I thank the Editor and the reviewers for their careful evaluation of the manuscript and for their constructive and insightful comments. Below, I provide a point-by-point response to all reviewer comments. The reviewers' comments are reported in black, while my responses are highlighted in **blue** for clarity. In addition, the manuscript has been substantially revised to address all the issues raised during the review process. In particular, a new section entitled "Discussion" has been added to the manuscript to explicitly account for and integrate the main conceptual, methodological, and interpretative points raised by the reviewers.

REVIEWER #1

Dear Editor, Dear Author,

I have completed my review of the manuscript "Relaxing the Separation Between On-Fault and Off-Fault Seismicity Using an Empirical Mw–Deformed Area Relation" by Alessandro Valentini.

This manuscript deals with an important, yet poorly discussed, topic in fault-based PSHA: how to integrate fault models in regional to national ERFs, considering the completeness of the fault model and the difference in minimum magnitude between faults and distributed seismicity.

This article updates a published approach (by the same Author) with a physically motivated method to integrate fault-based and distributed seismicity, overcoming the unrealistic sharp separation between on-fault and off-fault models.

The proposed method sounds applicable, and it is adequately described. The manuscript is well-written and supplemented with informative figures. It is in an advanced stage, and I have only very few comments before publication.

Spatial distribution of the residual deformation area

I would like to suggest that the manuscript further clarify how the residual deformation area (obtained by subtracting the surface projection of the fault from the total deformed area) is spatially distributed around the fault.

In particular:

- It is clear that the assumption of the residual area is uniformly distributed around the fault projection, however InSAR observations often show strongly asymmetric deformation patterns, especially for dip-slip faults (e.g., hanging-wall dominated deformation). It would be useful to explain how this asymmetry is or could be in future incorporated in a general operational framework.

I thank the reviewer for this insightful comment and fully agree that the spatial distribution of the residual deformation area deserves further clarification. In the current formulation, the residual area is distributed uniformly around the surface projection of the fault plane only as a simplifying geometric assumption to define a deformation-based buffer. However, the surface projection of the fault plane itself is considered an integral part of the total deformed area. As a consequence, when deformation is evaluated with respect to the surface fault trace rather than the full fault-plane projection, the resulting deformation footprint naturally exhibits an asymmetric pattern. This effect is clearly illustrated in the application example presented in Section 4, where the Paganica normal fault produces a deformation field characterized by a larger hanging-wall contribution, consistent with InSAR observations of dip-slip earthquakes. I have now explicitly clarified this point in the methodological section, emphasizing that the asymmetry between hanging wall and footwall deformation is implicitly incorporated through the fault-plane geometry. I also agree that future developments could refine this approach by explicitly parameterizing the asymmetric propagation of deformation based on InSAR-derived observations. For instance, empirical relations describing the preferential distribution of deformation toward the hanging wall could be used to construct more realistic, directionally dependent deformation buffers. We have added a short discussion on this perspective in the revised manuscript.

- providing some details on how the buffer geometry could be constructed for complex or segmented fault geometries. The paper illustrates the method using a single buffer width value, but it is not fully explicit how the approach should be implemented for curved, branched, or segmented fault traces.

The proposed approach is not restricted to simple or planar fault geometries and can be directly applied to curved, segmented, or branched fault traces. In practice, the deformation-based buffer is constructed locally around the surface projection of each fault segment, following its mapped geometry, rather than assuming a single idealized fault trace. As already described in the manuscript, when deformation buffers associated with multiple nearby fault segments or distinct faults overlap, the reduction of distributed seismicity rates is applied independently for each buffer. The tapering weight is computed separately for each fault and combined multiplicatively at each grid point. This formulation naturally accommodates complex fault networks and segmented geometries, and results in a stronger reduction of distributed seismicity where the deformation influence of multiple faults overlaps. To improve clarity, we have expanded the manuscript to explicitly state how the buffer geometry is constructed and applied in the presence of curved, branched, or segmented faults.

Sensitivity and choice of the taper exponent

I would like to suggest that the manuscript further discuss the choice of the taper exponent p, which controls the steepness of the reduction of distributed seismicity rates near the fault. I think it could be useful to clarify whether p should be considered a tunable parameter, and which values should be used.

I thank the reviewer for this constructive comment. The taper exponent p is indeed a key parameter of the proposed formulation, as it controls the smoothness of the transition between fault-based and distributed seismicity. In the current implementation, p is intentionally treated as a user-defined, tunable parameter whose optimal value may depend on the tectonic setting, data availability, and modeling objectives. As now clarified in the revised manuscript, $p = 1$ corresponds to a linear decay of distributed seismicity rates toward the fault, while $p > 1$ produces a progressively steeper reduction near the fault plane. Larger values of p therefore lead to stronger suppression of distributed rates in the immediate vicinity of major faults, where large independent off-fault ruptures are least likely. For example, for the same buffer width, a point located at 1 km from the fault experiences a substantially stronger rate reduction for $p = 2$ than for $p = 1$, whereas the two formulations converge toward similar values near the outer buffer boundary. Based on these considerations, we suggest using p values between 1 and 2 as a reasonable range for most applications, and we recommend performing sensitivity tests to evaluate which value best represents the targeted seismicity model. We have expanded the discussion in the manuscript to explicitly state that p is not fixed, but rather a controllable parameter that allows users to adapt the tapering behavior to their specific modeling context.

Integration of different faulting styles and complex tectonic settings

I would like to suggest that the manuscript further clarify how the method should be applied in regions characterized by complex tectonics.

In particular:

- discussing how to handle structures showing mixed or transitional kinematics (e.g., transpressional or transtensional faults), given that different regression coefficients are provided for normal, reverse, and strike-slip faulting.

In regions characterized by complex tectonics or by faults exhibiting mixed or transitional kinematics (e.g., transpressional or transtensional structures), the attribution of a single faulting style may be ambiguous. In such cases, I recommend adopting the regression derived from the full dataset (“all kinematics”), which provides a kinematically agnostic estimate of the expected deformed area. This choice allows the method to be applied consistently even where fault kinematics are uncertain, spatially variable, or evolving over time. A short paragraph has been added in the new discussion section.

- emphasizing that the choice of the regression model has an influence on the resulting buffer width and therefore on the final distributed seismicity rates.

I fully agree that the selection of the regression model directly influences the predicted deformed area, the resulting buffer width, and therefore the magnitude-dependent tapering of distributed seismicity rates. I have now explicitly emphasized this dependency in the revised manuscript, clarifying that the choice of regression coefficients represents a modeling decision that should be guided by the tectonic context, data quality, and the objectives of the seismicity model.

Transition in cumulative magnitude–frequency distributions

I would like to suggest clarifying how the proposed tapering strategy translates into cumulative magnitude–frequency distributions. It would be useful to discuss whether there is a double tapering effect: one associated with the spatial transition between off-fault and on-fault sources, and another associated with the truncation at the fault maximum magnitude (*Mmax*). A clearer discussion of how these two effects interact would improve the interpretability of the resulting MFDs.

I thank the Reviewer for this clarification. I wish to clarify that there is no "double tapering" effect in the model, but rather a smooth transition of activity rates between two distinct source components. Inside the fault-plane projection, the off-fault seismicity rates for magnitudes *M >= Mmin,fault* are completely removed to prevent double-counting. Consequently, for these locations, the cumulative MFD follows the regional background trend only up to *Mmin,fault*. Above this threshold, the MFD is exclusively defined by the seismogenic source model, which is truncated at the fault-specific *Mmax*. For points within the buffer (but outside the projection), the rates for *M >= Mmin,fault* are progressively scaled down (spatial tapering) but not replaced. This ensures that as we approach the fault, the "diffuse" contribution to high-magnitude events vanishes exactly where the "fault" contribution becomes dominant. Therefore, the *Mmax* of the total MFD at any point is dynamically bounded by either the regional limit or the fault-specific limit, depending on which source is active, without redundant tapering.

Depth dependence

I would like to suggest commenting on whether the deformation footprint and buffer definition should depend on seismogenic depth or fault dip, particularly for deep or blind faults where surface deformation may be less representative of rupture dimensions.

I thank the reviewer for this important comment. In the proposed framework, the total expected deformed area is controlled solely by earthquake magnitude (Mw) through the empirical Mw–area relation, whereas the definition of the deformation-based buffer depends on the geometry of the fault-plane surface projection. As a result, seismogenic depth and fault dip influence the partitioning between fault-projection area and residual deformation area, but not the total deformed area itself. Specifically, deeper seismogenic thicknesses and lower dip angles lead to larger fault-plane surface projections, which reduce the residual area assigned to the buffer while preserving the same magnitude-controlled deformation footprint. Conversely, near-vertical faults (dip ≈ 90°) yield negligible surface projections, in which case the entire deformation footprint is represented by the buffer. We have now clarified this behavior in the revised manuscript. Regarding blind faults, the method is primarily intended for fault sources that are capable of rupturing the surface, as also clarified in response to Reviewer #2. While the approach can technically be applied to blind or deep-seated structures, we acknowledge that InSAR-derived surface deformation may become less representative of rupture dimensions in such cases, potentially leading to an overestimation of the deformation footprint. We have explicitly stated this limitation and cautionary note in the manuscript.

Computational implementation

I would like to suggest briefly discussing the computational cost and numerical stability of the method when applied to large national-scale ERFs with thousands of faults and millions of grid points, and if a code/script is provided along with the manuscript.

I thank the reviewer for raising this practical point. The proposed method involves simple algebraic operations (distance calculations and magnitude-dependent rate tapering) applied locally within deformation buffers around faults. As such, its computational cost is modest and scales linearly with the number of grid points affected by the buffers. A closely related implementation has already been applied at the national scale in Valentini et al. (2017) for the entire Italian territory, involving thousands of fault sources and large smoothed-seismicity grids, without any issues related to computational cost or numerical stability. On this basis, we do not expect the present formulation to introduce additional computational challenges when implemented in large-scale ERF models. No standalone code or script is provided with the manuscript, as the practical implementation depends strongly on user-specific inputs (e.g., fault representations, grid resolution, and seismicity models). However, the method only requires the implementation of a simple tapering equation and the calculation of deformation-based buffers, both of which are fully described in the manuscript and can be readily integrated into existing ERF workflows.

Very minor comment

I would like to suggest updating the statement that “fault-based ERFs are now standard in high-strain regions” by also citing the most recent national models.

Some recent national models have been cited in this statement.

Best Regards,

Francesco Visini

<u>REVIEWER #2</u>

Review of Paper by Alessandro Valentini: 0000-0001-5149-2090

By Aybars Gürpinar

In general, the paper is well written and states its objective clearly. It presents a method to better integrate ‘earthquake rupture forecasts’ (ERF) into probabilistic seismic hazard studies. The author proposes to use empirically derived moment magnitude - deformed area relationships for this purpose. These relationships have been derived using InSAR observations for recent earthquakes.

At the end of the paper an example of the application of the method is provided using the data of the 2009 L’Aquila earthquake.

The paper is certainly worth publishing as it provides a novel way to deal with the issue of ‘double-counting’ of seismic events in a PSHA project.

However, the paper would benefit from using a more crisp terminology for the concepts presented.

Some examples follow:

‘Off-fault’ earthquakes need to be defined. This likely refers to ‘diffuse seismicity’, i.e. events that are not obviously associated with known seismogenic structures.

In some places, the term ‘fault’ is used without a qualification even though it would be beneficial to qualify it with terms such as ‘surface or near surface fault’, ‘seismogenic fault’, ‘secondary fault', ‘coseismic deformation’ etc. as appropriate. This would bring more clarity to the text. In this context a distinction between ‘active’ and ‘capable’ faults may also be relevant.

Regarding terminology, one reference could be the IAEA Safety Standard SSG-9 Rev.1 (2022) which addresses seismic hazards (both vibratory ground motion and fault displacement) for nuclear installations. Compliance with the IAEA definitions would also make the presented method applicable to nuclear installations which would be very desirable.

I thank the reviewer for this excellent and highly relevant comment. I fully agree that the use of clear, consistent, and standardized terminology is essential for both scientific clarity and practical applicability. Following this suggestion, the manuscript has been carefully revised to harmonize definitions and terminology with those adopted in the IAEA Safety Standard SSG-9 Rev.1 (2022). In particular, I now explicitly define *off-fault earthquakes* as diffuse seismicity not clearly associated with mapped seismogenic structures, and I consistently distinguish between different fault-related concepts, including surface or near-surface faults, seismogenic faults, secondary faults, and coseismic deformation. Where appropriate, I also clarify the distinction between *active* and *capable* faults, in line with the IAEA definitions. This revision improves the internal consistency of the manuscript and ensures compatibility with internationally recognized standards for seismic hazard assessment, thereby facilitating potential applications of the proposed method to critical infrastructures, including nuclear installations.

Aside from the terminology, one other topic that may need to be introduced and discussed briefly is uncertainties involved in the method presented. In PSHA applications representation of uncertainties is very important.

I thank the reviewer for this important observation and fully agree that the explicit discussion of uncertainties is essential, particularly in the context of PSHA applications. In the revised manuscript, I have introduced a dedicated *Discussion* section that explicitly addresses the main sources of epistemic uncertainty associated with the proposed method. In particular, I clarify that the choice of the taper exponent p represents an epistemic uncertainty, as different values of p can lead to different levels of rate reduction near faults and therefore influence the resulting seismicity model. Similarly, the selection of the empirical Mw–area regression (e.g., faulting-style specific versus all-kinematics relations) affects the predicted deformation footprint and buffer width and is treated as an explicit modeling assumption. These aspects are now discussed in detail, together with their implications for seismic hazard modeling and sensitivity analyses within a PSHA framework.