little confused about exactly what they are. Add an equation?”

Response :
Thank you for this comment. We have added an equation to line 240 where we define the scaling factors. Also see our response to the major comment above.

Comment:

Line 231: “I think of rigidity as relating to the shear modulus, which requires knowledge of the density too, although of course the shear-wave velocity dominates. I'm wondering which information you used for the density profile.”

Comment:
Line 245: “rigidity is not the same as a velocity model, right?”

Response to both comments :
Thank you for this comment. The high rigidity profile corresponds to the 1D average of a 3D community velocity model of Cascadia (Stephenson et al., 2017), see Figure 3 caption. The low-rigidity profile is proposed by Sallarès and Ranero (2019). We have used both the density and the shear wave velocity (Vs) profiles from these papers to convert them into rigidity using

the square root of the density times the shear wave velocity squared. We now clarify this in lines: 261-264 as:

“We construct the low rigidity profile from density and shear wave speeds of Sallarès and Ranero (2019) in all our scenarios as ρVs^2 where ρ and Vs are the density and shear wave speed of the rock, respectively. Exceptions are model 5 and model 7 (Table S1), where we use a higher-rigidity profile based on the densities and shear wave speeds given in Ramos et al. (2021).”

Comment:
Lines 250-252: “please also cite some older work which used slip-weakening. It has been used for a long time. Thanks. E.g., Guatteri and Spudich, BSSA, 2000. Going back even farther in time is o.k. too.”
Line 255: “please cite Ida, 1972 or Andrews, 1976 or Day, 1982 for slip-weakening.”

Response to both comments :
Thank you for your comments. We added a reference to Day, 1982 and Guatteri and Spudich, BSSA, 2000 in Section 2.4. We already cite Ida, 1972; Palmer et al., 1973 and Andrews, 1976 and have now added in this section as well. See also our response to the major comment on page 4.

For our convenience, this paragraph (lines 278-283) now reads as copied here, with new additions in bold:

“We use a linear slip-weakening friction law (Ida, 1972; Palmer et al., 1973; Andrews, 1976; **Day, 1982**). Linear slip-weakening friction is widely used in dynamic rupture simulations (e.g., **Guatteri and Spudich, 2000**; Harris et al., 2018) and can reproduce coseismic on-fault observations as well as seismic and geodetic ground motions (Gallovič et al., 2019; Tinti et al., 2021; Gallovič and Valentová, 2023), specifically for large megathrust earthquakes (Galvez et al., 2016; Ramos et al., 2021; Ulrich et al., 2022; Madden et al., 2022; **Yao and Yang, 2023**; Li and Gabriel, 2024). **Slip-strengthening parameterization can mimic rate-strengthening behavior on co-seismic time scales (Quin, 1990)**.”

Comment:
Line 265: “does this total mesh depth include slab? if no, and I'm guessing no, perhaps mention this mesh depth extent elsewhere, e.g., where are describing the mesh details?”

Response :
Thank you for this comment. We have corrected this in a previous part of the paper, in line 188 of Sec. 2.1 Computational domain as “and a depth of 150~km. “
In this section, we already clarified that the slab depth extends to 50 km and the total mesh extends to a depth of 150 km along the entire domain.

Comment:
Lines 268: “In this section it might be good to again mention Yao and Yang, EPSL, 2023.”

Response :
Thank you for the suggestion. We added the proposed citation to line 310.

Comment:
Lines 292-293: "perhaps a simple way this might be done was presented in Harris and Day, GRL, 1999. We used dynamic rupture simulations of an earlier EQ (Parkfield 1934), then added interseismic stressing, to produce the initial stress conditions for the next earthquake's dynamic rupture (Parkfield 1966). Or is this manuscript describing something different?."

Line 293: "please check Day et al., BSSA, 1998 (and there are also many subsequent similar papers out there) which use kinematic rupture simulations of a previous earthquake to produce the initial stresses for a dynamic rupture model. I'm not sure if I'm understanding this sentence well, is this what is being described here too?"

Response to both comments:
Thank you for your thoughtful suggestions. The paper Harris and Day, 1999 uses stress changes from the Parkfield 1934 earthquake to perturb the stress field on adjacent faults, then adds accumulated interseismic stress, (though it does not provide a detailed explanation of the computation process). In contrast, our approach differs in that we do not rely on stress perturbations from previous earthquakes. Instead, we assume a gradually increasing stress gradient with depth, and we compute the stress change resulting from the slip in the SDM, using it as a boundary condition in a pseudo-static simulation. We now clarify this better and cite the suggested reference Day et al., 1998, as:

"While this approach has not been used to consistently infer initial stresses from SDMs for dynamic rupture simulations before, lines: 333-335: "its implementation is comparable to using a kinematic finite source model of an earthquake to determine initial dynamic parameters for modeling the event (Day et al., 1998, …)."

Comment:
Lines 324: "Thank you for citing Harris et al., 2018 here, but I think that we did the best job describing the nucleation method in Harris et al., JGR, 2021, so please cite that paper here. Thanks!"

Response :
Thank you for this comment. We have removed Harris et al.. 2018 and added the "Harris et al., JGR, 2021" reference to section "2.6 Rupture nucleation" line 366.

Comment:
Line 344: "please add references here to the empirical relations."

Response :
Thank you very much for this suggestion. We added the proper citation in the text line 386-387: "proposed by Allen and Hayes (2017)".

Comment:
Figure 5: Remove the word "examplary" from figure 5 caption.

Response :
Thank you very much for this suggestion. We removed “examplary” from the figure’s caption.

Comment:
Line 462: “please remind the reader here what a "scaling factor" is.”

Response :
Thank you for this suggestion. We added a reminder to line 504: “(the product of slip deficit rates and a certain duration)”.

Comment:
Line 470: “Maybe call these "locking stress shadows", or something different, to distinguish them from real stress shadows?”

Response :
Thank you very much for this important comment. The term “stress shadow” that we use is defined as when down-dip asperities partially or entirely immobilize the shallow part of the fault (lines 80-83). We follow previous studies naming for consistency (see Wang and Dixon, 2004; Hetland and Simons, 2010; Almeida et al., 2018; Lindsey et al., 2021). I added a clarification that this term differs from the traditional stress shadow (i.e., negative Coulomb stress change) in lines 85-86: “This concept is not to be confused with the stress shadow describing a negative Coulomb failure stress change on the fault, as defined by Harris & Simpson 1996, 1998”. Please also see our response to the major comment on page 5.

Comment:
Line 550: “should probably add here that these simulations are elastic, and inelastic yielding will probably change the results.”

Response :
Thank you for this suggestion. We have added in line 595 : “In elastic models, reduced shallow rigidity…

Comment:

Line 606: Remove the word “the”.

Response :
Thank you for this suggestion. We removed “the” from line 647.

Comment:
Line 635: “evidence" isn't quite the right word here. Use "examine" instead? Or "explore"?”.

Response :
Thank you for this suggestion. We changed it to “observe this behaviour in two models” in line 676.

Comment:
Line 647: Add “of our”.

Response :
Thank you for this suggestion. Added the suggested phrase to line 692.

Comment:
Line 677: “please also cite work by Paul Okubo and Jim Dieterich”.

Response :
Thank you for this suggestion. We have added “Okubo and Dieterich, 1986” to line 743.

Comment:
Line 699: Use "important" instead of "crucial"?”

Response :
Thank you for the suggestion. We changed the word “crucial” to “important” in line 778.

Comment:
Line 700: Use the phrase "also significant" instead of "crucial"?”

Response :
Thank you for the suggestion. We changed the word “crucial” to "significant" in line 779.

—---

Reviewer B:

This is a great paper exploring more realistic 3D simulations of ruptures on the CSZ megathrust using geodetic coupling models as initial constraints. The work is creative, original, and includes important findings that will be of broad interest to CSZ scientists, in particular, and to megathrust science in general. I attach an annotated PDF with minor remarks.

Major Comments:

Comment:
(i) Situate in the broader context of the 1700 event
It's unclear whether the paper is or isn’t making claims about what the modeling implies for our understanding of the 1700 rupture. It frequently uses the paleosubsidence as a comparison but is it to say whether this or that behavior in 1700 is likely? The issue is muddled.

Comment:
I think the paper should in fact make claims about what the authors think this implies for 1700. Beyond the Melgar 2021 paper the authors already cite, that discusses the issue of single or serial ruptures, the paleoseismologists are also re-litigating what the paleoseismic record actually shows both onshore and offshore. See as an example these 2 papers by Lydia Staisch and collaborators:

https://pubs.geoscienceworld.org/gsa/gsabulletin/article/doi/10.1130/B37343.1/645040/Turbidite-correlation-for-paleoseismology

https://pubs.geoscienceworld.org/ssa/bssa/article/114/3/1739/635548

There are others… how are we to understand this new modeling work in that context? It would be nice to see intro and discussion material where the authors explain how their findings fit in.

Response to both comments :
Thank you for this comment. While the focus of this study is to explore dynamic trade-offs in dynamic rupture scenarios of the Cascadia Subduction Zone, we have added a new Discussion section lines 717-735 as:

"4.6 Implications for the 1700 A.D. megathrust rupture

While the focus of this study is to explore dynamic trade-offs in dynamic rupture scenarios of the Cascadia Subduction Zone (CSZ), we observe that most dynamic ruptures in our study arrest before they can propagate along the entire margin and the margin-wide scenario overestimates the 1700 A.D. coseismic subsidence amplitudes. This apparent contradiction may be reconciled by several partial ruptures rather than a single, margin-wide event (Melgar, 2021).

Using our framework for estimating the initial stress conditions and careful consideration of how rigidity, pore fluid pressure, and SDMs interplay, partial ruptures are dynamically favored along the Cascadia margin. The mechanical, frictional, and stress conditions in the central CSZ exert first-order control on rupture dynamics. There, the dynamic conditions conducive to margin-wide ruptures are different from those required for partial ruptures, and include a slip deficit in the central CSZ exceeding 10~m. This model leads to an overestimation of the 1700 A.D. coseismic subsidence amplitudes. While the fact that a dynamic model does not fit paleosubsidence does not rule out such behavior as likely for future events, the physical implausibility of the required assumptions on initial parameters render the margin-wide scenario unlikely in our framework. Future dynamic rupture models may also explore alternative slip deficit models, particularly focusing on shallow coupling.

We use paleoseismic subsidence measurements from the 1700 A.D. event from Wang et al., (2013) to compare our physics-based model predictions to, following Ramos et al., 2021. Our aim is to estimate the relative subsidence amplitudes in the context of various dynamic rupture model parameters. However, such paleoseismic data may also capture pre- and post-seismic elastic and viscoleastic deformation, potentially overestimating co-seismic subsidence. More recent paleoseismological analyses (e.g., Kemp et al., 2018, Pagett et al., 2022, Staisch et al., 2024, Niemienski et al., 2024), can be included in future modeling studies."

We also added to the Introduction lines 36-39:

"The last large earthquake(s) occurred in 1700 A.D. (Atwater and Yamaguchi, 1991) and likely caused a tsunami documented in Japanese historical records (Satake et al., 2003). It remains debated if paleoseismological evidence implies margin-wide or a series of partial ruptures (Melgar, 2021). "

Comment:
Two minor notes here, I think you are missing some paleosubsidence points. When I compare your plots to Melgar 2021 or Melgar et al. 2022 I think you are missing one subsidence point in BC and one in CA, perhaps the later one is the one published in the later Padgett or Kemp papers.

Response :
Thank you for pointing this out. In this study, we use the paleosubsidence dataset from Wang et al. (2013), which provides a comprehensive record covering the entire CSZ margin. While we are aware of more recent datasets, such as those from Kemp et al. (2018) and Padgett et al. (2022), these are more localized and do not span the entire margin. Regarding the subsidence points mentioned in Melgar (2021) and Melgar et al. (2022), their total record includes 14 subsidence points, which matches the dataset used in our study. The referred missing points may correspond to subsidence estimates with modified locations compared to those in Wang et al. (2013) (see also Ramos et al. (2021)).

We now clarify this in the newly added Discussion section in the manuscript in lines 730-735 as:

"We use paleoseismic subsidence measurements from the 1700 A.D. event from Wang et al., (2013) to compare our physics-based model predictions to, following Ramos et al., 2021. Our aim is to estimate the relative subsidence amplitudes in the context of various dynamic rupture model parameters. However, such paleoseismic data may also capture pre- and post-seismic elastic and viscoleastic deformation, potentially overestimating co-seismic subsidence. More recent paleoseismological analyses (e.g., Kemp et al., 2018, Pagett et al., 2022, Staisch et al., 2024, Niemienski et al., 2024), can be included in future modeling studies."

Comment:
Additionally, it would be good to hear your thoughts in the discussion on the role of afterslip/post-seismic processes in what the paleosubsidence captures. Do you think the paleosubsidence represents just coseismic deformation?

Response :
Thank you for this suggestion. In response to this and a similar question by Reviewer A, we have added in lines 732-733:

"However, such paleoseismic data may also capture pre- and post-seismic elastic and viscoleastic deformation, potentially overestimating co-seismic subsidence."

Comment:
(ii) relationship to hazards/future events
The previous comment was about what your models say about the past, my second major comment is what do they tell us about what is possible in the future? I realize some of this is discussed already but it felt likes stronger statements could be made.

Here things that I wonder about include: if a dynamic model *doesn't* fit paleosubsidence do you think that rules that behavior out as likely for future events? Should we treat paleosbusiodence from 1700 as a strong "boundary condition" of sorts when we think of models that reflect potential future behaviors? Or, are models that wildly violate paleosubsidence still equally likely? It would be good to have guidance here from the authors.

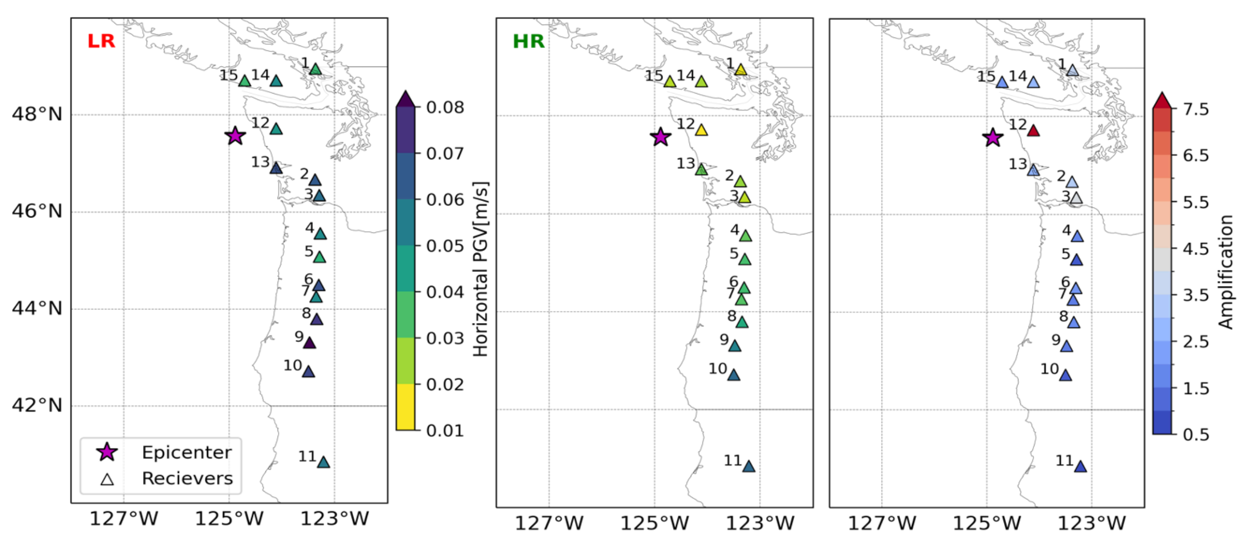
Response :
Thank you for this valuable comment. In this study, our primary focus was on comparing different models under varying settings and initial conditions. However, we agree that determining the plausibility of these scenarios is crucial for future ground motion and hazard analysis. To address this, we have added to the discussion lines 726-728 the following:

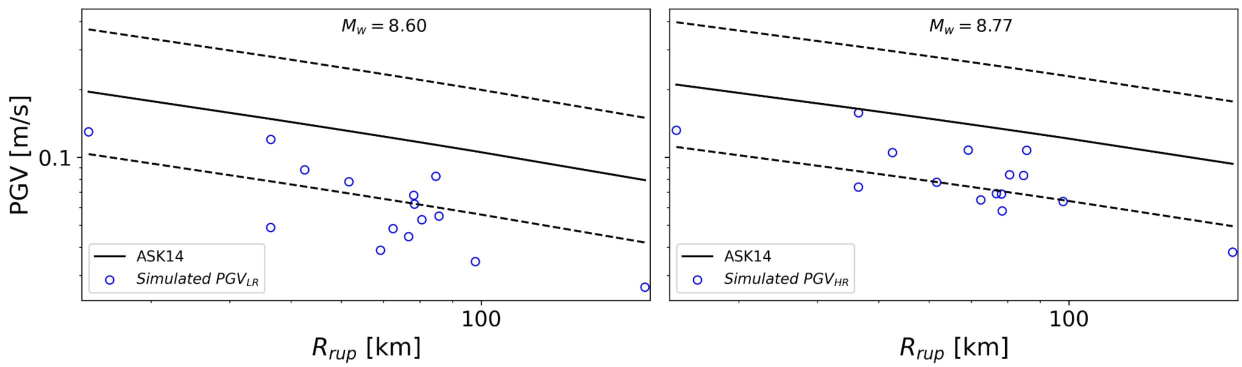
“While the fact that a dynamic model does not fit paleosubsidence does not rule out such behavior as likely for future events, the physical implausibility of the required assumptions on initial parameters render the margin-wide scenario unlikely in our framework.”

And to section “4.7 Model limitations” lines 765-766:
“Such future work may focus on evaluating the potential of the scenarios presented here in producing realistic ground shaking estimates.”

Comment:
And finally, it would be nice to hear some thoughts on how each of these models implies for ground motion. Do you have any thoughts on what we might learn about how your research helps us to narrow down potential future outcomes in terms of shaking?

Response :
We added a new Supplementary Figure S17 (reproduced below) in which we show analyses comparing ground motions for high and low rigidity velocity models under the high pore fluid pressure scenario (γ=0.97). Additional analysis goes beyond the scope of this study and these preliminary results will be expanded in future work, including a 3D velocity model. We do not rule out any of the modeled scenarios and plan to test their implications for ground motion in greater detail.





New Supplementary Figure S17: (Top) Horizontal Peak ground velocities (PGVs) for the low rigidity model 2 (LR; left) and high rigidity model 5 (HR; middle) scenarios under the high pore fluid pressure assumption (γ = 0.97). (right) PGV amplification factors of the LR/ HR scenarios. (Bottom) PGV attenuation relationship of the respective models compared with the ASK14 GMPE (Abrahamson et al., 2014).

To address this point in the revised manuscript, we have added in the Discussion section "4.7 model limitations" lines 760-766 :

"Fig. S17 illustrates the effect of model rigidity on ground shaking, comparing peak ground velocities (PGVs) from the low-rigidity Model 2 and high-rigidity Model 5 to the ground motion prediction equation ASK14 (Abrahamson et al., 2014). While the high-rigidity Model 5 better matches empirical PGVs, both models generally underestimate them.

Incorporating a more realistic 3D velocity model in future simulations will allow us to account for bi-material effects on rupture dynamics Ma and Beroza (2008) as well as the effects of sedimentary basins (e.g., Olsen, 2000; Pilz et al., 2021; Niu et al., 2025) on ground shaking. Such future work may focus on evaluating the potential of all the scenarios presented here in producing realistic ground shaking estimates."

Minor Comments, as annotated within the manuscript:

Comment:
Lines 35 - 38: "The last large earthquake occurred in 1700 A.D. .. Since then, the CSZ has been accumulating strain , with almost no interplate seismic activity."

"We don't actually know that, do we? We could have had a vigorous sequence of aftershocks and smaller M7+ events that would be difficult if not impossible to see in the paleoseismic record."

Response :
Thank you for this comment. Yes that is an important point, see also comments by Reviewer A above. We revised this text in lines 39-40 as:

"Since then, the CSZ has been accumulating strain, though the paleoseismic record provides limited constraints on the occurrence of interplate seismic activity, including potential M6+ or M7+ events."

Comment:
Lines 44 - 45: "The lack of instrumental records of a sizable megathrust earthquake complicates the mitigation of future seismic and tsunami hazards posed by the CSZ."

"We do not mitigate hazard. The potential for future large events cannot be reduced. We mitigate "risk" which includes human factors such as "exposure", –that's the number of and precarity of assets in the hazard zone."

Response :
Thank you for this comment. We changed the word "hazards" to "risks" in line 50, see also our response to Reviewer A above.

Comment:
Lines 136-137: "In this paper, we present a unified workflow linking SDMs to 3D dynamic rupture simulations by converting SDMs into heterogeneous initial stresses using the Slab2.0 geometry."

"Not there yet and maybe this is addressed: A discusion item should be how you anticipate the results would change (if at all) given the CASIE21 findings of significantly different slab geometries by Carbotte et al. 2024."

Response :
Thank you very much for this comment. That is a good point. We added a discussion point in lines: 754-759, section "4.6 Model limitations" under the "Discussion":
"This study uses the Slab2 CSZ geometry (Hayes et al., 2018). However, a recent and more detailed slab model provided by the Cascadia Seismic Imaging Experiment 2021 (CASIE21, Carbotte et al., 2024), reveals differences including sharper transitions in slab dip, more pronounced segmentation, and localized variations in sediment underthrusting and subduction. Future dynamic rupture studies incorporating the CASIE21 geometry may focus on capturing smaller-scale heterogeneities that may affect magnitude and rupture characteristics of dynamic scenarios (Ide & Aochi, 2005; Wirp et al, 2024)."

Comment:
Figure 1 caption: "All text in all figures (tick marks, legends, labels,) should be at least as big as the text int he figure caption. It detracts from the impact of your fantastic images that I can't easily read them!"

Response :
Thank you for this comment. We modified the Figure's text as requested.

Comment:
Line 165: Section title: "2.1 Computational domain"
"Is there 3D seismic structure built in? This is unclear from this section."

Response :
Thank you for your question. We only use a 1D velocity structure. The seismic structure is introduced in section “2.3 Depth-dependent variable rigidity and 1D velocity models.”

Comment:
Lines 174-176: “We transform longitude/latitude coordinates to Cartesian coordinates in km, centered at 128°W and 46.8°N, using a Plate Carr’e (also known as Equirectangular or Equidistant Cylindrical) projection.”
Totally minor question, is there a reason for this choice other than this being the default in CartoPy?

Response :
Thank you for your question which alerted us to a mistake in the paper. We updated the correct projection in the revised manuscript and in the Zenodo repositor
y to:

“We transform longitude/latitude coordinates to Cartesian coordinates in km, centered at 128°W and 46.8°N, lines 188-189: “using a custom projection based on a spherical Earth approximation (see “Data and Code availability”).“

Comment: Lines 200-202: “Ramos et al. (2021) show that dynamic rupture models using the Gaussian SDM of Schmalzle et al. (2014) can fit
the 1700 A.D. paleoseismic data (Wang et al., 2013) better compared to using the shallow-coupled Gamma SDM of the same study. We contrast this with two shallow-coupled SDMs of Lindsey et al. (2021) representing a large slip deficit
rate near the trench.”

“The deep SDR in the Lindsey models is likely an artifact of using a purely elastic inversion schemes (i.e. dsiregarding mantle viscoelastic processes). Does this matter?
This is discussed at length in Li et al. (2018), which is incidentally what I believe is a better shallow coupling model. https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2018JB015620.”

Response :

We have clarified that in lines 230-231: “[...] the shallow-coupled model by Lindsey et al., 2021 relies on block model corrections derived by Li et al., 2018.”

We agree that future dynamic rupture studies should investigate other coupling models and believe that this study will provide motivation and a framework on how to do this. We added to the new Discussion section lines 728-729: “Future dynamic rupture models may also explore alternative slip deficit models, particularly focusing on shallow coupling.”

Comment: Figure 2: “Minor nitpick that depends on journal policies. "Rainbow" is not a good colormap IMHO, it introduces false looking boundaries/discontinuities in the data. Sequentials are better: https://matplotlib.org/stable/users/explain/colors/colormaps.html.”

Response :
Thank you for your suggestion. We understand that the "rainbow" colormap can introduce perceptual artifacts and false discontinuities. Following your recommendation, we have replaced it with a perceptually uniform sequential colormap.

Comment:
Line 258: “These values for Dc, C etc are totally (true?) unconstrained, we don't know them. Ramos et al. (2021) justified that chocie and you don't want to re-litigate that here but it would be good for the reader to know a little bit about why these are sensible choices, since these parameters are so important. Could you explain the choice just a little bit?”

Response :
Thank you for this comment. We have added:
Lines 291-292: “ [...] In dynamic rupture simulations with linear slip-weakening friction and depth-dependent initial stress, frictional cohesion is often used to counteract low effective normal stress at shallow depths. [...] “
Lines 294-299: “The critical slip-weakening distance governing large earthquakes is challenging to constrain (Mikumo et al., 2003) and maybe scale-dependent (Gabriel et al., 2024). [...]
This choice of Dc is comparable to the range of Dc used in slip-weakening simulations of the Tohoku-Oki earthquake, which constrained Dc using the frequency range of back-projection results (Huang et al., 2014), and the 2004 Sumatra earthquake (Ulrich et al., 2022).”

Comment:
Line 345: “All hypocenter locations have a depth of 16 km in our simulations.”
Can you explain why this choice?

Response :
Thank you for this comment. We explain how we chose the location of the hypocenter in the following lines 379-380: “We align the location of the nucleation area with the highest values of slip rate and total slip deficits of the Gaussian SDM.”

.

Comment:
Lines 363-393: “This long narrative might be better served by a detailed table. hard tot rack what is what otherwise.”

Response :
Thank you for this comment. We summarize the information from this paragraph in Table S1, provided in the “Supplementary Materials,” in accordance with Seismica guidelines. However, we felt it was important to retain a detailed written description of each model in the main text to complement the table, as the table serves primarily as a concise summary.

Comment:
Figure 6: “I'm curious about kinematic parameters such as rise time and slip rate. Do those also scale? See simple scaling laws in Melgar & Hayes (2017) https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017GL074916.”

Response :
Thank you for your insightful comment. In this study, our primary focus was on fault slip as a comparative measure between our models, supported by adherence to established empirical scaling laws. While we recognize that parameters such as rise time and slip rate are critical, particularly for ground motion modeling, these aspects were beyond the scope of the present study. We appreciate the reference to Melgar & Hayes (2017) and plan to extend this work in future studies to include ground motion analysis, where we will incorporate and compare our findings with available scaling laws, including the one you suggested.

Comment:
Lines 411-413: "While based on initial conditions that may appear less realistic, model 1 matches the 1700 A.D. subsidence data better in the northern part of the CSZ."

"Quantified how? It would be good to have some sort of formal statistic to assess this."

Response :
Thank you for your valuable comment. We chose a visual comparison because the 1700 A.D. paleoseismic subsidence data has a large standard deviation, making it difficult to define a meaningful quantitative metric. In our view, a formal statistic would not fully capture the level of agreement due to these uncertainties. However, we appreciate your suggestion and will consider exploring quantitative methods in future work.

Comment:
Lines 472-474: "In addition, the margin-wide rupture mostly fits the 1700 A.D. paleoseismic subsidence observations in the South within observational uncertainties. However, this scenario overestimates the subsidence in northern and central CSZ with respect to observations."

Why must it match the 1700 observations? Would we expect the subsidence pattern to be the same over many M9 sized earthquakes?

Response :
Thank you for your comment. We have clarified this better in our new Discussion section (see our response above).

Comment:
Lines 647-650: "As expected, deeper frictional transition depth facilitates deeper dynamic rupture propagation (Fig. S1). We evidence this behavior with two models: (1) model 3 in which the slip-neutral friction zone (27-32 km) is replaced by slip-weakening friction (Fig. S1b) and (2) model 4 in which both slip-neutral and slip-strengthening friction are replaced by the slip weakening friction at depths greater than 27 km (Fig. S1b)."

"Might be worthwile to note here that Melgar et al. (2022) also found a way to make this deeper slip still respect the paleoseismic subsidence in much simpler kinematic simulations
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2021GL097404
This is also discussed by Wirth et al. (2019) who argued the opposite that deep slip would not work because of that subsidence signal.

https://pubs.geoscienceworld.org/ssa/bssa/article/109/6/2187/574615/Impact-of-Down-Dip-Rupture-Limit-and-High-Stress Your results contribute tot his debate. Some connections to what this implies for hazards (shaking) would be good in this discussion."

Response :
Thank you for this comment. We added a sentence citing both Melgar et al. (2022) and Wirth et al., 2019 here in lines 679-682 as:
" Melgar, 2022, showed that deeper slip may still respect the paleoseismic subsidence in simpler kinematic simulations. However, Wirth et al., 2019 argued that deep slip would be inconsistent with paleoseismic subsidence. Our results contribute to this discussion by demonstrating how different frictional behaviors influence rupture extent and moment magnitude, with implications for shaking hazard."

We are planning to follow on this work with ground motion analysis and look into the effect of the locking depth on ground motions. Ramos et al. (2021) already showed that deeper locking depth produce larger subsidence (see Figure 7 in their paper), showing that for every 2 km increase in locking depth, the maximum uplift and subsidence amplitudes increase by approximately 1 m.

Comment:
Lines 737-744: "We have shown that partial ruptures are favored along the Cascadia margin, which may suggest that the dynamic conditions conducive to margin-wide ruptures are different from those required for partial ruptures. Our updated framework for estimating the initial stress conditions and careful consideration of how rigidity, pore fluid pressure, and SDMs interplay corroborate the observed tendency for Mw < 9 events. However, margin-wide rupture is only realized if the slip deficit in the central CSZ exceeds 10 m, which leads to an overestimation of the 1700 A.D. coseismic subsidence amplitudes. Our results suggest prioritizing the reconciliation of the mechanical, frictional, and stress conditions in the central CSZ, as its state exerts first-order control on rupture dynamics and, consequently, tsunamigenesis or strong ground motion."

"This is a huge contribution of this work and deserves an entire dsicussion section, in my opinion. The debate on whether 1700 was one or many is alive and well and these findings could be framed more strongly as contributing tot hat debate."

Response: As explained in our reply above, we have added a new Discussion section to address this comment.

We again thank both reviewers for the detailed and constructive comments.