Response to Referee 1

1. Summary of the manuscript

Title: Basin-Scale Geometric Focusing: A Probabilistic-Geometric Framework for Global Tsunami Hazard Assessment and the 2025 Kamchatka Peninsula Tsunami

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The manuscript proposes a hybrid probabilistic–geometric framework to identify coastal areas exposed to transoceanic tsunamis at global scale, combining Gutenberg–Richter-type seismic weighting with ray-tracing simulations. The central idea is to use ray density and coastal ray-termination points as a rapid proxy for bathymetry-induced focusing or shadowing. The approach is validated using the 2025 Kamchatka event through phase-corrected FUNWAVE-TVD simulations and comparison with DART and tide-gauge records. The work shows a reasonable spatial correlation between the geometric proxy and the maximum amplitudes obtained from the hydrodynamic model. The article is interesting and potentially useful as a rapid screening tool, although it still requires a more rigorous delimitation of its scope, of the probabilistic component, and of what can properly be referred to as "hazard assessment".

Response: We are very grateful to the reviewer for his/her constructive critiques and comments. In the following, we state the referee's comments (in black) followed by the response and actions taken (in blue).

2. Structure of the manuscript by sections

2.1: The introduction presents the problem of transoceanic tsunami propagation and the role of bathymetry in energy focusing and defocusing. It also justifies the use of ray tracing as an alternative to full numerical models and positions the contribution of the study.

2.2: Section 2: Probabilistic Tsunami Hazard Assessment. This section builds the probabilistic component from a historical catalogue, a global Gutenberg–Richter law, and a spatial KDE over a curve representing fault zones. The resulting output is an annual rate, λ, used as a weight for the rays.

2.3: Section 3: Ray Tracing Methodology. This section formulates the ray equations, describes the mesh and numerical implementation, and presents several global experiments designed to separate the effects of fault geometry, bathymetry, and probabilistic weighting.

2.4: Section 4: M8.8 Kamchatka Peninsula event. This section introduces two source scenarios (NOAA and USGS), describes the FUNWAVE simulation, applies a phase correction, and compares results with observations to evaluate the predictive capability of the geometric approach.

2.5: Section 5: Conclusions. The conclusions summarize the findings and present the method as an efficient tool for prioritizing exposed areas. The closing section is generally clear, although in some places it overextends the actual scope of the method.

Response: We thank the reviewer for the accurate summary and the section-by-section breakdown.

3. General comments

3.1: The article addresses a relevant problem with an attractive methodological combination: using ray tracing to capture the geometric organization of tsunami energy and adding seismic weighting to prioritize more likely sources. The validation with a real event and with a higher-fidelity hydrodynamic model adds credibility, and the comparison with real observations clearly improves the manuscript.

Response: We thank the reviewer for recognizing the value of the methodology and the observational validation. In addition to the Kamchatka event, based on reviewer feedback, we have now added the 2011 Tohoku event as a secondary validation case to demonstrate the model's robustness.

3.2: As currently presented, the method appears closer to an "exposure screening" tool, or a preliminary prioritization tool for transoceanic corridors, than to a complete Probabilistic Tsunami Hazard Assessment framework. Uncertainties are not propagated, aleatory and epistemic variability are not explicitly treated, and the final coastal indicator is a normalized geometric proxy rather than a directly interpretable physical intensity measure. I recommend either lowering the expectations in the title and abstract and moderating the conclusions (what would be enough), or substantially strengthening the probabilistic component.

Response: We completely agree with this fundamental distinction. We have removed claims of a full "Probabilistic Tsunami Hazard Assessment" (which requires physical intensity exceedance probabilities) and reframed the manuscript around "Exposure Assessment" and "Focusing Potential." We updated the title, abstract, and manuscript body to ensure the geometric proxy is accurately described as a measure of relative coastal exposure and transoceanic focusing corridors.

3.3: The probabilistic formulation requires stronger justification. The use of a single global b-value, a completeness threshold selected by visual inspection, and a KDE over a curve constructed from epicentres substantially simplifies the problem. This simplification may be acceptable for an exploratory study, but it should be better justified, preferably through sensitivity analyses that help clarify the limitations of the approach.

Response: We completely agree with the referee that relying on visual inspection to determine the catalog completeness is insufficient. In the revised manuscript, we have updated our methodology to formally assess the Magnitude of Completeness (M_c).
Completeness Threshold & b-value: We utilized the Maximum Curvature (MAXC) method (Woessner and Wiemer, 2005), which identified the peak of the non-cumulative frequency–magnitude distribution near $M_w = 7.05$. However, because raw MAXC estimates often fall within the catalog's detection 'roll-over' region and can underestimate the true magnitude of complete reporting, we adopted a conservative operational threshold of $M_c = 7.4$. We now state explicitly in the text that this threshold represents a modeling choice rather than an exact, immutable catalog property. To directly address the reviewer's request for sensitivity analysis, we evaluated the stability of the estimated b-value over nearby completeness thresholds (e.g., $M_c = 7.3$ to 7.5), confirming that the value remains stable and the principal basin-scale exposure patterns are not materially altered. Using the $M_c = 7.4$ dataset ($N = 212$ events), the b-value was calculated via the Aki-Utsu maximum likelihood estimator along with its formal standard error ($\sigma_b = b/\sqrt{N}$), yielding $b = 1.05 \pm 0.07$. This updated mathematical formulation and its associated uncertainties have been fully integrated into Section 2.1 and Figure 1d.
1D KDE Formulation: Regarding the KDE, we acknowledge the concern regarding the projection of 2D epicentral scatter onto a 1D curve. While resolving 3D geometry is critical for local nearshore modeling, this dimensional reduction is physically and computationally justified for basin-scale, transoceanic exposure screening. Far-field hazard is governed primarily by macro-scale, along-trench seismic productivity, making a 1D longitudinal projection an established, robust methodology for large-scale analysis (Scourse et al. 2018). Furthermore, the 500 km smoothing bandwidth (σ) is physically grounded in subduction zone scaling laws. Because great megathrust earthquakes ($M_w \geq 8.5$) are restricted by down-dip width saturation ($\sim$200 km), rupture propagation is forced longitudinally along the trench (Allen and Hayes, 2017). Empirical scaling formulas confirm characteristic rupture lengths of 440–588 km for M_w 8.8–9.0 events. Our 500 km bandwidth physically scales the probability density to match these realistic rupture lengths, explicitly avoiding the severe epicentral over-fitting that occurs with narrower bandwidths (which artificially treat massive ruptures as point-sources). This is quantitatively validated by our parametric sensitivity sweep: spatial variance stabilizes at the 500 km threshold, while correlation analysis ($r > 0.95$) demonstrates that macro-scale hazard clustering is statistically stable and independent of the chosen bandwidth. These physical justifications and sensitivity analyses have been added to the revised manuscript.

3.4: The validation is promising but still limited for supporting global claims. The use of a single case study (the 2025 Kamchatka event) and one main spatial metric (Spearman's $\rho = 0.66$) is a useful first step, but it is not sufficient to demonstrate general robustness. The manuscript would benefit from a more quantitative discussion of sensitivity to the selected wave period, the 500 m termination criterion, the effective source length and width, and the stability of the results with respect to ray-tracing parameters.

Response: To support global claims, we have added the 2011 Tohoku tsunami as a secondary validation case, comparing ray densities to FUNWAVE maximum wave envelopes and evaluating them statistically.

Wave-period sensitivity and ray-tracing stability: We agree that the selected period requires explicit sensitivity assessment. Long-period seismic tsunamis are generally weakly dispersive during deep-ocean propagation, so bathymetric refraction is expected to dominate the principal basin-scale pathways. Nevertheless, trajectories may retain some period dependence through the finite-depth dispersion relation. We therefore evaluated representative periods within the adopted range and found that the principal focusing corridors remain stable, although local ray densities and coastal termination patterns vary. For the Kamchatka and Tohoku validation cases, the characteristic periods were estimated independently from the source dimensions and observational spectra.

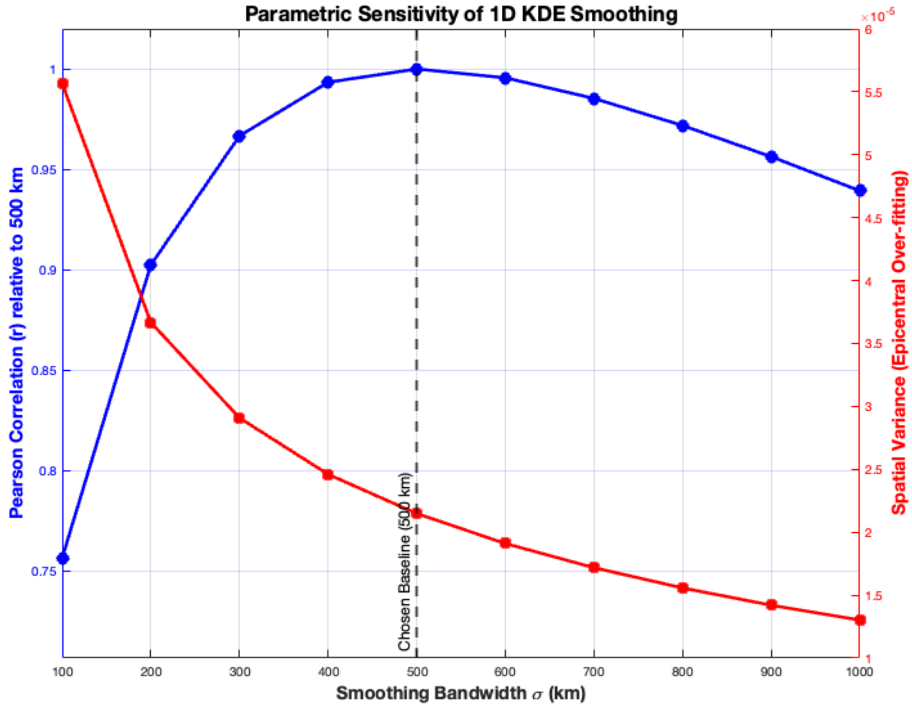

Figure 1: Parametric sensitivity of the 1D Kernel Density Estimation (KDE) smoothing bandwidth (σ). The red curve (right axis) shows spatial variance; values stabilize at the 500 km baseline, consistent with characteristic rupture lengths of great (M_w 8.8–9.0) megathrust events. The blue curve (left axis) shows the Pearson correlation coefficient ($r > 0.95$) relative to the 500 km baseline, demonstrating geographic stability across the bandwidth spectrum.

500m cutoff: We expanded the text to clarify that the scope of this framework is strictly basin-scale transoceanic focusing. As tsunami waves enter water shallower than 500 m, they require a proportionally higher resolution mesh to resolve accurately. Implementing such high resolution along global coastlines would impose heavy computational costs, directly defeating the primary purpose of this study as a rapid, efficient exposure screening tool. Furthermore, continuing to propagate rays into these shallow zones using our existing coarse deep-water mesh would yield unphysical and inaccurate results. Therefore, the 500 m cutoff acts as a necessary and strict "hand-off" boundary, safely isolating basin-scale focusing from local nearshore amplification that requires specialized high-resolution hydrodynamic modeling.

Source dimensions: We clarified in the text how the effective source dimensions (L_{eff} and W_{eff}) are inherently captured within the 1D kinematic ray release. First, the effective source length dictates the physical extent of the ray-release line; by seeding rays along this exact fault length, the spatial footprint of the rupture is naturally preserved. Second, the effective dimensions govern the initial tsunami wavelength. As detailed in our methodology, L_{eff} and W_{eff} are used to calculate the characteristic length (L_c), which then dictates the exact characteristic wave period assigned to the rays (e.g., 53 minutes for Kamchatka). Thus, releasing rays perpendicularly along the fault strike natively encodes both the 2D source dimensions and the physical directivity.

3.5: The manuscript would benefit from a more rigorous and consistent use of the terms hazard, exposure, and risk. In several places, the ray-based output is presented as "hazard", whereas the actual model output is a normalized geometric proxy of coastal ray convergence. Unless exceedance probabilities of physical intensity measures are computed at coastal locations, the terminology should be adjusted toward "relative exposure", "focusing potential", or "screening-level hazard indicator".

Response: We agree entirely. We have conducted a thorough review of the manuscript and systematically replaced "hazard" with "relative exposure," "focusing potential," and "exposure screening" when referring to the ray-tracing outputs.

3.6: The writing is generally understandable, but in some places it is repetitive. The introduction repeats the computational-efficiency argument for ray tracing several times. The paragraphs around the discussion of numerical models, Satake (1988), and Tehranirad et al. (2015) could be shortened and merged into a more concise rationale.

Response: We have consolidated the three separate paragraphs discussing the computational efficiency of ray tracing in the introduction into a single, concise rationale summarizing the transition from Satake (1988) to the Boussinesq benchmarking by Tehranirad et al. (2015).

3.7: The discussion of Figure 3 is informative but somewhat over-narrative. It could be shortened and made more quantitative, for example, by reporting changes in normalized coastal focusing for selected regions rather than relying mainly on descriptive expressions.

Response: We have revised the discussion of Figure 3, replacing qualitative adjectives with quantitative metrics. The text now explicitly reports the percentage changes in the normalized exposure index for key regions (e.g., the artificial overestimation along Northeast Australia in the uniform scenario vs. the bathymetry-corrected reduction).

4. Main comment

My main concern is that the manuscript currently makes claims that are broader than what is demonstrated by the analysis. In its present form, the work reasonably validates a geometric proxy for transoceanic coastal exposure, but not a complete global probabilistic hazard framework. This distinction is important because it affects the title, the abstract, and the interpretation of the results. The authors should either substantially reinforce the probabilistic treatment and uncertainty quantification, or reformulate the scope of the manuscript as a global screening/prioritization tool for transoceanic tsunami exposure.

Response: We have lowered the tone of the manuscript, reformulating the scope to strictly present transoceanic tsunami exposure, as recommended.

5. Specific comments

5.1: Lines 1–3 (title): There is a missing space in "AProbabilistic-Geometric". In addition, the title is somewhat too ambitious. I suggest replacing "Global Tsunami Hazard Assessment" with "Global Tsunami Exposure Screening" or an equivalent formulation, unless the probabilistic component is substantially strengthened.

Response: Thank you for catching the typo. We have fixed the spacing and changed the title to "Basin-Scale Geometric Focusing: A Probabilistic-Geometric Framework for Global Tsunami Exposure Assessment and the 2025 Kamchatka Peninsula Tsunami" to accurately reflect the scope of the work.

5.2: Lines 5–10 (abstract): The abstract works well overall, but it mixes "hazard", "exposure", and "coastal focusing patterns" as if they were equivalent. The authors should clarify from the outset what variable the ray model predicts and what it does not predict.

Response: We have revised the abstract to cleanly separate these concepts. We explicitly state from the outset that the ray model maps relative geometric convergence (focusing potential and exposure) rather than absolute wave height probabilities.

5.3: Lines 22–33: The exclusion of non-seismic tsunamis and nearshore processes is well motivated, for which decisions is the method suitable? And not?.

Response: We agree with the need to explicitly define the method's scope and utility, and we have expanded the introduction to clearly delineate these boundaries. Specifically, we clarified that non-seismic tsunamis (e.g., submarine landslides and volcanic eruptions) are highly dispersive and typically have less influence on cross-basin propagation; therefore, this framework is restricted to seismically generated tsunamis. Consequently, this method is highly suitable for rapid, basin-scale exposure analysis, probabilistically identifying the most exposed coastal areas, and prioritizing distant coastlines for high-resolution modeling using rapid geometric computations. Conversely, it is explicitly not suitable for assessing non-seismic events due to their dispersive nature, nor is it appropriate for evaluating near-source impacts, site-specific inundation mapping, or local evacuation planning, which require specialized near-field hydrodynamic tools.

5.4: Lines 35–55: This part repeats the justification for ray tracing several times. It could be shortened considerably without losing content. The central argument is already clear from the references to Satake (1988) and Tehranirad et al. (2015).

Response: We have consolidated the manuscript, to avoid repetitive arguments.

5.5: Lines 61–75: The introduction of the probabilistic component is well placed, but a sentence is needed to clarify that λ is not a direct probability of coastal amplitude exceedance.

Response: We have added a clarifying sentence to the introduction of the probabilistic component, explicitly stating that λ serves as a source weighting factor to calculate relative ray density, and does not represent a direct probability of coastal amplitude exceedance.

5.6: Lines 94–106: In Figure 1 and its caption, magnitude is described as being on the "Richter scale". For large events, this should be revised and the appropriate magnitude scale should be used, most likely Mw. In addition, "AMagnitude of Completeness" requires typographical correction.

Response: We have replaced all mentions of "Richter scale" with "Moment Magnitude (M_w)" and fixed the typo.

5.7: Lines 109–126: The construction of the curve Γ and the KDE is a delicate parts of the manuscript. how Γ is generated?,

Response: We clarified in the text that the curve Γ was constructed by tracing major tsunamigenic fault systems and subduction interfaces globally, discretizing this path into 10 km intervals. To account for the spatial scatter of historical earthquakes, epicenters falling within a 400 km search radius were orthogonally projected onto the nearest segment of this curve prior to applying the 1D Gaussian smoothing.

5.8: Lines 184–191: The normalized ray termination count is used as a proxy for amplitude. This is a crucial assumption and deserves a more nuanced discussion. Is it a proxy for maximum amplitude, incident energy, or relative probability of impact?

Response: The normalized ray-termination count is interpreted as a relative geometric focusing or exposure index. It indicates where ray trajectories preferentially converge along the coastal boundary, but it is not a probability of impact, an absolute wave-amplitude estimate, or a direct measure of incident energy flux. We have revised the manuscript to make this distinction explicit.

5.9: Lines 230–240: The summary of the Kamchatka event includes information on impacts and damage that should be supported by specific references if retained.

Response: Citations have been added for the run-up heights (e.g., Severo-Kurilsk, Hawaii) and impacts, referencing the NOAA/NCEI Global Historical Tsunami Database and USGS event summaries.

5.10: Lines 284–303: The phase correction appears useful, but the current presentation mixes several elements (compressibility, elasticity, shortest path, and DART observations) in a dense block. I suggest separating more clearly the method, the data, and the result of the correction.

Response: We have streamlined this block. We now briefly summarize that standard Boussinesq models assume an incompressible ocean over a rigid bed, causing theoretical phase speed overestimations. We reference Abdolali et al. (2019, 2026) for the formulation incorporating water compressibility, Earth elasticity, and background density, separating the pure kinematic time-shift application from the hydrodynamic amplitude outputs.

5.11: Lines 333–340: Spearman's $\rho = 0.66$. How many coastal points are used? What is the statistical significance? Does it change if selected regions are excluded or if the coastal proximity criterion is modified?

Response: To address the coastal proximity criterion and strictly isolate far-field propagation physics from localized coseismic displacement, we explicitly excluded all coastal points within a 1,000 km radius of the epicenter. Applying this region exclusion filter left 161 valid far-field coastal observation points for the analysis. For these 161 points, the spatially corrected Spearman correlation is $\rho = 0.65$. To determine the statistical significance while properly accounting for the spatial autocorrelation of coastal wave heights, we implemented a rigorous spatial block-bootstrap resampling scheme (partitioning the coastline into contiguous 500 km segments). This methodology yielded an unbiased 95% confidence interval of [0.35, 0.83]. Because the confidence interval is strictly positive and bounded well above zero, the monotonic relationship is highly statistically significant ($p < 0.01$). The details of this proximity filtering and the statistical significance are now fully documented in the revised Section 4.2. Following the reviewer suggestion, we added Tohoku 2011 event and performed similiar analysis for it in section 5.

6. Typos and local style improvements

6.1: Review spacing and typographical errors throughout the manuscript, including "large-scale ray-tracing", "farfield", "AMagnitude", and "wether".

Corrected.

6.2: Standardize the use of "far-field" and "near-field" throughout the text.

Corrected.

7. Final assessment

I recommend Major revisions. The manuscript presents an interesting and potentially useful probabilistic–geometric framework for the rapid identification of transoceanic tsunami focusing corridors. The combination of recurrence-based source weighting, ray tracing, FUNWAVE simulations, and observational comparison is valuable. However, the current framing is broader than what the analysis demonstrates. In particular, the manuscript should more clearly distinguish between probabilistic tsunami hazard assessment, exposure screening, and relative geometric focusing. The probabilistic component remains simplified, the uncertainty and sensitivity analysis is limited, and the validation relies mainly on one event and one principal spatial correlation metric. These issues affect the interpretation of the results and the claims made in the title, abstract, and conclusions. I consider major revision appropriate. The manuscript has clear potential if the scope is reframed and the methodological assumptions are better justified.

Response:

We appreciate the referee's constructive and detailed comments. We have thoroughly revised the manuscript in response to these suggestions.