

Responses to Referee #2

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.

The probability that an earthquake becomes a surface-rupturing event is a key ingredient in probabilistic displacement hazard analysis. Robust estimates of these probabilities are limited by the scarcity of surface rupturing events. Understanding how different fault properties, such as geometry, connectivity at depth, or sinuosity, affect this probability, is hampered by the lack of detailed observations at depth. Gomez-Novell et al. bring an innovative approach to this data gap. They use rupture simulators to test the effect of different fault geometries at depth on the probability that an event becomes a surface rupturing one on the Mt Vettore fault in Italy. Their study highlights how geometry influences the probability of surface rupture and offers a pathway to incorporate inferences from simulators into PFDHA. The contribution is original and useful and I support eventual publication.

Author's response (AR): We kindly thank the reviewer for the very positive feedback on the article.

I have some minor comments that are mostly focused on improving the clarity of the article:

Figure 2: the segmentation and smoothness degrees the authors test are very reasonable but I find the trace-trace trace-smooth etc. wording to be very confusing. I think the suite of geometries may be captured by two constraints: a segmentation (n of separate segments) constraint, and a roughness (for example, RMS roughness as used in fault roughness studies). These would describe the suite of geometries quantitatively and remove the confusion infused by the naming choices.

AR: This comment highlights a confusing selection on model nomenclature that we agree with and, as such, we will take appropriate measures to solve it in the revised manuscript. In detail, we will adopt a parametrized nomenclature. Letters for the connectivity level: D- Disconnected, C-Connected constant and L-Listric; increasing numeric values for increasing sinuosity/roughness of the fault, going from 0 (minimum sinuosity) to 1 (maximum sinuosity). As such, the models will become:

- Disconnected: D0 (Linear-linear), D0.3 (trace-linear), D0.6 (trace-smooth), D1 (trace-trace).
- Connected constant: C0, C0.3, C0.6, C1
- Listric: L0, L0.3, L0.6, L1

We believe this nomenclature for the sinuosity is more intuitive, helping readers to clearly understand the models that have higher or lower sinuosity, without the need to numerically compute it from the models. In the manuscript text (section 2.2), we will provide a proper introduction to this new nomenclature, and we will replace all mentions to the former nomenclature with the new one (figures and text).

Line 229 typo in extra them

AR: We will fix this in the revised manuscript.

Figure 7 - I don't understand why the connected listric would produce larger magnitude events than the connected constant - isn't the listric geometry quite unfavorable for slip propagating into those regions?

AR: The listric model produces larger magnitude events than the non-listric because the fault rupture area available is larger. Even though the listric geometry causes dip variations at depth, these dip variations are not very large and quite gradual. Consequently, the fault plane stresses likely do not show sharp transitions at depth enough to prevent ruptures from propagating.

Figure 8 - telling these models apart visually is a bit hard. Can the authors fit a logistic regression to highlight the differences between the two end-member models? Should be easy to do since the authors do it anyway to provide the parameters in the next table and have the regressions in Fig 9.

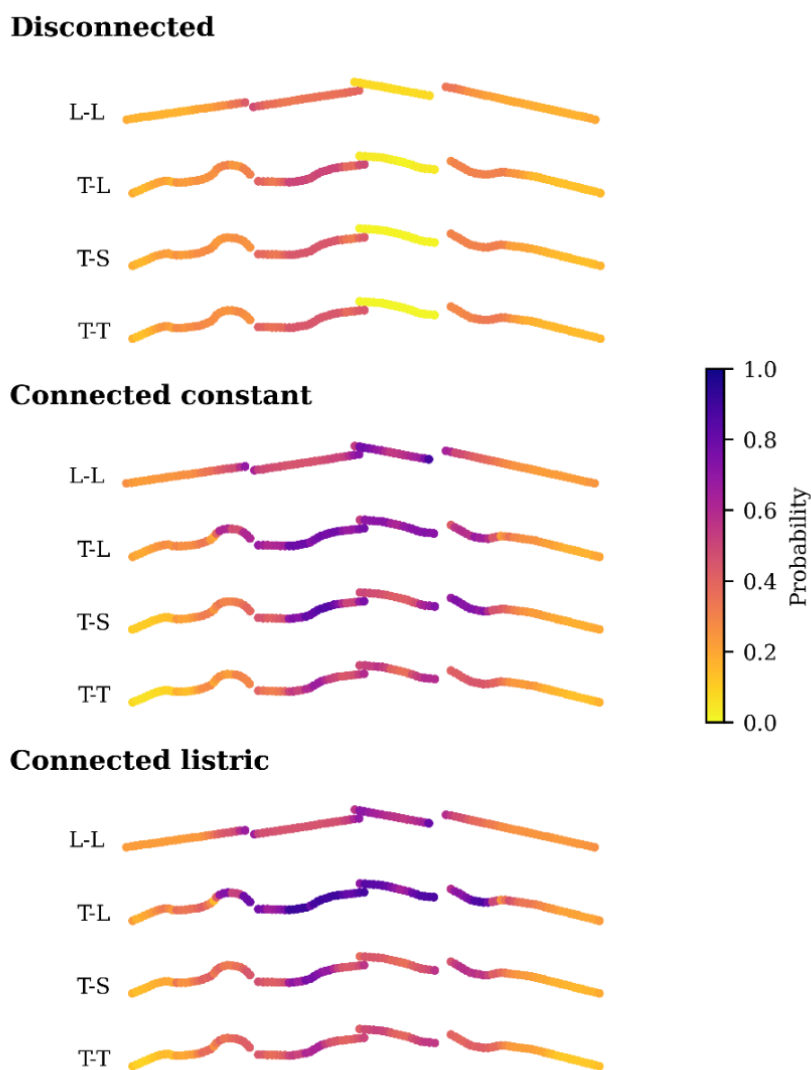
AR: The purpose of this figure is to show that earthquake rates for a given time period do not correlate with surface rupture probability, which is evident when looking at the magnitude bin-specific probabilities. While a logistic regression might improve the visualization, it would represent a fit to the data rather than the data itself. Such a fit can deviate from the actual values and potentially mask the rate-independence relationships we want to highlight.

Line 327 - the authors point out that a and b are not the rate and state friction coefficients but the intercept and slope of the logistic fits. This is a useful consideration. They should also point out that a and b are not the parameters in the magnitude-frequency distribution, since this is another possible source of confusion given the nature of the article.

AR: Agreed. We will add this clarification in the revised version in the manuscript.

Figure 10 - consider not using a divergent color map, since the probabilities go from 0 to 1.

AR: We will consider this for the revised manuscript. We will use a perceptually uniform sequential colormap like “plasma”, as we show below.



I appreciate how this article weaves the modeling results with the results from empirical studies in the literature.

AR: We are thankful for the positive feedback on this comment.

The authors could refer to Valentini et al. (2025)’s call for more model-driven advances to supplement current PFDHA approaches as part of the justification for this work.

AR: We agree and we will add this in the revised manuscript version, specifically in section 4.5. Please note that Valentini et al. (2025)’s paper was not published when we submitted the manuscript.