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January 12th, 2026

*M*anuscript title: "**Probabilistic and *Deterministic Seismic Hazard assessments of the area comprised between west Gulf of Cadiz and east Alboran Sea"*** by Adrián José Rosario Beltré, Carlos Paredes Bartolomé and Miguel Llorente Isidro.

Dear Veronica Pazzi (NHESS Editor):

On behalf of my co-authors, I am pleased to resubmit the revised version of our manuscript entitled "Probabilistic and Deterministic Seismic Hazard Assessments of the area Comprised between west Gulf of Cádiz and east Alboran Sea". We are sincerely thankful for the comments received by all the reviewers. We found these comments highly positive and constructive, and they have been helpful to improve the final quality of our manuscript. We have carefully followed all the recommendations and suggestions provided by the reviewers. Key revisions include: (i) a clearer physical motivation for seabed hazard modelling (water column, soft sediments, and bathymetry-driven 3-D distance metrics) and a refined description of the offshore GMPE benchmarking, explicitly framing this as an acceptability check using near-coastal/onshore recordings rather than a formal seabed GMPE validation; (ii) implementation of a weighted GMPE logic-tree scheme and presentation of consolidated probabilistic and deterministic seismic hazard maps; (iii) reframing of the comparison against European models as a baseline PSHA computed by the authors using the published ESHM20 inputs under rock/onshore conditions, with explicit disclaimers that these baseline results are not official ESHM20 products and must not be interpreted as an offshore extension; and (iv) strengthened discussion of limitations, including GMPE magnitude-range extrapolation at long return periods and the need for Sa(T)/UHS for detailed structural design (identified as future work given current offshore GMPE availability).We attach a point-by-point response document where we detail the changes that we have made to fulfil with the reviewers' requirements. In addition, we include a word document where we indicate the changes introduced in the text with the Track-Changes tool of MS word.

Regarding your request to provide a track changes document between the old manuscript and the new one, we would like to clarify the following. In the earlier NHESS submission (nhess-2023-52), we addressed all reviewer comments, and this resulted in the two standard revision files: (i) a clean revised manuscript (publicly available at https://doi.org/10.5194/nhess-2023-52-AC4) and (ii) a revised manuscript with changes marked (that we are attaching), together with the Response to Reviewers document. However, following the receipt of the subsequent editor's report, the previously revised manuscript underwent major additional modifications. Specifically, a new model of seismogenic sources was adopted, updated GMPEs tailored for marine environments were implemented, and the probabilistic approach was integrated into the seismic hazard assessment. Collectively, these changes resulted in a significantly restructured and enhanced manuscript. Due to the way these changes were implemented (directly on the clean revised manuscript as part of a broader restructuring), we do not have an intermediate "changes marked" document that captures the additional edits introduced after the editor's report, prior to the current version now under peer review. Therefore, we can only provide the old revision with the changes marked, which documents all edits made by reviewers for nhess-2023-52.

We sincerely hope that the manuscript can now meet the required standards of the journal. We confirm that the work is original, and that neither the manuscript nor any parts of its content are currently under consideration or published in another journal.

All authors have approved the resubmission of this manuscript.

Yours,

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Response to Reviewers

Current version (egusphere-2025-3248)

RESPONSE TO REVIEWER

Ms. Ref. No.: Egusphere-2025-3248

Title: Probabilistic and Deterministic Seismic Hazard Assessments of the area comprised between west Gulf of Cádiz and east Alboran Sea.

Dear Alexandra Carvalho PhD

Reply (R): Thank you very much for the time and effort you have devoted to reviewing our manuscript, and for serving once again as a reviewer of our work. We sincerely appreciate your constructive and valuable suggestions, which we believe will significantly enhance the quality of the manuscript. Please find below our detailed responses to each of your comments. We have implemented all changes in a revised version of the manuscript.

I. General Comments

Beltré et al. present a seismic hazard analysis in the Ibero-Maghrebian Region (IMR), particularly focusing on the marine area between the western Gulf of Cádiz and the eastern Alboran Sea. The authors apply both probabilistic (PSHA) and deterministic (DSHA) approaches, incorporating recent developments in seismic zonation (ESHM20 model) and selecting GMPEs relevant to offshore conditions. The study also uses a medium-resolution digital terrain model.

This is a well-written paper that thoroughly addresses all the necessary steps for seismic hazard assessment in a marine domain — an area where knowledge and hazard understanding are still limited.

R: We sincerely thank the reviewer for these valuable comments.

Nevertheless, it is important to make some remarks:

While the use of Peak Ground Acceleration (PGA) is standard in seismic hazard studies, relying solely on PGA is limiting, especially considering the stated aim of informing seismic design and risk management of subsea structures. PGA does not reflect the frequency content of ground motion nor the dynamic response of structures with different natural periods. For instance, flexible structures are more affected by long period motions, which PGA alone cannot capture. This is particularly relevant given that the paper cites works (e.g., Rui et al., 2024) listing several subsea infrastructures with distinct design demands.

A more complete hazard assessment should therefore include spectral acceleration (Sa(T)) values at multiple periods. The inclusion of spectral hazard maps for representative periods (e.g., 0.3s and 1s) would provide a clearer understanding of the seismic demand on different structural types.

R: We appreciate this insightful observation and fully agree that PGA alone does not capture the frequency content of ground motion or the dynamic response of structures with varying natural periods. As the reviewer rightly points out, flexible subsea structures are more sensitive to long-period motions, and spectral acceleration (Sa(T)) at periods such as 0.3s and 1.0s provides a more comprehensive measure of seismic demand.

While our seismic hazard maps are presented for PGA due to the fact that the available offshore GMPEs are currently developed for PGA and PGV (e.g., Dhakal et al., 2024; Nakanishi and Takemura, 2024), we acknowledge that a more comprehensive hazard assessment should include spectral acceleration (Sa(T)) at multiple periods. However, because existing offshore GMPEs do not yet provide estimates for Sa(T), we are currently unable to produce spectral hazard maps.

Nonetheless, we recognize this as an important direction for future work and will consider extending our analysis to include GMPEs that allow spectral acceleration maps to be estimated as suitable data and models become available. Given the importance of this comment, we now explicitly address this in sections 5.1 (line 513) and 6 (line 664) of the revised manuscript.

Dhakal, Y. P., Kubo, H., & Kunugi, T. (2024). Prediction Equations for Peak-Ground Accelerations and Velocities in Northeast Japan Using the S-net Data. Journal of Disaster Research, 19(5), 760-771.

Nakanishi, R., & Takemura, S. (2024). Development of an offshore ground motion prediction equation for peak ground acceleration considering path effects based on S-net data. Earth, Planets and Space, 76(1), 146.

Additionally, although the comparison between GMPEs is an important step, the final output should converge into a unified hazard map. As the authors correctly mention, the selection of GMPEs carries significant uncertainty, and the use of a logic tree is a standard and effective method for incorporating epistemic uncertainties. It is therefore not entirely consistent that no epistemic uncertainty is considered in the treatment of GMPEs.

R: Thank you for this comment. A logic tree was implemented within SHA through a weighted GMPEs, based on statistical performance and regional relevance, to properly address epistemic uncertainty. Section 4.3 was adjusted accordingly.

II. Specific comments

1) Table 1 and the accompanying text refer to Mw7.7 for the 1755 Lisbon earthquake. While it is true that Fonseca (2020) provided a reassessment suggesting this magnitude, it is important to note that several other studies estimate the event's magnitude to be significantly higher, in the range of Mw8.5–8.7. These alternative estimates should also be mentioned.

R: Thank you for pointing this out. The text has been revised and now reflects a range of magnitude estimates for the 1755 Lisbon earthquake (Mw 7.5 - 8.5) in Table 1 and the text (line 117), citing both Fonseca (2020) and earlier studies such as Carvalho et al. (2009) and Mendes-Victor et al. (2009), which suggest magnitudes up to Mw 8.5.

Carvalho, A., Costa, A. C., & Oliveira, C. S. (2009). A finite–fault modeling of the 1755 Lisbon earthquake sources. In The 1755 lisbon earthquake: revisited (pp. 433-454). Dordrecht: Springer Netherlands.

Mendes-Victor, L., Oliveira, C. S., Azevedo, J., & Ribeiro, A. (Eds.). (2008). The 1755 Lisbon earthquake: revisited (Vol. 7). Springer Science & Business Media.

2) The GMPEs considered in the analysis are constrained to maximum magnitudes of 7.1 and 7.4. Given the tectonic context of the Ibero-Maghrebian Region, these values are notably low and may lead to unreliable hazard, particularly for long return periods such as 2,475 and 5,000 years. This limitation should be explicitly addressed in the manuscript.

   R: We agree with this observation. Consequently, this limitation was explicitly highlighted in the manuscript, clarifying that extrapolating GMPEs beyond their validated magnitude range introduces uncertainty, especially for return periods of 2475 and 5000 years. We also discuss the implications and justify the use of these GMPEs based on the lack of specific offshore models for higher magnitudes in the IMR. The paragraph in lines 500-505 was reworded to address the above.

3) The comparison of GMPEs with observed ground motion is certainly valuable; however, its reliability strongly depends on local site conditions. Is there any information regarding the soil conditions at the recording stations? Moreover, which site condition or soil type was assumed for the GMPEs applied in the analysis? A more detailed discussion of these aspects is necessary, and Figure 4 would benefit from further interpretation.

   R: We fully agree with the reviewer that local site conditions are critical for any comparison between predicted and observed ground motions. In our study, the observed accelerograms used for the comparison in Fig. 4 come from onshore/near-coastal IGN stations, not seabed stations. Therefore, this comparison is not intended as a validation of seabed GMPEs. Rather, it is a benchmarking / plausibility check to ensure that the selected seabed-calibrated models do not produce attenuation trends grossly inconsistent with regional observations for offshore events in overlapping Mw–R ranges, given the current lack of ocean-bottom strong-motion data in the IMR. We now explicitly address this in Section 3.3 of the revised manuscript. For the GMPEs, we assumed soft soil conditions (Vs30 = 200 m/s, Eurocode Class E) consistently across the study area, and this will be clarified in the revised text.

4) Assigning unitary weights to all GMPEs does not adequately reflect the epistemic uncertainty inherent to ground motion modelling and is inconsistent with standard PSHA methodology. It is recommended that a logic tree approach is used, with an appropriate weighting scheme. The text and Figure 7 should be revised accordingly, and a final, consolidated hazard map should be provided for each return period (e.g., in a new Figure 9).

R: We appreciate this recommendation. The logic tree structure was updated by implementing a weighted GMPE scheme based on statistical performance and regional relevance. Accordingly, the text in Section 4. was revised and Figure 7 was updated to reflect this new structure. In the section of seismic hazard assessment results and discussion, the individual hazard results for each GMPE were replaced with new consolidated PSHA hazard maps for each return period, as well as a new consolidated DSHA hazard map.

III. Technical corrections

1) Figure 2: Please confirm whether the classification in Zone A2 is correct — are PTAS390 and PTAS391 indeed characterized by strike-slip as the predominant rupture mechanism?

R: Yes, it is correct that zones PTAS390 and PTAS391 are characterized by strike-slip as the predominant rupture mechanism in the ESHM20 model (see Danciu et al., 2021c).

Danciu, L., Nandan, S., Reyes, C., Wiemer, S., & Giardini, D. (2021c). OpenQuake Input Files for the 2020 Update of the European Seismic Hazard Model (ESHM20) [Data set]. EFEHR European Facilities of Earthquake Hazard and Risk. https://doi.org/10.12686/ESHM20-OQ-INPUT

2) Figure 3. This is a very informative and visually appealing figure. However, the labels in the violet-shaded area, particularly those referring to site B, are difficult to read. Consider increasing the font size or enhancing contrast.

R: Thanks for this comment. The font size and contrast of the labels in the purple shaded area of Figure 3 were increased to improve clarity.

3) Figure 4. The scale on the x-axis is unclear — the exponent is barely visible. Improving its readability would help interpretation.

R: We agree with this observation. Consequently, the format of the text on the x-axis in Figure 4 was revised to ensure that the exponent is clearly visible.

4) Figure 5. The legend is not easily interpretable. As it applies to all eight panels, consider placing a single horizontal legend beneath the figure for clarity and consistency.

   R: We agree with this observation. The legend was consolidated into a single horizontal bar beneath all panels in Figure 5 for consistency and readability.

5) Lines 418 to 422: he text refers to "three" GMEqs and "three" seismic hazard results; however, four GMEqs and corresponding hazard outputs are presented. Please revise accordingly.

   R: The reviewer is right, and this was a mistake. This paragraph has been revised, and the new paragraph is as follows:

   "*The logic trees used in this study incorporate a combination of three seismic source models and* ***four*** *GMPEs, selected to estimate the PGA in our study area. In the logic trees used in the DSHA (Fig. 7a) and PSHA (Fig. 7b), the weights assigned to each seismic source model were those used in the ESHM20 (Danciu et al., 2024).* "

6) Figure 8. It is difficult to compare the hazard maps when each GMPE uses a different color scale. For consistency and improved interpretability, please consider applying the same scale across all GMPEs for each return period.

   R: Figure 8 was replaced by a new figure as a result of the estimation of new consolidated probabilistic seismic hazard maps. A new color scale was applied.

7) Figure 9. Please consider applying the same scale across all GMPEs.

   R: Figure 8 was replaced by a new figure as a result of the estimation of new consolidated deterministic seismic hazard map. A new color scale was applied.

RESPONSE TO REVIEWER

Ms. Ref. No.: Egusphere-2025-3248

Title: Probabilistic and Deterministic Seismic Hazard Assessments of the area comprised between west Gulf of Cádiz and east Alboran Sea.

Dear João Fonseca PhD

Reply (R): Thank you very much for your detailed and constructive comments, and for helping once again as a reviewer of our work. We appreciate the care you took in examining our manuscript and we agree with many of your observations. We have addressed all comments and revised the manuscript accordingly. Please find below our detailed responses to each of your comments. We will implement your suggested changes in an updated version of the manuscript.

Beltré et al. (in the sequence, the manuscript) set out to discuss and evaluate the applicability of seismic hazard assessment to offshore regions, focusing on the Ibero-Maghreb Region (IMR). They argue, and rightly so, that with the increased occupation of the seabed by engineering infrastructures, the related seismic risk evaluation requires robust hazard assessment at its base. The authors correctly identify the specific geological conditions of the seabed as a differentiating factor. Reference is also made to the possible effect of water-column dynamics, but in a less clear manner.

(R): Thanks for this comment. With respect to the effect of water-column dynamics, we agree that our treatment was brief. Sediment and deep-sea water characteristics significantly influence the Peak Ground Acceleration (PGA) experienced during seismic events. Sediment composition, water depth and seabed morphology play a significant role in determining how seismic waves propagate and interact with the underwater environment. We now synthesize what is known about how the water layer and soft seafloor alter impedance contrasts, incidence/refraction, and spectral content at the seabed. We make explicit how this motivates the use of seabed-adapted GMPEs and a 3-D distance metric that includes bathymetric depth.

Although we support this with references already cited in the paper (e.g., Chen et al., 2023 and 2024; Dhakal et al., 2021; Diao et al., 2014; Lan et al., 2021; Hu et al., 2020; Tan and Hu, 2023), we also include additional studies. Nakamura et al. (2015) show that thick, low-velocity sediment layers and seawater significantly amplify long-period motions. This amplification leads to higher PGA in underwater environments and contributes to prolonged seismic waves, which differ from land-based attenuation patterns during seismic events. Dhakal and Kunugi (2023) and W. Chen et al. (2024) demonstrate, through numerical experiments with varying terrain and water depth conditions, that sediment characteristics, such as the strong nonlinearity of soft sediments, and deep-sea water dynamics significantly influence PGA by altering seismic wave propagation and amplification effects. Finally, Zhang et al. (2020) highlight that the temporal and spectral characteristics of seismic ground motions differ markedly between offshore and onshore environments. Offshore ground motions tend to exhibit higher spectral accelerations and peak ground velocities, particularly in low-frequency ranges, compared to their onshore counterparts.

So, the sentence in line 46 was rephrased and now read as follows:

"*In offshore settings, the water column, seabed morphology and compliant soft sediments rheology modify source-to-site path and site terms. The water layer alters impedance contrasts and incidence/refraction of P/SV waves at the seafloor, shifts spectral content, and can lengthen significant duration at the seabed relative to adjacent onshore sites (Nakamura et al., 2015; Zhang et al. 2020; Dhakal and Kunugi, 2023; W. Chen et al., 2024). These effects, together with basin geometry and bathymetric geography, motivate use of seabed-adapted GMPEs and 3-D distance metrics that include depth (e.g., R_{RUP}) when estimating seabed PGA (e.g., Chen et al., 2024; Hu et al., 2020; Dhakal et al., 2021; Tan and Hu, 2023; Diao et al., 2014; Lan et al., 2021).*"

Chen, W., Lin, J., Zheng, Y., Su, L., Chen, G., & Huang, L. (2024). Nonlinear seismic response of seabed with terrain variation and seawater-seabed coupling. Soil Dynamics and Earthquake Engineering, 180, 108579. https://doi.org/10.1016/j.soildyn.2024.108579

Nakamura, T., Takenaka, H., Okamoto, T., Ohori, M., & Tsuboi, S. (2015). Long-period ocean-bottom motions in the source areas of large subduction earthquakes. Scientific reports, 5(1), 16648. https://doi.org/10.1038/srep16648

Dhakal, Y. P., & Kunugi, T. (2023). Preliminary analysis of nonlinear site response at the S-net seafloor sites during three Mw 7 class earthquakes. Frontiers in Earth Science, 11, 1180289. https://doi.org/10.3389/feart.2023.1180289

Zhang, Q., & Zheng, X. Y. (2020). Temporal and spectral characteristics of seismic ground motions: Offshore versus onshore. Marine Structures, 74, 102812. https://doi.org/10.1016/j.marstruc.2020.102812

While addressing the specific aspects of seabed ground motions, the manuscript engages on a comparison between probabilistic (PSHA) and deterministic (DSHA) seismic hazard assessment. For the latter, the manuscript proposes a "zone-based" (line 314) implementation for which no references are given in a clear way, leading me to assume that it was developed by the authors.

(R): We agree that the manuscript did not clearly cite the methodological precedent for the zone-based DSHA implementation. To clarify: this method was not originally scripted and programmed by the authors, but rather follows the algorithmic workflow used in prior works (i.e., Loi et al., 2018; Huang and Wang, 2012; Wang et al., 2012; Vipin, 2013; Candia et al., 2019; Ramkrishnan et al., 2021), who developed a deterministic seismic hazard maps using a zone-based framework implemented via Excel-based script, FORTRAN or Matlab codes, like ours. Their work demonstrates how deterministic hazard can be computed using areal source zones when fault-specific data are sparse or diffuse, particularly in offshore or complex tectonic regions.

In our study, the zone-based DSHA approach is justified by the lack of well-constrained fault geometries in many offshore areas of the IMR, where seismicity is diffused and submarine structures are poorly resolved. In such contexts, using areal sources to represent seismogenic zones is a practical and accepted alternative, especially when the goal is to estimate maximum credible ground motions for infrastructure planning.

To further support this approach, we now cite additional relevant studies that have employed similar deterministic hazard frameworks using areal or zone-based sources:

Loi, D. W., Raghunandan, M. E., and Swamy, V.: Revisiting seismic hazard assessment for Peninsular Malaysia using deterministic and probabilistic approaches, Natural Hazards and Earth System Sciences, 18, 2387–2408, https://doi.org/10.5194/nhess-18-2387-2018 , 2018 .

Huang, D. and Wang, J.-P.: Seismic hazard map around Taiwan through a catalog-based deterministic approach, in: 15th World Conference on Earthquake Engineering, 2012.

Vipin, K. S.: Assessment Of Seismic Hazard With Local Site Effects : Deterministic And Probabilistic Approaches, Thesis, 2013.

Candia, G., Macedo, J., Jaimes, M. A., and Magna-Verdugo, C.: A New State-of-the-Art Platform for Probabilistic and Deterministic Seismic Hazard Assessment, Seismological Research Letters, 90, 2262–2275, https://doi.org/10.1785/0220190025 , 2019.

Ramkrishnan, R., Kolathayar, S., and Sitharam, T. G.: Probabilistic seismic hazard analysis of North and Central Himalayas using regional ground motion prediction equations, Bull Eng Geol Environ, 80, 8137–8157, https://doi.org/10.1007/s10064-021-02434-9 , 2021.

Wang, J. P., Huang, D., & Yang, Z. (2012). Deterministic seismic hazard map for Taiwan developed using an in-house Excel-based program. Computers & Geosciences, 48, 111–116. https://doi.org/10.1016/j.cageo.2012.05.014

Krinitzsky, E. L. (2002). How to combine probabilistic and deterministic methods for maximum credible earthquake ground motions for use in engineering design. Engineering Geology, 63(2), 149–158. https://doi.org/10.1016/S0013-7952(01)00074-5

Reiter, L. (1990). Earthquake Hazard Analysis: Issues and Insights. Columbia University Press.

Kramer, S. L. (1996). Geotechnical Earthquake Engineering. Prentice Hall.

K. Campbell, K. (2005). Overview of Seismic Hazard Approaches with Emphasis on the Management of Uncertainties. 2nd ICTP Workshop on Earthquake Engineering for Nuclear Facilities: Uncertainties in Seismic Hazard. Trieste, Italy. pp. 14-25. February 2005

These references are now explicitly included in the revised manuscript to clarify that our DSHA implementation is grounded in established practice and literature.

This was explicitly addressed in the text by adding the following paragraph to section 4.1:

"This study adopts a zone-based DSHA approach, following the standard algorithmic workflow as used in Wang et al. (2012), Vipin (2013), Candia et al. (2019) and; Ramkrishnan et al., (2021) which implement the classical deterministic hazard assessment procedure (Reiter, 1990). Deterministic ground motions are computed for areal seismic sources in regions lacking well-defined fault geometries. This method has precedent in offshore hazard studies and is particularly suitable for submarine environments where seismicity is diffuse and fault structures are unresolved. Similar approaches have been discussed (i.e., Krinitzsky, 2002, Kramer, 1996, Campbell, 2005) and are widely used in engineering applications where deterministic estimates are required for preliminarily infrastructure design."

The key result of the paper, in my view, is a comparison (the manuscript's Figure 11) between the PGA values obtained in the manuscript using GMPEs derived for seabed conditions, and values obtained with a more standard approach. However, the way the comparison was conducted raises some issues:

- The selected seabed GMPEs are "*evaluated*" (Line 244) by comparing their predictions with recorded strong ground motion for six earthquakes with offshore epicenters (Lines 244-252). The manuscript does not specify the locations of the instrumental recordings, mentioning only that the accelerations were "*sourced from IGN*". If the recording sites were not located on the seabed, these data are not suitable for validation of seabed-specific GMPEs;

  (R): We confirm that the earthquake database used in this work for lines 244-252 are form onshore/near-coastal accelerogram recording IGN stations. We are carrying out a sanity-check/benchmark using the best recent available regional strong-motion data in overlapping Mw-R ranges. We are not validating seabed GMPEs. Offshore GMPEs are emerging from dense seafloor networks (S-net, DONET, NEPTUNE) and explicitly include water depth, sediment properties, burial state and path corrections through marine layers; they show larger long-period amplitudes and different attenuation than onshore models. So, unfortunately, current models remain regionally focused (Japan, Sagami Bay) where S-net seafloor recordings or regional OBS arrays have been deployed. Current offshore GMPEs have been developed explicitly for seafloor recordings and subduction settings using this S-net and other ocean-bottom datasets (Hu et al., 2023; Li and Ji, 2023; Nakanishi and Takemura, 2024; Tan and Hu 2024; Dhakal et al., 2024). The rationale for including this benchmark is that, in the absence of offshore OBS networks in the IMR, these land-based records still offer insight into attenuation trends for nearby offshore events. This step provides context for the selected GMPEs and ensures their forecasts are not grossly inconsistent with observed trends for offshore events in the IMR. In our manuscript, Figure 4 compares observed onshore PGAs with predictions from selected seabed-specific GMPEs, showing that, for several offshore events, horizontal PGAs tend to be below the average predictions of seabed-calibrated models at short-to-moderate distances. This trend is consistent with the findings of Zhang and Zheng (2019), which show that offshore horizontal PGAs are systematically larger than onshore PGAs for earthquakes at similar epicentral distances (average amplification factor ≈1.8). They attribute this to the combined effects of soft seafloor sediments and high saturation, which enhance horizontal motion while vertical components are suppressed by the water column. Our results are consistent with this mechanism: the amplification observed in the predicted PGAs from the GMPEs relative to the observed onshore PGAs probably reflects this absence of local site effects.

This benchmark serves as a plausibility check to verify that the selected seabed GMPEs produce reasonable attenuation trends when applied to regional offshore events, even though the accelerograph recordings are from closest coastal stations, although we clearly recognize the limitations that must be reflected in the manuscript. This limitation reflects the current absence of offshore strong motion networks in the IMR, a gap repeatedly noted in previous studies (e.g., Molina Palacios, 1998; Vilanova and Fonseca, 2004; Terrinha et al., 2009; Martínez Loriente et al., 2013; Somoza et al., 2021; Perea et al., 2023; Olaiz et al., 2025). Our approach follows the precedent set by these works, which relied on inland proxies to characterize offshore hazard. We now explicitly state this limitation in our manuscript and frame the comparison as a diagnostic exercise, while emphasizing the urgent need for OBS networks to enable true seabed GMPE validation.

We will replace the term *"validated/evaluated"* with *"benchmarked against nearby observed ground motions."* The diagnostic residual analysis will be qualified as illustrative, with methodological moved to the Supplementary Material. Finally, we will emphasize that the primary justification for using HZ23, NT24, and DKK24 GMPEs is their seabed calibration (S-net/KiK-net), complemented by literature evidence of systematic land–seafloor differences, rather than by our own validation in the IMR.

This was explicitly addressed in the text by adding the following paragraph in line 244:

"We benchmarked the selected GMPEs against observed horizontal PGAs for six regional offshore earthquakes using onshore/near-coastal accelerograms from IGN (https://www.ign.es/web/siscatalogo-acelerogramas). This benchmark is a statistical plausibility check in overlapping Mw-R ranges. But it is not a validation nor recalibration of seabed GMPEs in the IMR. The primary evidence supporting seabed GMPEs (HZ23, NT24, DKK24) remains their calibration on ocean-bottom data (S-net/KiK-net) and the documented land-seafloor differences reported in the literature (e.g., Dhakal et al., 2021; Hu et al., 2023; Chen et al., 2015, 2024). This approach follows previous IMR hazard studies (e.g., Molina Palacios, 1998;

Vilanova & Fonseca, 2004; Terrinha et al., 2009; Martínez-Loriente et al., 2013; Somoza et al., 2021; Perea et al., 2023; Olaiz et al., 2025), which also relied on inland proxies due to the absence of seabed recordings. Therefore, this comparison should be interpreted as an acceptability check within the GMPEs magnitude-distance validity ranges, rather than a direct GMPE re-calibration. The scarcity of offshore strong-motion data underscores the need for OBS deployments to enable future seabed-specific GMPE validation in the IMR."

Martínez-Loriente, S., Gracia, E., Bartolome, R., Sallarès, V., Connors, C., Perea, H., ... & Zitellini, N. (2013). Active deformation in old oceanic lithosphere and significance for earthquake hazard: Seismic imaging of the Coral Patch Ridge area and neighboring abyssal plains (SW Iberian Margin). *Geochemistry, Geophysics, Geosystems*, *14*(7), 2206-2231. https://doi.org/10.1002/ggge.20173

Molina Palacios, S. (1998). Sismotectónica y peligrosidad sísmica del área de contacto entre Iberia y África (Doctoral dissertation, Universidad de Granada). http://hdl.handle.net/10481/53680

Olaiz, A., Álvarez Gómez, J. A., De Vicente, G., Muñoz-Martín, A., Cantavella, J. V., Custódio, S., ... & Heidbach, O. (2025). Onshore and offshore seismotectonics of Iberia: an updated review. *Solid Earth*, *16*(10), 947-1024. https://doi.org/10.5194/se-16-947-2025

Perea, H., Jiménez, M. J., Lozano, L., Gómez Novell, O., Canari Bordoy, A., García-Fernández, M., & García Mayordomo, J. (2023). Reassessing seismicity and seismic hazard in offshore areas: The case of the Alboran Sea (western Mediterranean). http://hdl.handle.net/10261/338277

Somoza, L., Medialdea, T., Terrinha, P., Ramos, A., & Vázquez, J. T. (2021). Submarine active faults and Morpho-Tectonics around the Iberian margins: seismic and tsunamis hazards. *Frontiers in Earth Science*, *9*, 653639. https://doi.org/10.3389/feart.2021.653639

Terrinha, P., Matias, L., Vicente, J., Duarte, J., Luis, J., Pinheiro, L., ... & Matespro Team. (2009). Morphotectonics and strain partitioning at the Iberia–Africa plate boundary from multibeam and seismic reflection data. *Marine Geology*, *267*(3-4), 156-174. https://doi.org/10.1016/j.margeo.2009.09.012

Vilanova, S. P., & Fonseca, J. F. (2004). Seismic hazard impact of the Lower Tagus Valley fault zone (SW Iberia). *Journal of Seismology*, *8*(3), 331-345. https://doi.org/10.1023/B:JOSE.0000038457.01879.b0

Zhang, Q., & Zheng, X. Y. (2019). Offshore earthquake ground motions: Distinct features and influence on the seismic design of marine structures. Marine Structures, 65, 291-307. https://doi.org/10.1016/j.marstruc.2019.02.003

- The PGA values proposed in the manuscript are compared to "*those obtained using input data files containing the information of the seismogenic sources (...) and the OQ Engine configuration and input files. These were used to calculate the*

ESHM20 hazard mapping scheme". Despite the obscure formulation, it seems that the comparison is between the new values proposed in the manuscript and the values that the ESHM20 model would have shown if it extended to the IMR seabed. If this is the case, this seems an unwarranted application of the ESHM20 input data to obtain results for an area that its (ESHM20's) authors did not include in their model.

(R): We have reframed this comparison. We now present a "baseline PSHA" computed with the published ESHM20 source model and GMPE logic tree as-is (i.e., rock/onshore conditions), restricted to our study window, solely as a reference baseline to illustrate how seabed-adapted GMPEs and bathymetry change PGA relative to a standard continental model. We explicitly state that these baseline maps are not ESHM20 products and should not be interpreted as an official extension of ESHM20 offshore. The text has been reworded to avoid any implication or misunderstanding of "extending" ESHM20 offshore or attributing the results to the ESHM20 consortium.

The title of section 5.4 has been changed to: "*Distribution of PGA amplification relative to a baseline PSHA using the ESHM20 model (rock, onshore).*"

The sentences on pages 583-587 were replaced by:

"*We computed a baseline PSHA using the published ESHM20 source model and GMPE logic tree exactly as released (rock/onshore site conditions), clipped to our study zone in the IMR, to provide a reference for how seabed-specific GMPEs and bathymetry alter PGA. These baseline results are not part of the official ESHM20 products and are used here strictly as a methodological contrast.*"

In the legend for Figure 11, we changed *"ESHM20"* to *"Baseline PSHA (ESHM20 model; rock)"* and add the disclaimer sentence above in the caption.

Regarding the reviewer's opinion on the key findings of the paper, we would like to clarify that the primary contribution of the manuscript is the set of new seabed-focused PSHA and DSHA hazard maps, which provide the first consolidated regional-scale characterization of seabed PGA in the IMR using updated seismogenic sources and offshore/soft-soil ground-motion modelling. We recognize that, in the previous structure, the baseline-vs-seabed comparison figure could be perceived as the central result, which was not our intention.

To address this, we have revised the manuscript structure and emphasis so that the PSHA and DSHA maps are clearly presented and discussed as the key results, while the baseline comparison is treated explicitly as a secondary diagnostic tool used only to contextualize the effect of seabed-specific modelling choices. Concretely:

1) We have strengthened the Results narrative so that the consolidated PSHA hazard maps (100, 475, 2475, 5000 years) and the consolidated DSHA map are presented first, with

expanded interpretation, including drivers of spatial patterns (source model controls) and the role of bathymetry and GMPEs (attenuation controls).

2) We have reframed and demoted the baseline comparison to a methodological contrast, avoiding any implication that it is the paper's headline result. The text now explicitly states that the baseline PSHA is computed by the authors using published ESHM20 inputs under rock/onshore conditions and is not an official ESHM20 product nor an offshore extension. It is included only to quantify relative change (ΔPGA) and help readers interpret differences between standard continental assumptions and seabed-specific modelling.

Statements such as "*the main input parameters in the classic DSHA (Reiter, 1990) are the maximum magnitude associated with the characteristic earthquake as the MCE for each seismic source area and a set of attenuation relationship or GMPEs*" (lines 306-307) fail to capture, in my view, the essence of DSHA (standard DSHA doesn't use source areas, identifying directly the capable geologic structure near the site whose rupture corresponds to the worst-case scenario).

(R): We agree our phrasing could misinform. Our deterministic implementation identifies the capable controlling source for each site (fault or areal zone) and computes the percentile of strong ground motion for the MCE of that source at the shortest distance under the selected distance metric (e.g., Rrup, Rjb). We revised Section 4.1 to remove the ambiguous "zone-based" rephrasing and to state that DSHA can be performed with fault sources and, where appropriate, areal/background sources when faults are diffuse or unresolved, following Reiter (1990), Kramer (1996), and Krinitzsky (1995, 2002), already cited.

The sentence in lines 306-307 were replaced with:

"In DSHA, the capable controlling source at a site (typically a mapped fault segment and, where faults are diffuse, an areal/background sources) identified. The maximum credible earthquake (MCE) for that source and an appropriate GMPE are then used to compute a chosen percentile (e.g., 50th or 84th) of ground motion at the shortest source-to-site distance under the adopted distance metric (Reiter, 1990; Kramer, 1996; Krinitzsky, 1995, 2002)."

Additionally, we removed will remove the term "*zone-based*" in line 314 and rename Step (i) to "*Build a catalogue of seismic sources (faults and, where needed, areal/background zones).*"

In terms of implementation, the manuscript refers that "For DSHA calculations, a MATLAB© code has been developed to calculate the seismic intensity with a selected GMPE, from different seismic source types" (lines 326-438), and later add "*MATLAB code [available] from the corresponding author upon reasonable request*" (line 680). In my view this is not acceptable: if the code is not public, the results cannot be reproduced

independently and therefore cannot be regarded as scientific. Preferably, the code developed in-house should be validated through separate publication before being used in an applied study.
(R): We agree that reproducibility is essential. We (a) publish all DSHA MATLAB® scripts and configuration files used in this study as open source, and (b) include a minimal verification notebook that reproduces the example maps using a reduced grid. A permanent open DOI (via GitHub) is now provided, and the link is shared in the code availability section.

I found the extensive emphasis on PSHA/DSHA comparison a distracting tangent. As an example, statements to the effect that "the hazard maps obtained from deterministic and probabilistic estimations demonstrate a clear correlation: as the return period increases, PGA values calculated by PSHA increase and approach those obtained via DSHA" (line 548-550) are presented as a relevant conclusion of the study, whereas in my view they are a statement of what should be expected.
(R): Thanks for this comment. The comparison between DSHA and PSHA has been reduced largely, retaining only what is necessary to motivate the specific seabed modeling choices. Section 5.3 has been removed entirely from the main text.

Occasionally, the manuscript suggests a lack of command of basic aspects of seismic hazard assessment that is concerning given the focus of the study, and the apparent objective of being didactic, as suggested by the extensive theoretical considerations (as opposed to simply referencing standard texts). As examples:

- in line 386 the manuscript states that the GR recurrence parameter I can be "*adjusted from the seismic catalog of the project using statistical techniques of least squares or maximum likelihood*". It was well established 45 years ago that least-squares should not be used for this purpose;
  (R): We agree with the reviewer. We have removed the term "least squares" from the sentence and now indicate that we use maximum likelihood (with completeness assessment) for the GR parameters, in line with standard practice (e.g., McGuire, 2004; Kijko, 2019).

- Figure 7 displays logic trees with nodes where the sum of the weights largely exceeds unity;
  (R): Thank you for this comment. A logic tree was implemented within the SHA using weighted GMPEs, based on statistical performance and regional relevance, in order to properly address epistemic uncertainty. Figure 7 was reconstructed with weights totaling 1.0 at each node. The text was updated accordingly.

- While describing Cornell's method line 363-364), the manuscript states "having established the probability relationships that govern seismic events in a seismogenic area (point, fault plane, or spatial region) regarding the distribution of locations, sizes, and recurrence times, and knowing the past seismicity of the area". The "probability relationships that govern seismic events" must be derived

from the seismicity of the area, and therefore are not independent from it as the wording may suggest;
(R): Thanks for this comment. We rephrased the paragraph to reflect that those relationships are derived from regional seismicity and source characterization. Now, the paragraph reads as follows:

"*The principles of PSHA find their roots in the seminal works of Cornell (1968), Cornell et al. (1971), and McGuire (1976). Cornell uses the total probability theorem (Kramer, 1996) to consolidate information on various variables (magnitude, distance, source location) into a single measure of seismic hazard for a specific site. Once established the probability relationships that govern independent seismic events in a seismogenic area it is possible to quantify the probability of exceeding a specific level of ground motion (Kijko, 2019). It takes into consideration the seismicity of the area from the distribution of locations, sizes, recurrence times and energy attenuation (Benito and Jiménez, 1999), to integrate the probabilities of exceeding a certain level of ground motion from various seismic sources, at different distances and with different magnitudes, that may occur at a site.*"

- The manuscript states (lines 455-459) that "*the findings of this study demonstrate the effectiveness of using both DSHA and PSHA methodologies to generate seismic hazard maps within the IMR. These maps are based on the most recent GMPEs specifically tailored for offshore environments and show statistically robust compatibility with the available seismic data*". This passage seems to suggest that the compatibility of the maps with the seismicity comes from the selection of GMPEs, apparently ignoring the fact that the seismic data determines the choice of area-source geometry, which in turn is reflected on the hazard maps (in this particular case both PSHA and DSHA use area sources).
  (R): We agree with the reviewer. Therefore, we will revise that paragraph to make clear that compatibility with observed seismicity primarily reflects the source spatial model and its seismic parameters, while GMPEs govern the source-to-site attenuation term. Therefore, both are necessary.

To conclude, reading the manuscript I got the impression that the research set out to address the very relevant issue of seismic hazard assessment in the seabed of the IMR region, but lost its focus by venturing excessively into a discussion of the relative merits and demerits of PSHA versus DSHA. The DSHA code, which is the basis of a substantial part of the manuscript, is not shared, defeating the possibility of scrutiny. I am forced to conclude that in its current form the manuscript is not fit for publication. Should the authors consider rewriting it to try a subsequent submission, the attached pdf includes extensive comments on the passages that need improvement.
(R): Thank you for your comprehensive feedback. We acknowledge your concerns regarding focus and reproducibility, and we have taken steps to address them in the revised manuscript:

We substantially reduced discussion on the relative merits of PSHA versus DSHA. The main text is now focused on the core objective: seismic hazard assessment for seabed conditions in the IMR, emphasizing bathymetry, seabed-specific GMPEs, and implications for subsea infrastructure. The DSHA MATLAB© scripts and all configuration files are open source in a repository.

Regarding the attached PDF, we reviewed the file and found that it pertains to a different manuscript. It appears this was included inadvertently. Therefore, we have relied exclusively on your comments provided in the current review and have addressed each point in our detail response. If there are specific passages in our paper that you intended to highlight for improvement, we would be grateful if you could confirm or resend the correct annotated document.

Response to Reviewers

Old version (nhess-2023-52)

RESPONSE TO REVIEWERS

Ms. Ref. No.: nhess-2023-52

Title: "Review article: Deterministic seismic hazard assessment of the area comprised between west Gulf of Cádiz and east Alboran Sea."

Dear João Fonseca and Alexandra Carvalho,

Reply (R): We thank you very much for your constructive and valuable suggestions. We believe that the quality of our manuscript has improved considerably after your review. We have carried out an exhaustive revision of our manuscript following your suggestions. We have succeeded in synthesizing and improving the content of the manuscript. All changes made are shown in the Revised manuscript (with changes marked) submitted.

Regarding the most relevant changes to the manuscript, we have greatly reduced the extension of the manuscript, with less text and fewer images and references. Sentences have been simplified and reworded for better reading. Some details have been moved from text to tables. It is basically a new manuscript keeping the findings and the general ideas.

1) Comments of Reviewer João Fonseca:

Beltré et al. submitted a manuscript entitled "Review article: Deterministic seismic hazard assessment of the area comprised between west Gulf of Cádiz and east Alboran Sea" presenting the results of a deterministic seismic hazard assessment (DSHA) for the Cadiz Gulf / Alboran Sea region and benchmarking their results against previous studies for the emerged parts of their study area given the lack of previous studies for the submerged regions. The manuscript discusses at length the advantages and limitations of probabilistic (PSHA) versus deterministic assessments, and similarly for zoned versus non-zoned deterministic studies. Another aspect that deserves particular attention is the use of a Digital Terrain Model (DTM) to allow the correct estimate of the distance from source to site.
Reply (R): We sincerely thank the reviewer for these valuable comments.

Although the manuscript presents itself as a review paper, it does not correspond in the opinion of this reviewer to the requirements of such qualification, namely "[to] summarize the status of knowledge and [to] outline future directions of research". The remainder of this review will address the relevance of the manuscript as a potential research paper.
R: We agree with this comment. Our work was conceived and carried out as a research paper. It was the editor's recommendation to change our manuscript type to review.

The manuscript adopts an in-depth description of the details of the work done that makes it read more like a technical report than a scientific paper, where the relevant aspects should be highlighted and additional details synthetized in tables or supplementary material. With the profusion of details, the reader loses the purpose of the manuscript: To

present a new methodological approach? To advocate the need for hazard assessment of submerged regions? To put forward a product ready to be used for the design of infrastructures? All of the above? Perhaps because of the profusion of apparent goals, the manuscript fails, in the opinion of this reviewer, to be convincing for any of them.
R: We agree with this observation. Consequently, we will work on reducing and synthesizing the additional details. Thanks to the selection of elements suggested in the written text on the website as well as in the interesting and detailed comments included in the attached pdf, our effort to reduce it will benefit the paper with a more concrete content.

When advocating for DSHA instead of PSHA, the authors seem to reinstate a dated controversy of the 1990s in the United States, after which vast clarifications of concepts and domains of applicability were obtained that were not discussed. While addressing a relatively untouched topic which is seabed hazard assessment, the authors fail to highlight the growing exposure of submarine structures and associated risk, which would be an interesting point in favor of the study. In what concerns the applicability of the results for practical purposes, the non-standard aspects of the approach would require much better justification for it to be considered fit for use.
R: We will clarify that our paper is one step in a path towards SHA by probabilistic methods, splitting the work into several articles (tasks or phase steps) to provide as much info on our work as possible. As you have seen, this first work is approached from a deterministic perspective, providing preliminary results, which will be refined in successive articles using probabilistic methods. Without doubt, one of the priority uses of these more precise results, before the determination of the effects on the instability of submarine slopes and the possible origin of tsunamis, could be for the anti-seismic dimensioning of infrastructures (e.g., oil and gas pipelines, communications cables, exploration structures, wind generators, etc.), including the design of tunnels or bridges located on the seabed.

To illustrate the latter aspect: the authors advocate the adoption of a zoned DSHA method according to Wang and Huang (2014), relying heavily on that single reference – in fact, from which the EXCEL code to perform the computations was obtained - to support that not-so-standard choice. To differentiate zoned from non-zoned DSHA, the authors state that “the application of the DSHA is not zoned, if it is done on a homogeneous seismicity region, with a single seismic source of global influence” (line 394). However, it is well known that one of the strongest arguments in favor of DSHA, and one that makes it adopted for the design of critical structures, is that it is reality-based, taking into account the geological structures surrounding the site and their seismogenic characteristics. DSHA has at its core the concept of maximum credible earthquake (MCE) at a particular geological structure or set of well identified geological structures, and usually avoids the use of area sources altogether. In the manuscript, the authors describe in details the relevant geologic structures of the region of interest, but at the time of computing the hazard these structures do not seem to be taken in consideration, the MCE being considered equal to the MMax of each area source in previous (probabilistic!) studies.
R: We agree with this observation. Accordingly, we will review that line. It could be said that we actually use the DSHA using a zoned approach. It is true that the aforementioned paragraph (line 394) is incorrect and misleading. It even indicates that a zoned DSHA

method is adopted, justifying it by the work of Wang and Huang (2014), which is neither exclusive nor correct. Likewise, for the reviewer's clarification, we will detail how some of the seismically relevant geological structures that are taken into account in the seismic hazard calculation process have been described.

Even more concerning, it was not possible for this reviewer to understand how the distance from source to site was computed prior to its insertion in the GMPEs: after describing several second-order corrections to the distance, the authors mention that those corrections were applied to “the minimum distance calculated with the original script” (line 618, referring to a pre-existing EXCEL script that was adapted for the calculations). Were the authors assuming that a fault capable of producing the MCE for each area zone existed at the minimum distance from the site to each area source? This would certainly increase the hazard unwarrantedly.
R: Thanks for this comment. We will recheck to further clarify how the distance is being considered.

Another non-standard approach that the authors take, still following Wang and Huang (2014), is the consideration of “non-control area sources” in addition to the “control area source” (i.e., the source producing the maximum maximum credible earthquake). In this case, the authors remark that the hazard is higher at sites near the “confluence” of source zones, an observation that in and by itself should raise concerns: if the pattern of the hazard is driven by the source zone geometry, something must be wrong with the methodology (actually, this limitation is even clearer in Wang and Huang (2014)’s hazard assessment for Taiwan, which shows a “checkered” pattern when “non-control sources” are considered, with no similarity whatever to the results obtained when non-control sources are not used (their figures 5 and 6); it should be pointed out that Wang and Huang (2014) was published in a journal which is very remote from mainstream seismological research, which may account for a less rigorous review process for that paper; it is a tell-tale detail that no mainstream hazard publication has refered Wang and Huang (2014)). The authors report a 445% (!) increase of the hazard at Cadiz (84% percentile) when non-control area sources are added to the control area source, and fail to identify that discrepancy as anomalous.
R: Thank you for this comment. We will review and try to clarify the statistical justification for adding uncontrollable seismic sources to the hazard assessment, beyond the fact that the cited authors use them in a publication of little impact, and improve the explanation of the limited real effect they have. We will adjust the definition of anomalous cases.

In addition to the serious reservations listed about, it must be mentioned that the manuscript contains inaccuracies easily corrected had the manuscript been previously subjected to any level of review prior to submission. Just to mention a few:

Line 93: “As shown by geodetic data and geodynamic studies that have been carried out for decades (e.g., Montessus de Ballore, 1894; Pastor, 1927; Munuera, 1963) tectonic models of NW-SE to WNW-ESE oblique convergence between the plates (Reilly et al., 1992; Herraiz et al., 2000) present displacements of 2 to 5 mm/yr” Pre-plate tectonics

studies can hardly have any relevance to the discussion rates of convergence between plates.
R: The reviewer is right. We recheck and consider updated references.

Line 212: “Seismic hazard is the probability that an earthquake occurs in a given geographic area, within a given time period, and with a ground motion intensity exceeding a given threshold (McGuire, 2004)”. This passage indicates a lack of familiarity with the concept of seismic hazard that is not adequate in a manuscript on seismic hazard assessment. Seismic hazard concerns probability of exceedance of a level of ground motion at a site over a period of time, not probability of occurrence of an earthquake in a geographic area.
R: The reviewer is right. We will review and clarify this passage.

line 387: “… the probability density of the 50th percentile, or average value, …”. The 50% percentile of a pdf corresponds to the median, not the “average” (in other passages. e.g., line 423, it is called “mean”, also wrongly), and even without that imprecision the sentence is devoid of meaning (the median is a value of the random variable, not of the probability density).
R: The reviewer is right. We will revise the passages to improve the explanation and clarify why the coincidence of the 50th percentile, mean and median that is being used interchangeably and is causing confusion to the reviewer and future readers.

In view of the above remarks, I regret that I have to recommend that the manuscript should not be accepted for publication in NHESS. Should the authors consider rewriting it to try a subsequent submission, the attached pdf includes extensive comments on the passages that need improvement.
R: Dear reviewer, we greatly appreciate the work you have done and your dedication to reviewing this manuscript. We are reviewing the text considering your interesting and appropriate appreciations, comments, recommendations and corrections. Likewise, we will integrate your comments with those we may obtain from other reviewers to prepare a revised version of our manuscript for submission in the near future.

Line 1: The title of the manuscript
Reply (R): We have removed the phrase ¨Review article¨ from the title.

Line 15: Evidence of submarine long-strike strike-slip fault systems
R: The sentence was removed.

Line 16: Redundant , evidence?
R: The phrase has been reduced to “This activity has caused moderate to high magnitude earthquakes and that may have triggered tsunamis either directly due to seabed elastic deformation or indirectly by triggering submarine landslides.”

Line 18: explain why this is needed
R: The following sentence has been included to explain why this is needed “Tsunami risk in the area is significant and increasing mainly because of the growing economic and social pressure on the coasts. Although seabed deformation after earthquakes in tsunami

hazard has a de facto standard study approach, seismicity and submarine landslides are not well considered amongst different initiatives. To understand tsunamis caused by landslides, we need to consider both geomechanical properties and the spatial distribution of Peak Ground Acceleration (PGA) as a triggering mechanism."

Line 20 to 26: several comments
R: This paragraph was completely rewritten.

Line 29 to 39: several comments
R: This paragraph was synthesized and rewritten.

Line 40 to 57: several comments
R: This paragraph was synthesized and rewritten.

Line 59: unclear
R: The sentence was rewritten to read as follows "The test area in this work has not been addressed in seismic hazard studies recently despite its significant record of earthquakes, submarine landslides and tsunamis (Rodriguez et al., 2017; Vázquez et al., 2022b)."

Line 75: what is the reference for the original version?
R: The reference has been added in the text.

Line 80: identification of sources is only a part of the hazard assessment, why is the full exercise needed?
R: This sentence was removed.

Line 84: All the geographic references and fault names mentioned here must be clearly shown in Figure 1. It may need a "geology" picture without seismicity, to be readable. Maybe split into A) and B)?
R: Fig. 1 has been updated according to the toponyms indicated in the main text. Thanks.

Line 101: it is not clear what this means
R: The sentence was rewritten as follows "This area is highly active geologically, with moderate to low-moderate seismic activity. Historical cases have demonstrated the occurrence of such hazards in the region (Vázquez et al., 2022a)".

Line 105: the figure is difficult to read. Maybe split in a) geology and faults, and b) seismicity.
R: The seismicity has been removed from Fig. 1 to make it clearer to read.

Line 115: According to Custodio et al. 2016, "Some offshore earthquakes occur at moderate depths of 40–60 km but do not delineate a Benioff zone. These earthquakes have been interpreted as evidence that the old and cold oceanic lithosphere is brittle through the upper mantle (Grimison and Chen, 1986; Stich et al., 2005b)." Depths of 10km and 33km in the catalogs are usually fixed by the location program, because they cannot be determined from the data.

R: Thank you for this comment. We have adjusted the depths and added the reference. The new sentence is as follows " This tectonic configuration makes seismic activity in the Gulf of Cádiz significant, with moderately deep earthquakes (h < 40 - 60 km), but no subduction zone has been clearly marked (Custódio et al., 2016)."

Custódio, S., Lima, V., Vales, D., Cesca, S., and Carrilho, F.: Imaging active faulting in a region of distributed deformation from the joint clustering of focal mechanisms and hypocentres: Application to the Azores–western Mediterranean region, Tectonophysics, 676, 70–89, https://doi.org/10.1016/j.tecto.2016.03.013, 2016.

Line 119: references needed
R: References have been added.

Line 125: references needed
R: This sentence was removed.

Line 130: show location of all structures in Figure 1
R: The location of all structures are shown in Fig. 1. Thanks.

Line 136 to 149: several comments
R: This paragraph was removed.

Line 154: this is in conflict with the mention of h<30km in the gulf of Cadiz, made above
R: The reviewer is right. With the correction made on line 115, the conflict between earthquake depths has been resolved.

Line 165: explain why the DTM is needed for hazard assessment
R: The need to use DTM for hazard assessment has been explained with the following sentence "The vertical component incorporated by the DTM in the point over which the hazard is assessed is exploited in the estimation of the relative distances, from the site to the seismogenic source, that some of the GMPE use."

Line 202: why is this a good cryterium?
R: We have rewritten and clarified the sentence. It now reads as follows " These models were chosen because the ZESIS model has been used in the SHA for Spain and the NAF is the most recent model for the North African area. Additionally, both models cover geographically and almost entirely overlapping the study area of this work."

Line 222: unclear
R: We have rewritten and clarified the sentence. It now reads as follows "The complementarity and practicality of the DSHA in submerged areas with uncertain and difficult to obtain information are two of the reasons why this work has been chosen as a preliminary SHA alternative to other more complex and data-intensive hazard assessment methods."

Line 225: median of ground motion doesn't make sense.
R: Deleted.

Line 226: not clear

R: The passage has been clarified as follows "simple earthquake information and ground motion attenuation relationships".

Line 229: unclear

R: The passage has been clarified as follows "statistical earthquake (size, time and location distributions)".

Line 234 to 239: several comments

R: This paragraph was removed.

Line 241 to 259: several comments

R: This paragraph was synthesized and rewritten.

Line 260 to 275: several comments

R: This paragraph was removed.

Line 282: rewrite

R: The sentence was rewritten as follows "However, there are other distances also considered in the GMPEs catalogue formulations where the terms of Eq. (1) involving R distance are given as a function of depth (i.e., RRUP)".

Line 286: is it not sufficient to know the elevation of the site?

R: Elevation refers to altimetry and bathymetry.

Line 290: why this reference here?

R: Deleted. Thanks.

Line 295: rewrite

R: The sentence was rewritten as follows "Recent tectonic classifications (Chen et al., 2018; Poggi et al., 2020; Hasterok et al., 2022) describe much of the study region as a Variscan-Hercynian zone in an active shallow crustal region (ASCR)."

Line 300: reference needed

R: Reference Vilanova and Fonseca (2012) have been added.

Vilanova, S. P. and Fonseca, J. F. B. D.: Ground-Motion Models for Seismic-Hazard Assessment in Western Iberia: Constraints from Instrumental Data and Intensity Observations, Bull. Seismol. Soc. Am., 102, 169–184, https://doi.org/10.1785/0120110097, 2012.

Line 303: with equal weights in different regions?

R: with the same set of GMPEs and weighting coefficients applied in the logic tree scheme for the whole study area.

Line 305 to 330: several comments

R: These paragraphs have been deleted.

Line 331 to 338: several comments
R: This paragraph was synthesized and rewritten.

Line 335: using what as comparison?
R: used as benchmark points with testing values from previous published works. (Table 3)

Line 339: MCEs are not "types of strong motion"
R: This sentence was removed.

Line 364: unclear
R: The passage has been rewrite as follows “with MCE > 5 as magnitude threshold”.

Line 365: these references pertain to the models applied in this study?
R: Yes, belong to the GMPs used for deep seismogenic zones.

Line 372 to 380: this kind of theoretical framework should be given at an earlier stage.
R: This paragraph was moved to the introduction section.

Line 381 to 393: several comments
R: This paragraph was synthesized and rewritten.

Line 394: unclear
R: This paragraph was removed.

Line 400: too late for general statements like this
R: This sentence was removed.

Line 406: unclear
R: The sentence was rewritten as follows “(ii) Assign the seismic potential to each source SS_j with its MCE_j and the prevailing focal mechanism (normal, reverse or strike-slip).”

Line 412: undefined
R: This is undefined because it is really an unknown to be solved for the arbitrarily selected probability of exceedance (left hand side of the equation).

Line 414: for what pdf?
R: As has been explained before, if Y is the variance of the uncertainty assumed as a Gaussian random variable acting over the random dispersion of the model GMPE, so the pdf is a Gaussian distribution.

Line 419: what if it is not?
R: Instead of the DSHA using only the control seismic source we will be the DSHA solving the nonlinear problem of the maximum probability distribution.

Line 423: this is the median, not the mean!
R: In Gaussian distribution, as has been commonly assumed for the error model, mean and median are the same.

Line 428: th-percentile
R: notation is correct.

Line 433 to 437: several comments
R: This paragraph was removed.

Line 438 to 440: several comments
R: This sentence was removed.

Line 470: arbitrary, correct or uncertain?
R: The reviewer is right. The word "correct" was deleted.

Line 482: what are these?
R: This sentence was removed.

Line 491: what is the basic scheme?
R: Thank you for this comment, the word is incorrect. The correct word is “classic scheme”.

Line 492 to 504: several comments
R: These paragraphs have been deleted.

Line 505 to 509: unclear
R: The paragraph was rewritten as follows “The logic tree used in this study incorporates a combination of seven selected GMPEs to estimate the PGA at the site. The weights assigned to each GMPE in the tree are based on their suitability for different depths of seismic sources. The GMPEs used in this analysis are derived from studies conducted by IGN-UPM (2017) and Poggi et al. (2020) and take into account the specific rupture mechanisms associated with each source. Fig. 3b illustrates the use of our logical tree with various MCEs from sources at different depths.”

Line 551: unclear
R: This paragraph was removed.

Lines 565 and 566: for DSHA only
R: This sentence was removed.

Line 570: no information on this sentence
R: The sentence was rewritten as follows “The maximum expected MCE or magnitude of a seismic source is considered to be the upper limit, assuming that no earthquake generated by that source will exceed this magnitude (Joshi and Sharma, 2008).”

Line 576: what is the value of R taken for the MCE of each zone, and how is it determined?
R: R is determined as in classic DSHA procedure: the shortest relative source-site distance. Some GMPE uses plannimetric projected distance, others include also vertical depth distance.

Line 615: rewrite
R: This paragraph was removed.

Line 622: unclear
R: We see no error. The passage is clear and correct.

Line 623: movement?
R: Thank you for your observation. The correct word is “motion”.

Line 640: code here is unexpected
R: The code was deleted.

Line 679: median, not "average"
R: Corrected, thanks.

Line 687 to 696: several comments
R: These paragraphs have been deleted.

Line 700 to 733: several comments
R: These paragraphs have been synthesized and rewritten.

Line 736 to 752: several comments
R: This paragraph was synthesized and rewritten.

Line 757: Credible
R: Corrected, thanks.

Line 759:
R: Corrected, thanks.

Line 768: unclear
R: This sentence was removed.

Line 775: unclear
R: This sentence was removed.

Line 783: what is the control earthquake of a source? Where is it located?
R: Thank you for your observation. The correct phrase is “according to the MCE of each source”.

Line 784: the blue symbols for R>60km are hidden, use other type of marker

R: No, they are not hidden, blue markers are overlapped by red ones. overlapping (i.e. negligible DTM effect) are for sources with 100 km distance.

Line 785: unclear. The point being?

R: This paragraph was removed.

Line 792 to 807: several comments

R: This paragraph was synthesized and improved.

Line 812: The values mean different things and should not be compared. The PSHA is associated with a return period (not specified in the comparison by the way)

R: This sentence was deleted during the revision and synthesis of the manuscript.

Line 969:

R: This paragraph was revised, synthesized and improved.

Line 1004:

R: This paragraph was revised, synthesized and improved.

2) Comments of Reviewer Alexandra Carvalho:

Beltré et al. present a seismic hazard analysis, for bedrock conditions, of the marine area comprised between west Gulf of Cádiz and east Alboran Sea, based on a deterministic approach (DSHA), considering areal seismogenic sources.
Reply (R): Thank you for this comment.

The article is classified as a review paper, even though presents as stated "…the first comprehensive deterministic seismic hazard assessment carried out in the Ibero-Maghrebian region", implying that the paper should be reviewed as a research paper.
R: We have removed the phrase ¨Review article¨ from the title of the paper.

The manuscript is very exhaustive and difficult to read because of so many detailed issues, some of them not deserving such a consideration as they are very technical and can be found elsewhere, and even so, some fundamental questions and debating are lacking. The state of the art is not truly addressed. If it might be considered a research paper, more than half of the manuscript is useless and should be deleted.
R: Thank you for your suggestion. The content of the manuscript was considerably synthesized and reduced.

The study uses a mid-resolution digital terrain model, which seems a positive innovation. Nonetheless, I want to express a deep concern about this Seismic hazard assessment:
R: Thank you for this comment.

DSHA lies on the selection of the controlling earthquake (the earthquake that is expected to produce the strongest level of shaking) or a maximum credible earthquake for a specific fault. It depends a lot on the quantity and quality of the data collected, detailed active fault data and estimation of the dimensions of the fault, so that both maximum magnitude potential and source to site distance calculations result in realistic values. In the study presented, the geometry and characterization of the geological structures do not seem to have been under consideration either in terms of defining the maximum credible earthquake or in terms of the distance to the structure. In fact, considering areal seismogenic sources, a not very well understood distance (the distance to the area plus the depth of the area?) and the maximum magnitude, Mmax , of zones, I wonder why the use of a DSHA approach.
R: The authors fully agree with the point of view expressed by the reviewer. The outcome of the seismic hazard assessment is strongly conditioned by the quality of the data used to describe, not only seismically, but also the geometry of the seismogenic sources. The authors are aware of the existence of a database of faults (identified as traces on the seafloor) called QUAFI which has recently been updated with the best data and has also been used in other works, with a different purpose than the one shown in this paper. However, as this database has not been used in the published official seismic hazard map of Spain, we have not incorporated it in the set of seismogenic sources either. We have only used the same areal sources (planar geometry and seismic characteristics) in order to compare our results with those obtained in this map since we have no other control information in the marine area. With the seismic information of the maximum credible earthquake in each source, and with the distances that each attenuation function

incorporates in its mathematical expression (according to the catalogue published by Douglas (2020); some are plannimetric distances and others are 3D distances) we have evaluated the seismic hazard according to a classical deterministic scheme as a first preliminary step in a study of this type, before using more sophisticated tools such as PSHA or NDSHA.

A few More comments:

line 36 …a Mw 7.7 and a unique reference for the 1755 Lisbon earthquake. Then, in line 119, it is mentioned a magnitude 8.5, with no reference. The same for the 1969 earthquake.
R: The reviewer is right. We have corrected the magnitudes and added more references to the Lisbon and Cape St. Vincent earthquakes. Thanks.

Line 43 " …and mitigate seismic hazard" ?. Risk is mitigated, not seismic hazard.
R: This sentence was deleted during the revision and synthesis of the manuscript.

Line 221: hazard IS NOT the probability that an earthquake occurs in a given geographic area.!!!
R: The reviewer is right. We have corrected the definition of seismic hazard. Thanks.

Line 285 : it is not true the statement " … obtaining higher Y because they are closer to seismogenic sources", as, as an example, directivity changes profoundly results.
R: This sentence was deleted during the revision and synthesis of the manuscript.

And many mistakes easily corrected.

I would recommend a deep level of review, by the authors, before trying to submit it again.

R: Thank you for your recommendation. An exhaustive review and synthesis of the paper content was performed.