Responses to Referee #1

In the following document we provide detailed responses to the different comments and explanations on how we will implement this in a revised version of the manuscript.

General comments:

The paper by Gómez-Novell et al investigates the role of fault geometry in the probability of surface rupture along a fault, using the RSQSim earthquake simulator. More specifically, the authors analyze how fault connectivity at depth and fault sinuosity both at surface and depth drives the magnitude frequency distribution, max magnitude and probability of surface rupture. The Mt. Vettore (Central Italy) area is taken as a test area, to run the models and compare the outputs with coseismic and long-term slip.

I enjoyed reading the paper and I want to congratulate the authors for putting together such interesting research. The text is properly organized, figures are illustrative and well-detailed in the text. The paper is of high significance, since it applies earthquake cycle simulators to the evaluation of surface rupture probability; to my knowledge, it is the first attempt in the literature, and the paper could pave the road for a wider application in PFDHA, by providing an alternative approach with respect to those more commonly applied. Given the above, I suggest accepting the paper following minor reviews.

Below I list some comments which I hope may be useful for the revision stage.

Author's response (AR): We kindly thank the reviewer for the positive and optimistic comments on our manuscript and its significance to PFDHA.

Specific comments:

Lines 10-12: this sentence may benefit from rephrasing. The likelihood of surface rupture is just one of the several components needed to run a PFDHA. Overall, PFDHA estimates the likelihood of exceeding a given displacement value, usually expressed as annual frequency of exceedance.

AR: We agree. We will better detail this in the revised version of the manuscript.

Introduction: consider to add the references to the recently published IAEA Tecdoc 2092 https://doi.org/10.61092/iaea.74us-dn4n and the paper by Valentini et al 2025 (10.1029/2024RG000875)

AR: We thank the reviewer for highlighting these recent references. None of them were published when we submitted the manuscript, so we will make sure to implement them in the revised version.

Lines 62-63: compared to many (most of?) other active faults worldwide, the Mt Vettore fault geometry is quite well-constrained. I would not say that data on the subsurface geometry is not available, given also that the fault was responsible for the widely studied 2016 earthquake sequence.

AR: While several models of Mt. Vettore's subsurface geometry have been proposed, especially since the 2016 earthquake sequence, we recognized little consensus on a preferred geometric model. This lack of agreement is evident in studies such as Tung and Masterlak (2018), who tested multiple source geometries to explain the August 2016 earthquake, underscoring the coexistence of different models. For instance, Lavecchia et al. (2016) propose a model, (used as a reference for our segment-connected configurations), where fault segments connect at ~7km with two distinct dip domains: 60º between 0 an 8 km, and 45º below 8km. In contrast, models by Cheloni et al. (2019) or Falucci et al. (2018) assume gradual listric geometries for the Mt. Vettore fault but do not discuss how or whether fault segmentation is linked at a specific depth. This lack of consensus combined with the exceptional body of surface geological data in the region is what makes the Mt. Vettore a good candidate for our study.

In the revised manuscript, we will clarify this rationale to emphasize why the Mt. Vettore fault was selected, while still acknowledging the existing models.

Section 2.2. Can you provide a numerical measure of the different degrees of sinuosity in your models? Something like the sinuosity index of a river. It may help the reader to grasp the variability among models.

AR: This comment links with reviewer's #2 comment on model nomenclature. In the revised manuscript, we will adopt a new nomenclature that includes a numerical attribute to describe sinuosity: 0, 0.3, 0.6 and 1 to describe all models from minimum to maximum sinuosity, respectively. With this, we do not think it will be necessary to compute the actual sinuosity of the models because this new nomenclature is intuitive and will already help readers understand the variability.

Section 2.2. Why have you selected the "fault" level of the Central Apennine database instead of the "trace" level? Is it a matter of model resolution (i.e., the 300 m wide fault elements)?

AR: Yes, but not only. Model resolution is an important reason for choosing the fault level over the trace one. The trace level implies smaller order geometric complexities that would require finer meshes and a substantial increase in computational cost. More importantly, at Mt. Vettore the trace level is strongly biased toward the 2016 surface rupture locations. In those areas, the mapping is very detailed, including secondary ruptures whose depth geometries and connections remain unresolved. Incorporating such features into RSQSim would require speculative assumptions about their subsurface relationships, introducing subjectivity into the model. With the fault level we achieve a better balance between model activity related to the main fault structure (not secondary, smaller ruptures), while preserving the geological segmentation described in literature.

Section 2.3.3. Several slip rate estimates at surface are available for the fault segments from Cupi to Mt Vettore; they cover different time intervals (e.g., post-galcial; long-term geological). Since you need to define the slip distribution to run the model, I'm wondering if considering a distribution directly derived from the surface measurements along the entire 40-km long system (instead of the distribution detailed at lines 163-170) could make an impact on the obtained results.

AR: The reviewer raises a very good point in this comment. Prescribing a more complex slip-rate distribution could indeed influence the results, though we expect the effect to be limited. Slip rate primarily governs: 1) the locations where earthquakes preferentially nucleate over long time spans (that is why we our use a tapered distribution at the fault edges to promote nucleation at realistic seismogenic depths), and 2) the total slip on each patch, which should converge to the prescribed value by the end of the seismic cycle (assuming full coupling).

In our case, modifying slip rate at surface would mainly affect the long-term cumulative slip distribution along-strike, which would reproduce the one prescribed. However, slip rate is not the sole control on cumulative slip. As shown in Fig. 12, despite imposing a parabolic slip-rate profile along-strike, the long-term cumulative throw distribution for Mw ≥ 5.5 events is strongly influenced by geometric features such as segmentation and bends, which locally reduce throw values. This indicates that a simplified input distribution already reproduces most of the observed cumulative throw patterns.

Introducing more complex slip rate distributions from surface measurements might slightly improve the fit between model and observations shown in Fig. 12, but it would also make it harder to isolate the effect of geometry on the resulting throw distribution. This would compromise independence between model and observations for comparison.

Line 225: I had some difficulty in understanding the meaning of Mmax in your condition rule. One needs to know that the Mmax obtained in the models is always lower than 6.6 (i.e., M2016 + 0.1), however this info is provided later in the text

AR: We agree with the reviewer and we will further clarify this in the text where the condition rule is specified. In detail, we will introduce that we use Mmax as a constrain because the maximum magnitude of the models is always around or below the Mw of the 2016 mainshock, and we will refer to the specific section of the text (section 3.1.3) where we explain the Mmax of each catalogue.

Line 229: delete one "them"

AR: We will fix this in the revised manuscript.

Line 375 (figure 10): nice image, thanks for providing this figure. I found myself moving back and forth between figures 9 and 10, to compare the regressions and the along-strike variation in rupture probability. I found it quite interesting such comparison. For instance, in the connected constant models, the trace-linear configuration seems to have a higher probability in fig 10; however, in fig 9, at Mw 6.0 the trace-trace model lies above the trace-linear. Consider the possibility to add in figure 10 a label or colored dot representing, for each model, the probability of surface rupture at Mw 6.0 extracted from figure 9.

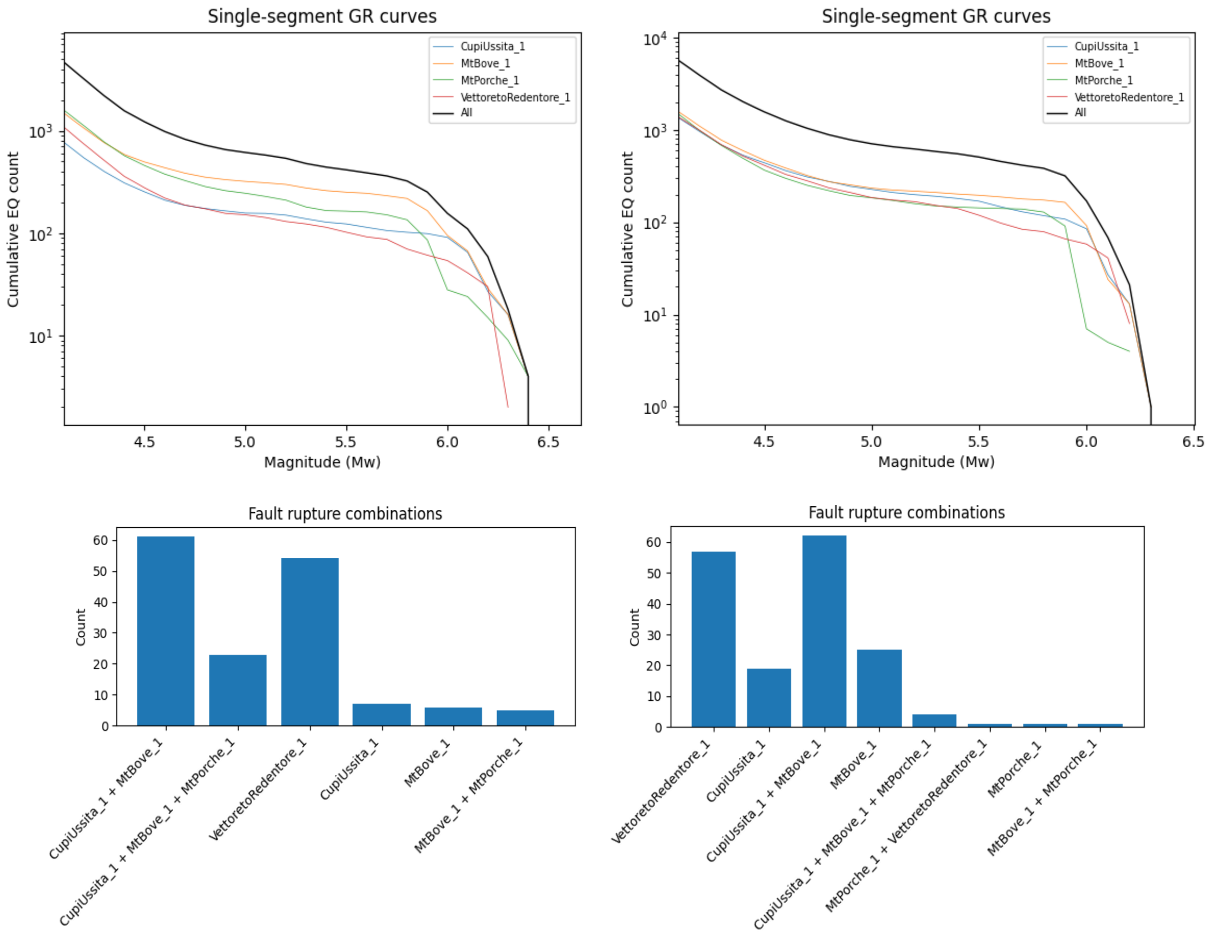
AR: We thank the reviewer for the positive feedback on the figure. In our opinion, adding the probability of Mw 6.0 can be confusing, because figure 10 aggregates probability for magnitudes >=6.0, while in figure 9 probabilities are bin-specific. The discrepancy between probabilities in both figures is an effect of the regression fit to the data points. In figure S4 of the Supplements we show the actual data points used to fit the regressions. In the connected constant trace-linear model, the data points of Mw 5-6 show a drop in the probabilities which overall lower the regression in comparison to the trace-trace model (see figure S4). However, if we look at these data points, we actually observe that the trace-linear model has higher probability for Mw 6.0 than the trace-trace model. This is consistent with figure 10, which shows the actual data points, not a regression fit.

Lines 380-385: the surface rupture probability depends on the fault area; in the disconnected model, this is proportional to fault length. In figure 10, Mt Bove shows the highest probability. Does this depend on the adopted Mw-area relation? I mean, using the Thingbaijam et al relation, a Mw 6.0 corresponds to roughly 200 km2; with the 12-km seismogenic thickness, it means a ca. 15-km long fault. Is this the size of Mt Bove segment? Is the Mt Porche area (disconnected linear-linear model) enough to generate a Mw 6.0 event?

AR: Earthquake simulators do not use empirical scaling laws to derive magnitudes from fault parameters. Instead, they rely on physical equations that integrate rock friction and slip rate to model tectonic loading, nucleation and rupture throughout the earthquake cycle. In the simulations, the magnitude-rupture area relation emerges from the input fault frictional properties (e.g., rate and state parameters, initial stress conditions). In our study, we verify that such simulated Mw-area relation is consistent with empirical scaling laws, but the relation is not part of the computation.

Having said that, the Mt. Bove shows the highest probability because it is in the central section of the fault where slip rate is greatest, leading to more frequent nucleations (see figure 3a). The seamless probability transition between the Mt. Bove and Cupi Ussita segments indicates that Mw>=6 often propagate across these two segments. Conversely, the sharp probability drop at the Mt. Porche segment suggests that Mw>=6 do not propagate into or out of this segment.

The following figures show the GR distributions for each segment in the Disconnected Linear–Linear and Trace–Trace models, along with the frequency of single- and multi-segment ruptures for Mw ≥ 6. Single and multi-fault ruptures at the Mt. Porche are consistently the less frequent. This likely reflects 1) its geometric configuration, preventing rupture propagation, and 2) its smaller size compared to the other segments. Regarding the reviewer's question on segment size: in the Linear–Linear model, Mt. Porche does not generate any Mw ≥ 6 ruptures.



Left column: Disconnected Linear-Linear / Right column: Disconnected Trace-Trace

Line 469: here you highlight the importance of constraining fault traces at surface. In some cases, different interpretations may be present. For instance, the CAD fault database is the result of a big effort in data harmonization by several groups which may have mapped the fault traces in a slightly different manner. Do you have any hint on how to incorporate such uncertainty into the modeling setup?

AR: Earthquake simulators are deterministic in terms of model set up and constraints, meaning that uncertainties are not intrinsically explored. Similar to what we do with subsurface geometry, one could indirectly explore such uncertainties by proposing a suite of different fault trace hypotheses and perform a sensitivity analysis to determine their impact into the results. For instance, an analysis like the one we mention is performed by Zielke and Mai (2025). In order to incorporate these epistemic uncertainties, a logic tree approach could be implemented were several conflicting interpretations are available.

Line 494: typo in broadest

AR: We will fix this in the revised manuscript.

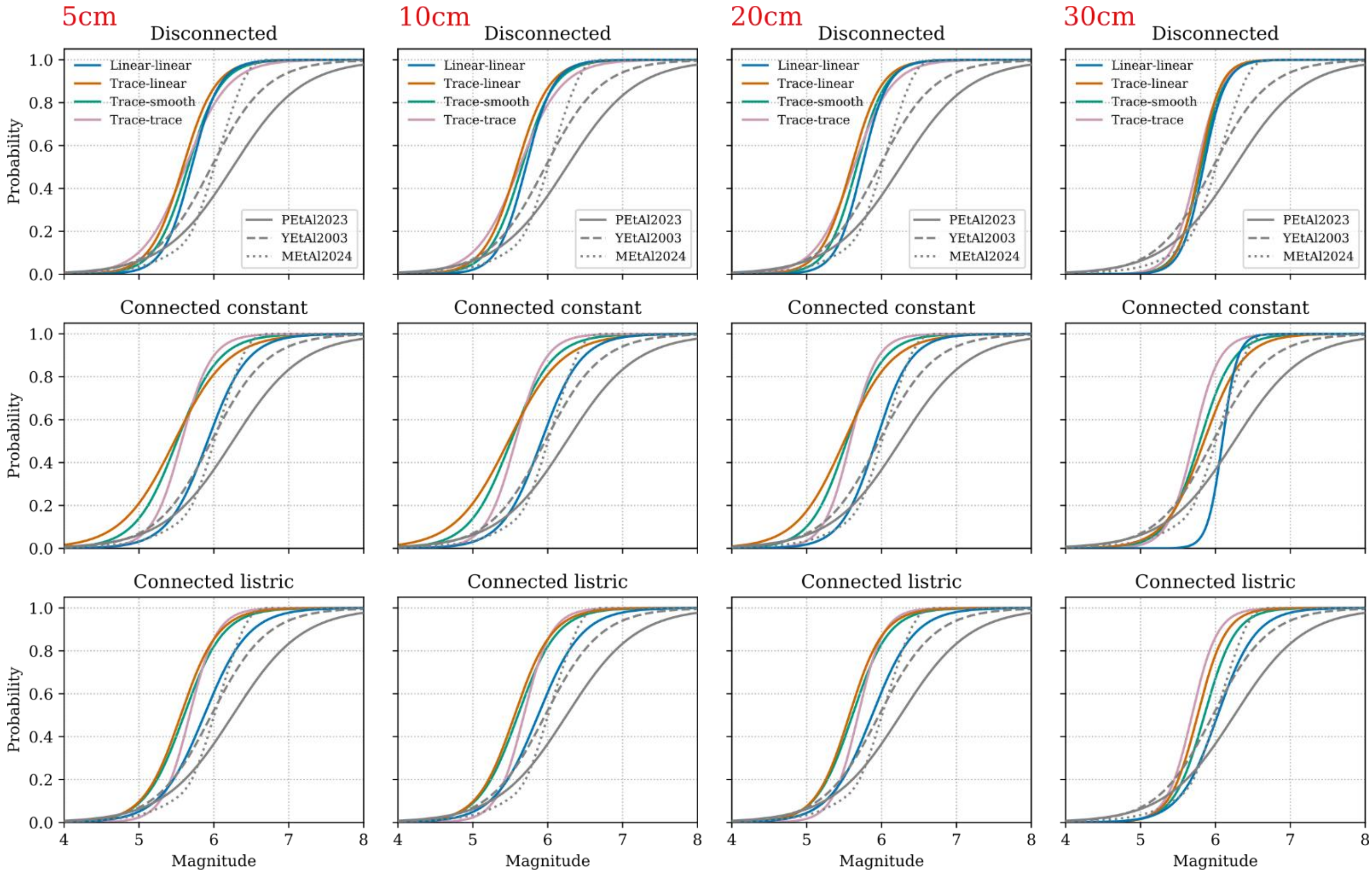
Lines 536-540: another factor at play to explain the lower probabilities obtained by empirical models could be the role of local properties and in particular near-surface materials. Loose sediments or weak rocks favor an accommodation of slip by tilting/warping rather than brittle fracturing.

AR: This is a very interesting point. We will add it to the discussions (after line 539) in the revised manuscript.

I acknowledge that it is beyond the scope of the paper, but as a side note I think it may be interesting to investigate the amount of surface slip in your surface rupturing events, and to see to which extent retaining only events with slip higher than (say) 5 cm moves the regressions toward higher magnitudes.

AR: This is an important point that we considered during the development of the study. We chose not to impose a slip threshold to avoid introducing arbitrariness in its selection. Moreover, damage-relevant offsets vary across infrastructures and engineering applications, and since our work has a scientific rather than engineering focus, we decided against including a threshold.

We attach the regressions of Fig. 9a computed with four slip thresholds (5 cm, 10 cm, 20 cm, 30 cm). The regressions are identical for all cases except for the 30 cm threshold, which is, obviously, only considered here for comparative purposes. The stability of the regressions, even at relatively large slip thresholds (i.e., 20cm), indicates that the simulated surface rupture behaviour is realistic, and does not produce unrealistically small surface slips.



Line 593: in figure 8 you consider earthquakes with Mw > 4.0; in figure 10 Mw > 6; in figure 12 Mw > 5.5. I understand the reasoning behind such choices, but explaining this aspect earlier in the manuscript could enhance clarity.

AR: Agreed. We will clearly specify the rationale between the different magnitude threshold selections in section 2.5, where we explain the methods conducted for the catalogue analysis.

Section 4.5. I agree that earthquake simulators can overcome some of the limitations of empirical datasets. However, the method applied in this paper requires quite detailed site-specific data, which may not be always available: do you think this aspect can limit the applicability of earthquake simulators for PFDHA studies? For instance, the earthquake approach in PFDHA is much more used than the displacement approach (Youngs et al 2003).

AR: We thank the reviewer for raising this point. Our study focuses on a region with abundant site-specific data, which we used primarily to evaluate how well our simulations reproduce observations and surface rupture behaviour. We agree that limited site-specific data can constrain the development and validation of any methodology, including ours. However, most of the datasets we use are not required to run the simulations but to assess their performance. Therefore, the applicability of earthquake simulators is not inherently restricted by the availability of detailed local datasets. On the contrary, in cases with scarce site-specific data (e.g., single-event displacement datasets for the displacement approach), earthquake simulators may provide an alternative to the earthquake approach in PFDHA. As shown in this study, simulator tools may also enable uncertainty and parameter sensitivity exploration, with large datasets that can strengthen statistical analyses.

That said, as in any hazard study, a minimum level of information is necessary to properly constrain the simulations and avoid speculative assumptions (e.g., fault slip rates, fault mapping and geometry). In the revised manuscript, we will clarify this limitation and better define the role of earthquake simulators within this framework.

Lines 643-644: Fault-specific analyses in PFDHA are better addressed with the displacement approach rather than the earthquake approach.

AR: Noted. Simulators provide the ability to generate populated rupture datasets with high resolution fault displacement data, which would enable fault-specific analyses with the displacement approach. We will specify this better in the lines highlighted.

References mentioned:

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