Reviewer A Comments

For author and editor

General Comments

The manuscript uses physics-based earthquake simulations for a single fault in central Italy. The approach is primarily used to understand variable displacement profiles on fault surfaces could impact seismic hazard. The manuscript may also contribute to the current debate on how best to use earthquake simulators to improve seismic hazard assessment. In my opinion, the manuscript should be published after minor revisions. Addressing the general points listed below may help improved the manuscript. In addition to these points I thought that the manuscript could be improved by providing more detail in places.

- While I accept the basic premise that physics-based earthquake simulators have not been widely used in seismic hazard assessment, an increasing number of papers have been published on this topic in the last 5-10 years (e.g., Shaw et al., 2018; Milner et al., 2021; Niroula et al., 2025). Also, increasingly physics-based outputs from earthquake simulators are being used to inform 'empirical' seismic hazard models. For example, Gerstenberger et al. (2024) use Coulomb stress outputs from RSQsim to inform their rupture sets. The manuscript probably needs to refer to these papers and to reflect the increasing use of physics-based simulators in seismic hazard assessment.
- As far as I am aware, most seismic hazard models do not use variable fault slip and, in this regard, the present manuscript is a step forward. However, both Milner et al. (2021) and Gerstenberger et al. (2024) use the Shaw (2019) hybrid loading model which produces displacements that taper towards fault tip lines. This probably needs to be acknowledged in the manuscript, although I do not think that these authors tested the impact of displacement tapering towards fault tips on seismic hazard (compared to uniform slip models).
- The present study focuses on displacement variability on a single fault, which is a good start, and how this impacts seismic hazard. However, I do wonder if the changes in seismic hazard (for different displacement profile shapes) would be reduced when the entire fault system is considered. The reason for this is that displacement gradients on interacting faults tend to be equal and opposite (i.e., as displacement dies out on one fault it picks up on a second nearby fault). Therefore, I would expect the total seismic moment across the system to be the same independent of slip -profile shape (as long as the uniform profiles are using average slip and displacement profiles on adjacent faults are similar in shape). Future papers could test the impact on seismic hazard for single and multiple faults, but in the meantime I would be inclined to add text to clearly signal that the results may vary depending on whether faults are considered in isolation or collectively as a system. On this note, I would also make it clearer in the manuscript title that you are studying a single fault. For example, change "....on normal faults" to "...on a normal fault".
- The hazard curves are for a single location, but it is not clear why this location was selected or whether the results change between locations. Can you indicate why you

chose this site. Can you also show hazard curves for multiple sites in the main manuscript.

In addition to the above comments, I have included a number of handwritten comments on a scanned hardcopy of the manuscript (apologies if my handwriting is difficult to read). Please address these comments as you see fit.

References below are either referred to above or handwritten on the manuscript.

References

Delogkos, E., Howell, A., Seebeck, H., Shaw, B., Nicol, A., Liao, Y-W., Walsh, J., 2023. Impact of variable fault geometries and slip rates on earthquake catalogues from physics-based simulations of a normal fault. *Journal of Geophysical Research - Solid Earth* 128(11), e2023JB026746. https://doi. org/10.1029/2023JB026746.

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Reviewer B Comments

For author and editor

First, I want to acknowledge the opportunity to review the article titled "Spatially heterogenous Holocene slip rates drive seismic sequence variability on normal faults" authored by Rodriguez Piceda and co-authors.

The article conducts an analysis of the impact that along-strike slip rate assumptions have on earthquake cycle simulations and on seismic hazard estimates. The article tackles an important but often overlooked topic in SHA by employing a suitable experimental design that systematically explores slip rate assumptions on a fault in Central Italy. The results clearly depict how slip rate assumptions are not trivial for physics-based SHA, as they condition how earthquake ruptures nucleate and propagate (in a physics-based framework) and, therefore, how their long-term statistics are defined (e.g., MFDs). The article is a relevant contribution to the growing scientific community working on physics-based SHA models. The article will also likely be of interest to the SHA community in general, although the contribution to this field is not novel; previous works co-authored by the some of the authors signing the present paper have already tackled this problem with a similar approach (Faure Walker et al. 2019).

I have only a few comments/questions:

A. Model parameters

The article uses QDYN, a rate-and-state quasi-dynamic earthquake simulator. Like all earthquake simulators, a big aspect is the model parameter selection. The set of parameters that constrain the models (friction coefficients, initial stresses, etc.) have a large impact on the simulated earthquake catalogues (M_{max}, MFD shape), rupture propagation characteristics and agreement with earthquake scaling observations (e.g., scaling relations). I think that discussing a bit more on the reasoning behind some of the parameter selections for the models would strengthen the paper, especially the *a* and *b* coefficients of the rate-and-state friction law and the width of the velocity-strengthening regions. Are the *a* and *b* values standards based on laboratory experiments? How reasonable/appropriate are they for the region studied in Central Italy? Would different values impact the outcomes of the paper in terms of MFD or earthquake sequence behaviour? I find the information on the Supplementary material helpful but not complete in this regard.

B. Slip rate vs. fault geometry

First, as the paper acknowledges, the slip rate variability along strike has been previously linked to fault geometric complexities like bends or relay zones (some references are cited in the manuscript). Some of these complexities have to do with near-surface effects and might not be extrapolable to the full seismogenic thickness of the fault, where faults are presumably geometrically simpler (higher confining pressures, transition to ductile regimes, etc.). In practice, this means that some of the slip rate variability along strike might be the response to localized stress concentrations induced by shallow geometric features, which might amplify or reduce the slip. In the paper, it is assumed that surface along-strike slip rate variability is persistent throughout the whole fault plane, even at depth. What is the rationale behind this assumption?

Second, the slip rate profiles explored in the paper are based on surface measurements on a geometrically complex fault but imposed onto a planar fault model. This modelling choice removes the possibility of simulating the physical effects of fault roughness or geometric complexity on earthquake nucleation, propagation, or arrest, all factors which might themselves be responsible for generating along-strike slip rate variations in the long-term. In other words, surface slip rate and fault geometry might not be independent parameters. Ignoring one of them could be conceptually artificial, especially when the modelling is focused on generating realistic earthquake sequences for hazard assessments.

This interdependence is evidenced in previous studies. For instance, Allam et al. (2019) showed that surface slip variability over seismic cycles can emerge purely from geometric complexity (fractal roughness), even when the along-strike slip rate is uniform. My question is: could the slip rate variability observed in the field be reproduced by simulating a fault with realistic geometry and a uniform slip rate? While I understand the computational limitations of implementing geometric complexity within the FFT framework in QDYN, it would add depth to the manuscript if this limitation was explicitly acknowledged and discussed.

On the same slip rate note, slip rate variability is not only defined by along-strike variations, but also down-dip. Slip rate usually tapers toward both the surface and the base of the seismogenic layer (see Finocchio et al., 2016) and including this variability has also an impact on earthquake sequence characteristics (e.g., Delogkos et al., 2023). I suggest the authors to consider discussing this aspect as well.

Other minor/formal comments

- The paper uses the term GMPE. Given that generally a GMPE (e.g., Bindi et al., 2011) is composed of several equations, I think the term GMM (Ground Motion Model) is more appropriate. Moreover, the GMM term is increasingly being used in literature compared to GMPE.
- The Supplementary text contains a grammatical error and font size change in Page 4: “apporixmatelly”, and a reference highlighted in yellow in page 7.
- The citation “Rodriguez Piceda et al. (2025)” of the Supplements is not listed in the references of that document.

Overall, this article is a strong contribution for physics-based applications to PSHA with useful insights on slip rate - dependent earthquake rupture behavior and hazard. The results further reinforce previous findings obtained from analytical approaches. I recommend the article for publication in Seismica pending the minor comments mentioned above.

Best regards,

Octavi Gómez Novell

References

- Finocchio, D., Barba, S., Basili, R. (2016). Slip rate depth distribution for active faults in Central Italy using numerical models, Tectonophysics, 687, 232-244, https://doi.org/10.1016/j.tecto.2016.07.031

Response to Reviewers

Dear Editor,

We are submitting a revised version of our manuscript '*Spatially heterogeneous Holocene slip rates drive seismic sequence variability on a normal fault*'. We would like to thank the two reviewers for their reviews. We carefully considered all their comments and revised the original version accordingly, as outlined below and in the revised version of our manuscript.

The main concern by reviewer #1 was related to missing references to recent studies using earthquake simulators for seismic hazard assessment. We have now cited these studies throughout the manuscript where appropriate. Reviewer #2 highlighted the need to test the robustness of the results through a sensitivity analysis of the rate-and-state frictional parameters and downdip variations of the long-term slip rate. We addressed this by including additional simulations and discussing their implications in the revised manuscript.

Our answers to each point raised by the reviewers are provided below, along with a detailed explanation of all changes made to the original manuscript on a comment-by-comment basis. All modifications have also been tracked in the revised manuscript. We hope that our revisions are satisfactory and that the updated manuscript meets the journal's standards for publication.

On behalf of all authors,

Constanza Rodriguez Piceda

<u>Reviewer#1</u>

The manuscript uses physics-based earthquake simulations for a single fault in central Italy. The approach is primarily used to understand variable displacement profiles on fault surfaces could impact seismic hazard. The

manuscript may also contribute to the current debate on how best to use earthquake simulators to improve seismic hazard assessment. In my opinion, the manuscript should be published after minor revisions. Addressing the general points listed below may help improved the manuscript. In addition to these points I thought that the manuscript could be improved by providing more detail in places.

We thank the reviewer for the positive feedback and address their comments below.

• Point 1: While I accept the basic premise that physics-based earthquake simulators have not been widely used in seismic hazard assessment, an increasing number of papers have been published on this topic in the last 5-10 years (e.g., Shaw et al., 2018; Milner et al., 2021; Niroula et al., 2025). Also, increasingly physics-based outputs from earthquake simulators are being used to inform 'empirical' seismic hazard models. For example, Gerstenberger et al. (2024) use Coulomb stress outputs from RSQsim to inform their rupture sets. The manuscript probably needs to refer to these papers and to reflect the increasing use of physics-based simulators in seismic hazard assessment.

These references are now included in the Introduction (L99-100; L-113-L114) and Discussion sections (L430,431).

• Point 2: As far as I am aware, most seismic hazard models do not use variable fault slip and, in this regard, the present manuscript is a step forward. However, both Milner et al. (2021) and Gerstenberger et al. (2024) use the Shaw (2019) hybrid loading model which produces displacements that taper towards fault tip lines. This probably needs to be acknowledged in the manuscript, although I do not think that these authors tested the impact of displacement tapering towards fault tips on seismic hazard (compared to

uniform slip models).

To include the earthquake simulator studies which make use of the hybrid loading approach this sentence in the Introduction section was modified as follows: “Similar to fault-based SHA, these simulations often either assume a single value of long-term slip rate or make use of a hybrid loading approach that retains a constant slip rate while smoothing stress concentrations near fault edges (Milner et al., 2021; Shaw, 2019)"

• Point 3: The present study focuses on displacement variability on a single fault, which is a good start, and how this impacts seismic hazard. However, I do wonder if the changes in seismic hazard (for different displacement profile shapes) would be reduced when the entire fault system is considered. The reason for this is that displacement gradients on interacting faults tend to be equal and opposite (i.e., as displacement dies out on one fault it picks up on a second nearby fault). Therefore, I would expect the total seismic moment across the system to be the same independent of slip -profile shape (as long as the uniform profiles are using average slip and displacement profiles on adjacent faults are similar in shape). Future papers could test the impact on seismic hazard for single and multiple faults, but in the meantime I would be inclined to add text to clearly signal that the results may vary depending on whether faults are considered in isolation or collectively as a system. On this note, I would also make it clearer in the manuscript title that you are studying a single fault. For example, change “....on normal faults” to “...on a normal fault”.

Following the reviewer’s suggestion, we added the following paragraph in the Discussion section addressing the potential implications of considering a single fault vs. a fault network:

“The present study focuses on slip-rate variability and its impact on seismic hazard on a single fault. Faults, however, do not appear in isolation but are embedded within fault networks where stress interactions strongly influence their slip behavior. Observations on relay ramps, overlapping fault segments and stepovers indicate that, as slip decreases on one fault, it commonly increases on a nearby faults (e.g., Cartwright et al., 1995; Manighetti et al., 2001; Peacock & Sanderson, 1991). This complementary pattern suggests that slip-rate variability and its influence on seismic hazard may differ depending on whether faults are considered individually or collectively as a system. Future work could test the impact of spatially variable slip rates by comparing isolated faults with fault networks.”

We also changed the manuscript title to “Spatially heterogeneous Holocene slip rate drives seismic sequence variability on a normal fault”

• ***Point 4: The hazard curves are for a single location, but it is not clear why this location was selected or whether the results change between locations. Can you indicate why you chose this site. Can you also show hazard curves for multiple sites in the main manuscript.***

We chose this site due to its proximity to the Parasano-Pescina fault. We included this clarification in the Methods section (L258-L259). An additional figure Fig. S9 showing the hazard curves for the more distant sites of Avezzano and Sulmona was added to the supplementary material. We included an explanation of these sites in the Results section where we describe that the comparison between models remains consistent with that of Trasacco, although the PoE values are lower due to the larger distance from the Parasano-Pescina fault (L379-L381).

• ***Point 5: In addition to the above comments, I have included a number of handwritten comments on a scanned hardcopy of the manuscript (apologies if my handwriting is difficult to read). Please address these comments as you***

see fit.

We thank the reviewer for their suggestions. Suggested references in the scanned copy were included in the manuscript (L113-L114, L430-431). The questions raised in the copy were transcribed and answered below:

o ***L89-L92**: **Specifically, fault-based SHA neglects that periods of increased aseismic slip can release a portion of the accumulated geologic moment, influencing the timing, rupture extent and size of future earthquakes" How common is this observed in nature? What % of the total moment does it accommodates?***

We included a sentence in the Introduction section to convey that aseismic slip can accommodate a significant fraction of the tectonic strain, with estimates ranging from ~10% to 85% depending on the fault and tectonic setting (Avouac, 2015). (L95-L97)

o ***L 105: Delogkos et al., 2023, Shaw et al., 2022 [uses] hybrid loading model [and] does not assume constant slip. Traditional backslip models are not favoured anymore.***

We modified this sentence to acknowledge the recent works that implement hybrid loading approaches (L111-L114). However it is important to note that in these studies the spatial heterogeneity is generally limited to the tapering of the stressing rate near fault tips. They do not necessarily incorporate the full spatial variability of slip-rate distributions even in cases where such information is available.

o ***L 111: Which software are using?***

We used the quasi-dynamic earthquake cycle simulator QDYN (Luo et al., 2017). This was specified in the manuscript (L120-L121)

o ***L126-L128**: **"This implies that the geologic moment rate might be***

insufficient to characterize the earthquake potential of faults, calling into question some underlying assumptions in fault- and physics-based SHA." This statement may be true for traditional SHA but is not for SHA based on simulated earthquakes. e.g. Shaw et al., 2018; Milner et al., 2021. Also, Gerstenberger et al., 2024 use some physics-based data in their SHA models (e.g. Coulomb stress to define fault rupture models).

In this section, our intention was not to challenge all assumptions underlying fault- or physics-based SHA, but specifically to question the geologic moment rate assumption and the choice of a single slip rate value for the calculation of earthquake rates. We recognize that more recent approaches (e.g. Gerstenberger et al., 2024) integrate physics-based information such as Coulomb stress for recurrence estimations (e.g. Toda et al., 1998) or multifault rupture probabilities, which are beyond the scope of our manuscript focused on slip-rate spatial variations. These references are included in the text where relevant (L99-100; L-113-L114).

o ***Figure 1: Show slip rate profile?***

Slip rate profiles are shown in Figure 3.

o ***Figure 1**: **Difficult to see yellow trace***

The trace color was changed to blue to improve its visualization.

o ***L160: (...) while maintaining kinematic consistency with the fault longterm slip rate. What do you mean precisely?***

The sentence in the Methods was reformulated to convey that the method permits the relaxation of accumulated stresses while maintaining the target fault long-term rate (L168-L171). The implementation of backslip of Heimisson (2020) used in QDYN differs from the RSQSim backslip implementation in that faults do not have to slip backwards to determine the backslip stressing rate.

The problem is formulated such that the average steady-state slip-rate at any point is also the loading rate.

o ***L216**: All events? How big are these events?*

We utilize the recurrence time of all seismic events. This is now mentioned in the manuscript (L247). Full-rupture events have a Mw of 6.2, as described in L314-L319.

o ***L274-277**: How often do we observe this in the real world?*

Bilateral ruptures are favored in our models because stress conditions are uniform on the center of the fault. An initial bilateral rupture phase is a behavior observed in recent earthquakes (e.g. 2025 Mandalay Earthquake, Inoue et al., 2025; 2021 Maduo Earthquake Liu et al., 2024; 2023 Kahramanmaraş earthquake, Liu et al., 2023)

o ***L328: PGA typically plotted in log space – why have you used a linear axis here?***

The x axis in Fig. 5a was changed to log scale.

o ***L416: Did you use a logic tree approach for this?***

As noted in the manuscript, we did not apply a logic tree approach to evaluate the epistemic uncertainties of the PSHA analysis, but we acknowledge that incorporating this type of analysis should be the target of future work (L489-L493).

o ***L419: Standard GMPEs don't account for variable fault slip. Can you confirm that you do?***

The GMPEs used in this manuscript are regression-based, relating ground motion parameters to specific parameters, including Mw, distance to the fault, faulting style, site conditions and region-specific terms. As such, the GMPEs intend to fit the average behavior of earthquakes, regardless of their slip distribution. However, heterogeneous slip rate distribution can indirectly influence rupture propagation and promote directivity effects, which in turn might affect ground motion. Therefore, when working with faults with large spatial variability of slip rate, it is important to consider GMPEs that incorporate directivity effects. We added this point the discussion section (L504-L506).

<u>Reviewer #2</u>

First, I want to acknowledge the opportunity to review the article titled "Spatially heterogenous Holocene slip rates drive seismic sequence variability on normal faults" authored by Rodriguez Piceda and co-authors.

The article conducts an analysis of the impact that along-strike slip rate assumptions have on earthquake cycle simulations and on seismic hazard estimates. The article tackles an important but often overlooked topic in SHA by employing a suitable experimental design that systematically explores slip rate assumptions on a fault in Central Italy. The results clearly depict how slip rate assumptions are not trivial for physics-based SHA, as they condition how earthquake ruptures nucleate and propagate (in a physics-based framework) and, therefore, how their long-term statistics are defined (e.g., MFDs). The article is a relevant contribution to the growing scientific community working on physics-based SHA models. The article will also likely be of interest to the SHA community in general, although the contribution to this field is not novel; previous works co-authored by the some of the authors signing the present paper have already tackled this problem with a similar approach (Faure Walker et al. 2019).

We thank the reviewer for the constructive feedback and address their comments below.

I have only a few comments/questions:

Point 1: Model parameters

The article uses QDYN, a rate-and-state quasi-dynamic earthquake simulator. Like all earthquake simulators, a big aspect is the model parameter selection. The set of parameters that constrain the models (friction coefficients, initial stresses, etc.) have a large impact on the simulated earthquake catalogues (Mmax, MFD shape), rupture propagation characteristics and agreement with earthquake scaling observations (e.g., scaling relations). I think that discussing a bit more on the reasoning behind some of the parameter selections for the models would strengthen the paper, especially the a and b coefficients of the rate-and-state friction law and the width of the velocity-strengthening regions. Are the a and b values standards based on laboratory experiments? How reasonable/appropriate are they for the region studied in Central Italy? Would different values impact the outcomes of the paper in terms of MFD or earthquake sequence behaviour? I find the information on the Supplementary material helpful but not complete in this regard.

The chosen a/b ratios are consistent with values from laboratory experiments on carbonatic-rich gouges (Chen & Spiers, 2016). This was added to the Methods section (L206-L207)

Following the reviewer's comment, we carried out a sensitivity analysis of the modelling results testing the sensitivity of the magnitude and recurrence across the different rate profiles to different a/b ratios (Figs. S3-S7). We included a paragraph describing the results of these alternative models in the Results section:

"The sensitivity analysis on the effect of rate-and-state frictional parameters on the seismic behavior yield consistent patterns across all tested a/b ratios (Figs. S3-S7). In each set of models with different long-term slip rate profiles, those with the 'all-data' slip-rate profiles show broader magnitude frequency distributions and higher coefficients of variation in recurrence intervals compared to the simplified slip-rate profiles. Among the simplified slip-rate cases, triangular profiles most closely reproduce the behavior of the 'all-data' models. Notably, in models with lower a/b ratios (i.e., more velocity-weakening) the differences in the seismicity magnitude and recurrence decreases. In these models, the resulting magnitude-frequency and recurrence distributions show less sensitivity to the imposed long-term slip rate profiles, yielding more consistent behaviors across slip-rate scenarios compared to the reference model."

Additionally, we included a paragraph in the Discussion section addressing the implications of these results:

"The sensitivity analysis on frictional parameters shows that more-velocity weakening faults, characterized by lower a/b ratios, produce earthquake sequences that are less influenced by the choice of long-term slip-rate profiles. Although some differences persist, recurrence intervals and magnitude-frequency distributions are more similar across slip-rate scenarios. In these models, rupture nucleation and propagation are primarily controlled by the strength of the attractor of the frictional limit cycle than to the spatial pattern of loading (Barbot, 2019; Cattania, 2019). In other words, when a/b is lower than 0.5, the system exhibits a more stable limit cycle, and is less affected by external loading heterogeneities."

Point 2: Slip rate vs. fault geometry

First, as the paper acknowledges, the slip rate variability along strike has been previously linked to fault geometric complexities like bends or relay zones (some references are cited in the manuscript). Some of these complexities have to do with near-surface effects and might not be extrapolable to the full seismogenic thickness of the fault, where faults are presumably geometrically

simpler (higher confining pressures, transition to ductile regimes, etc.). In practice, this means that some of the slip rate variability along strike might be the response to localized stress concentrations induced by shallow geometric features, which might amplify or reduce the slip. In the paper, it is assumed that surface along-strike slip rate variability is persistent throughout the whole fault plane, even at depth. What is the rationale behind this assumption?

The slip rate values used are taken from measuring the Holocene surface fault scarps which have been produced by numerous earthquakes and should therefore be a good representative rate. While we agree with the reviewer's perspective, we have no constraints on the slip rate distribution at depth over similar time periods, and thus we would have to make a series of unfounded assumptions to modify the slip rates. Additionally, the approach we used has been used by other modelling studies in the region, (Mildon et al., 2022; Wedmore et al., 2017) e.g. Mildon et al 2022, Wedmore et al 2017. In the absence of robust constraints about the slip rate distribution in depth, surface slip-rate values were extended uniformly downdip. This was added to the Methods section (L229-L232). This model design also permits a more straightforward interpretation of the results, as the variation only occurs along one dimension instead of two.

Second, the slip rate profiles explored in the paper are based on surface measurements on a geometrically complex fault but imposed onto a planar fault model. This modelling choice removes the possibility of simulating the physical effects of fault roughness or geometric complexity on earthquake nucleation, propagation, or arrest, all factors which might themselves be responsible for generating along-strike slip rate variations in the long-term. In other words, surface slip rate and fault geometry might not be independent parameters. Ignoring one of them could be conceptually artificial, especially when the

modelling is focused on generating realistic earthquake sequences for hazard assessments.

This interdependence is evidenced in previous studies. For instance, Allam et al. (2019) showed that surface slip variability over seismic cycles can emerge purely from geometric complexity (fractal roughness), even when the alongstrike slip rate is uniform. My question is: could the slip rate variability observed in the field be reproduced by simulating a fault with realistic geometry and a uniform slip rate? While I understand the computational limitations of implementing geometric complexity within the FFT framework in QDYN, it would add depth to the manuscript if this limitation was explicitly acknowledged and discussed.

We acknowledge that, although slip rate profiles are imposed on a planar fault model, the field-based data is derived from a geometrically more complex fault and that fault geometry can strongly affect rupture dynamics and short-term slip distributions. As it is currently implemented, QDYN cannot accommodate along-strike geometric variations on dipping faults with a free surface. However, it is worth noting that variability in coseismic slip (e.g. Allam et al., 2019), does not necessarily imply equivalent variability in long-term slip rate. Over many earthquake cycles, the effects of rupture complexity may average out, and the cumulative slip distribution can remain relatively smooth. This distinction is important, as the Holocene slip rate measurements we use reflect the integrated effect of many events.

While fault geometry and long-term slip rate are potentially interdependent, geometry is not the unique factor affecting slip rate variability, but also it might be influenced by interactions with adjacent faults (Gupta & Scholz, 2000; Nicol et al., 1996; Peacock & Sanderson, 1991), lateral variations in off-fault damage (Cappa et al., 2014; Perrin et al., 2016), or downdip linkage with pre-existing structures (Nixon *et al.*, 2014; Phillips *et al.*, 2016), as pointed out in the Introduction section (L73-L78). Therefore, our modelling approach focusses on the role of slip rate heterogeneity, independent of geometric effects, allowing us to isolate its influence on earthquake recurrence and magnitude distribution. We highlight in the Discussion that including geometric complexity would be an important direction for future work (L489-L509).

On the same slip rate note, slip rate variability is not only defined by along-strike variations, but also down-dip. Slip rate usually tapers toward both the surface and the base of the seismogenic layer (see Finocchio et al., 2016) and including this variability has also an impact on earthquake sequence characteristics (e.g., Delogkos et al., 2023). I suggest the authors to consider discussing this aspect as well.

Following the reviewer's suggestion we have included two additional simulations using the 'All-data' and 'Triangular max' slip rate profiles and incorporating a downdip distribution in which slip rates taper from the surface file-based values to 0.001 mm/yr at the base of the fault with a 100% increase at mid-depths. This set up reflects observations from single-event slip distributions and depth-dependent seismicity rates (e.g., Ragon et al., 2019; Delogkos et al., 2023; Scognamilio et al., 2018). The set up of these models is described in the methods section (L236-L240), while an additional figure showing the magnitude-frequency and recurrence distributions was included in the supplementary material (Fig. S8).

Models with downdip variations in long-term slip rate tend to produce more regular earthquake recurrence and characteristic magnitudes than those loaded with uniform slip rate in depth. This is likely because the tapering towards the surface and the bottom of the fault reduces slip-rate variability along strike at those depths, thus lateral propagation is less likely to be arrested promoting full ruptures with characteristic magnitudes. In these models, events also tend to nucleate at intermediate depths (e.g.

Video S14-S15, RP25), where the prescribed long-term slip rate peaks, which aligns with observations from single-event slip distributions and depth-dependent seismicity rates (Delogkos et al., 2023; Finocchio et al., 2016; Ragon et al., 2019; Scognamiglio et al., 2018). However, it is worth noting that models with uniform downdip loading but variable along-strike loading can also show depth-variable nucleation, with events nucleating at intermediate depths. This suggests that a variable downdip slip rate is not strictly necessary to reproduce the observed downdip patterns of seismicity and slip. These aspects were addressed in the Discussion section (L489-L509).

Other minor/formal comments

• Point 3: The paper uses the term GMPE. Given that generally a GMPE (e.g., Bindi et al., 2011) is composed of several equations, I think the term GMM (Ground Motion Model) is more appropriate. Moreover, the GMM term is increasingly being used in literature compared to GMPE.

We changed the term “GMPE” by “GMM” throughout the manuscript (L262, L490-L491, L506)

• Point 4: The Supplementary text contains a grammatical error and font size change in Page 4: “apporixmatelly”, and a reference highlighted in yellow in page 7.

This paragraph was removed from the supplementary material, since it corresponds to the Methods section in the main text.

• Point 5: The citation “Rodriguez Piceda et al. (2025)” of the Supplements is not listed in the references of that document.

The reference is Rodriguez Piceda (2025) and corresponds to the input files to run the simulations, and it is included both in the reference list of the manuscript and the supplementary material.

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