Dear Editor,

Please find attached the second version of our revised manuscript. We have carefully considered all the comments.

The first review (RC2) is a detailed and constructive review. We did our best to account for most points raised and to incorporate most suggestions. When we do not incorporate the suggestion, we explain why. Sometimes the manuscript has been re-written.

The second review (RC3) is identical to its first version, the only request is to include 6 references. We looked into these articles and now provide a very short summary of them in our response. As these articles are not directly linked to our work, we had included only 1 reference in the 1st revised version of our manuscript. Because the reviewer insists, we now include 3 of them.

Responses to reviewers' comments can be found below. The questions are presented in black, and our answers are shown in green. Each modification to the manuscript text is indicated in blue, with the corresponding text of the revised manuscript provided.

In addition, all prior changes in the manuscript are highlighted in yellow, while new changes for the second review are highlighted in orange for easy reference.

Thank you for considering this revised version of our manuscript.

Sincerely,
Bénédicte Donniol Jouve on behalf of the co-authors

RC2
Second Review of the manuscript egusphere-2024-787

After reading this manuscript again, I am delighted to know its significant improvement. Most of the reviewers' comments have been considered. However, I still have some comments and questions, listed below. There are a few adjustments, which could improve the manuscripts.
In many cases, the reviewers' comments have been addressed by adding citations only and without going into details.
I think the authors should explain some points better to make this manuscript a stand-alone paper which could be understood by the readers without checking the references. Some explanations, which are provided in the authors' response file, should be included in the manuscript, such as the response to Line 181, the response to Figure 4, and the response to Line 269 by the 2nd reviewer.

As a reminder, the question mentioned line 181 was :
"Line 181: Explain how 12 from "the 12 difference preprocessing parameters" comes from."
We modified the text as follows :
"Figure 4 displays the exploration tree set up to combine 12 different preprocessing parameters to filter the stations of the GNSS velocity fields (three selections of GPS stations times four outlier radii), with 12 different regularizations of the velocity fields inversion to determine strain rates (choice of the distance and spatial weighting scheme, choice of the weighting threshold) and with finally 36 different parameterizations to calculate the moment rate from the strain rates."

As a reminder, the comment for Figure 4 was :
"Figure 4: I would suggest including also the weights associated with each branch in Figure 4a and explaining how they were defined."
This is an exploration tree, not a logic tree as used in PSHA. Weights are not needed, we just combine all the alternative parameters. We fear that referring to weights might bring confusion. We made the following modifications in the caption of Figure 4a.
"Exploration of the tree results in 3x4x2x2x3x3x3x2x2=5184 alternative moment rate estimates."
and
"distribution of the geodetic moment rate estimates (histogram built from the 5184 values)"

As a reminder, the comment for line 269 was :
"Line 269: What are the reasons for the lack of good fit in Spain when the macrozones are used? Which are the specific criteria used to assess that the overall fit is good from Figure 11? I would say that the fit is relatively good only for the highly seismic regions, not for central and northern Europe looking at Figure 11."
We have re-written this part as follows:
"In regions of northern Europe, the overlap between geodetic and seismic moment distributions is low (below 35 per cent, in red). A rather good fit is obtained for the

Euro-Mediterranean region (overlap above 35 per cent, in blue), except in Spain (Fig. 11). A more detailed analysis focusing on Spain would be necessary to understand why.”

Some more points are the following.
1) The addition to the section conclusion is very little, it still seems to be more a summary than a section Conclusion. The strengths and limitations of the study could be better highlighted.

We agree that the conclusion needed to be re-written. Please find the new conclusion:

“Many studies have been published that study how GPS strain rate is correlated with changes in observed seismicity (e.g. Zeng et al. 2018). Far less studies have focused on the comparison between strain rates and long-term earthquake forecasts built for assessing probabilistic seismic hazard. These long-term earthquake forecasts rely strongly on past seismicity, whereas geodesy offers an independent view on the amount of deformation that might be released in the future. In the present study, we have compared the new European seismogenic source model (Danciu et al. 2024) with a strain rate model developed at the European scale (Piña-Valdès et al. 2022). The comparison is led in terms of moment rates.

For every area source zone of the ESHM20 source model, we have established a distribution for the geodetic moment rate, which accounts for uncertainties in the selection of GNSS stations, the calculation of the strain rates and the conversion into a moment rate. At the source zone scale, we compare the geodetic moment rate distribution with the seismic moment rate distribution, as inferred from ESHM20 source model logic tree. We show that the geodetic moment rate is rather well correlated with the seismic moment rate in the most seismically active regions of Europe (e.g. the Apennines, Greece, the Balkans, the Betics, southeastern France), whereas in the low seismicity regions the geodetic moment rate is much higher than the seismic moment rate (e.g. Parisian basin, northern and central Europe, Fennoscandia). Results show that both estimates are slightly more consistent when considering larger spatial regions. In moderate to high seismicity regions, the geodetic strain is in general representative of the current horizontal tectonic stresses. In the very low seismicity region of Fennoscandia, the geodetic signal might be dominated by glacial isostatic adjustment and the strain does not represent the long-term tectonic loading.

More work is needed to understand the consistencies or discrepancies obtained between strain rate based moments and moments relying on the long-term magnitude-frequency distributions built for PSHA. Some parameters such as the seismogenic thickness will need to be better evaluated to refine the estimation of the moment rate from strain rates. In regions of low seismicity where the geodetic moment rate appears disconnected from the seismic moment rate, for now this is not clear how geodetic data can contribute to establish long-term earthquake forecasts. However, in seismically active regions, our work demonstrates the strong correlation between long-term seismic moment rates and geodetic moment rates . In these regions, strain rates should be used to constrain earthquake forecasts for PSHA, either combined with earthquake catalog data, or as an alternative model independent from the earthquake catalog.”

2) I would not completely agree that the comparison between this study and the geodetic model for Italy is outside the scope of this manuscript since it could validate and strengthen the analysis conducted here.

The present paper is aimed at comparing moments based on the Pina Valdes et al. (2022) strain rate models and those inferred from the long-term magnitude-frequency distributions used for calculating probabilistic seismic hazard at the scale of Europe (ESHM20, Danciu et al. 2024). We agree that it would be interesting to compare the strain model obtained in Pina-Valdès et al. (2022) at the European scale, with the strain rate model derived for Italy by N. D'Agostino and used in Meletti et al. (2021). If the Italian model was available, we would have included this comparison in the Pina-Valdès et al. (2022) article. However the Italian strain rate model has not been published yet. It is very briefly described in Visini et al. (2021), in section 3.3.4.1, as well as the gridded seismicity model derived from it, named MG1. Visini et al. (2021) only provide the gridded-seismicity rates (Fig. 8 in their paper, the figure is small and the details can't be seen, same as Fig. 3 in Meletti et al. 2021). No proper comparison can be led with our strain rate model. As for the second geodetically-based source model in Meletti et al. (2021), MG2, the underlying methodology is quite different and the comparison would not be straightforward. As for MG1, the Meletti et al. and Visini et al. papers only provide a brief description and the seismicity rates based on MG2.
We have augmented the introduction to provide details on the MG1 gridded seismicity source model based on strain rates (see below), as well as on the more complex MG2 model. We don't think we can do more at this stage.

3) There are still editorial issues with the use of parentheticals for citing references, e.g (e.g. Zeng et al. (2018)) should be (e.g. Zeng et al., 2018) on line 56; (see Danciu et al. (2021)) should be (see Danciu et al., 2021) on line 101-102; Fault-Source Model 2020 EFSM20, Basili et al. (2023)) should be Fault-Source Model 2020 EFSM20, Basili et al., 2023) on Line 81; Mariniere et al. (2021)) should be Mariniere et al., 2021); etc.
Done

4) I would suggest an English proofreading before the final version is ready to be published because the English language is quite poor in some parts. I report only some of them below.
We have reread the entire article and checked every sentence. Corrections performed are visible in the version of the manuscript with tracked changes.

Below there are a few (technical or editorial) comments on the manuscript.
Line 9: Replace "high activity zones" with "highly seismic activity zones".
Done

Line 11: What does "local disparities underscore" mean? I would suggest rephrasing it.
We have rewritten this part of the abstract.

Line 12: Replace "low-to-moderate activity zones" with "low-to-moderate seismic activity zones".
Done

Line 20: replace "and its update the European" with "the updated European".

Done

Line 21: Faults are actually included in the seismic source models in active countries, such as South Europe, Turkey, Japan, California, etc. So probably the sentence "In regions where active faults are rather well-characterized, they must be accounted for in the hazard estimations" should be corrected.

This sentence is within a paragraph that states that seismic hazard models include fault models in regions where faults are rather well-characterized (see below), so we don't fully understand this comment. Nonetheless, we have modified the sentence :

"they must be accounted for" => "they are accounted for".

in the following paragraph:

"Nowadays, source models in up-to-date probabilistic seismic hazard studies are based both on past seismicity and active tectonics datasets. For example, the source model logic tree in the European Seismic Hazard Model 2013 (Woessner et al. 2015) and the updated European Seismic Hazard Model 2020 (Danciu et al. 2024) include two main branches, an area source model and a fault model. In regions where active faults are rather well-characterized, they are accounted for in the hazard estimations (Stirling et al., 2014, Field et al. 2014, Beauval et al., 2018). Fault models are mostly based on geologic information, covering much larger time windows than the available earthquake catalogs."

Line 26: replace "in the past, as the two" with "in the past, such as the two".

Done

Line 33: Delete the second and third "on".

Done

Line 41: Delete the second "if".

Done

Lines 55-56: It is not enough to add only a citation to describe the Italian geodetic model. The authors should briefly explain what "the method of Carafa et al, (2017)" is. How does the approach used to derive the Italian geodetic data differ from that used to derive the model of Piña-Valdés et al. (2022)? This is not beyond the scope of this manuscript since it gives the background and explains what already exists in the literature.

We have significantly augmented the paragraph to include a description of the two geodetic source models used in Meletti et al. (2021) :

"However, to our knowledge, in Europe, the only seismic hazard model that integrates a source model based on strain rates is the new Italian hazard model (Meletti, 2021). The gridded-seismicity model MG1 (Visini et al. 2021) relies on a strain rate tensor field calculated using the VISR software (Shen et al. 2015), as in Piña-Valdés et al. (2022). The rate of seismic moment is converted into earthquake rates assuming that earthquakes are distributed according to a tapered Gutenberg-Richter. Two alternative seismogenic thicknesses are considered (7 and 13 km). The total seismic rate is scaled to the seismic moment release of the Italian catalog (Visini et al. 2021). Meletti et al. (2021) also includes a second more complex geodetically-based source model, MG2. In this case, the model relies on the NeoKinema code (Bird 2009) that delivers interseismic and long-term strain rates and velocities on a finite element grid (see Bird and Carafa 2016)."

We must underline that the strain rate model which relies on a methodology close to the Piña-Valdés et al. (2022) model, and that has been used to establish the gridded-seismicity model MG1, has not been published. It is only briefly described in Meletti et al. (2021) and Visini et al. (2021). The strain rates are not displayed, only the gridded-seismicity rates obtained. MG1 relies on strain rates that have been derived with a methodology comparable to Piña-Valdés et al. (2022). However, before conversion into seismic rates, the total seismic rate is scaled to the seismic moment release of the Italian catalog (Visini et al. 2021). A fraction of the geodetic moment is thus assumed aseismic, so that as a whole over Italy, the rates forecasted fit the observed rates. What we do in the present article is quite different. We simply compare the moments, based on the strain rate models, and based on the ESHM20 long-term MFDs. We do not use the strain rate model to establish a source model for PSHA.

Bird, P. (2009). Long-term fault slip rates, distributed deformation rates, and forecast of seismicity in the western United States from joint fitting of community geologic, geodetic, and stress direction data sets, J. Geophys. Res., 114, B11403, doi:10.1029/2009JB006317.

Bird, P. and M. M. C. Carafa (2016). Improving deformation models by discounting transient signals in geodetic data: 1. Concept and synthetic examples, J. Geophys. Res. Solid Earth, 121, doi:10.1002/2016JB013056.

Shen, Z.K., M. Wang, Y. Zeng and F. Wang (2015). Optimal Interpolation of Spatially Discretized Geodetic Data, Bull. Seismol. Soc. Am., 105, 2117-2127.
Visini et al. (2021), annals of geophysics, doi:10.4401/ag-8608

Line 56: Including one reference only contradicts “A number of studies…”. Include more references. Also, did they use the same approaches?
We have added the reference Kreemer and Youngs (2022), which is also on the relationship between strain rates and seismicity.
We have also added the references Riguzzi et al. (2012) and Farolfi et al. (2020) (review by RC3, the only comment of this reviewer is to add 6 references of articles on the relationship between observed seismicity and strain rates).

Kreemer, C., and Z. M. Young (2022). Crustal Strain Rates in the Western United States and Their Relationship with Earthquake Rates, Seismol. Res. Lett. 93, 2990–3008, doi: 10.1785/0220220153.

Line 69: Delete “the” in “the most the compatibility”.
Done

Line 72: Replace “ESHM20 aims” with “ESHM20 aimed”. Also, update the citation Danciu et al. (2021) with the peer-reviewed article Danciu et al. (2024) throughout the manuscript.
In some cases, we keep both the Danciu et al. (2001) EFEHR report reference, publicly available on the EFEHR website, and the article Danciu et al. (2024), as the EFEHR report contains information that is not in the peer-reviewed article. In the other cases, we modified the references.

Line 73: Remove the space before the colon (in components :). This editorial typo appears often in the text, including in the caption of Figure 10 (seismic moment rate : 1 : ITAS308, 2 : ITAS331, 3 : ITAS339, 4 : BGAS043, 5 : FRAS164, 6: DEAS113, 7 : DEAS109, 8 : CHAS071).
Done

Lines 74-75: “that” is repeated twice in the same sentence. I would suggest rephrasing it for better readability.
Done

Line 87: Add “and” after the comma in “geologic features, seismicity pattern”.
Done

Line 89: Add “catalogue” before “completeness”.
Done

Line 91: Note that form 2 is not capital letter throughout the manuscript. The authors should check this.
Done

Formula 1: Explain what Mmax, N(m), a and b are. Also, does the minimum magnitude not appear in equation 1?
We added the explanation in the text according to your advice :
“where N(m) represents the cumulative annual rate of events as a function of magnitude (m); a and b are the Gutenberg-Richter recurrence coefficients, respectively the productivity and the exponential coefficient ; and Mmax is the maximum magnitude.”

Line 94-95: It is still unclear what the corner magnitude is. Is the bending at large magnitudes? How does it relate to the minimum magnitude for the calculations of the recurrence parameters? It would be useful to include the formula perhaps.
The corner magnitude is required by the well-known tapered Pareto distribution.
We have added the following sentence:
“With respect to the Anderson and Luco (1983) magnitude-frequency distribution, the sharp cutoff at a maximum magnitude in the truncated distribution is replaced by smooth tapering.”

Line 100: Leonard (2014) is an update of Leonard (2010) for stable continental regions only. Which one was used? Leonard (2010) for active shallow and subduction regions and Leonard (2014) for stable continental regions? The authors should be more precise. Also, replace “it” with “and”.
Leonard (2014), with title ‘Self-Consistent Earthquake Fault-Scaling Relations: Update and Extension to Stable Continental Strike-Slip Faults’, is an update of Leonard (2010) for stable continental regions as well as for active regions (interplate dip slip, interplate strike slip, SCR/intraplate dip slip, and SCR/intraplate strike slip).

Lines 100-101: I do not see more explanation added here to explain the smoothed seismicity model and the adaptive kernels. Adding only a citation is not enough to make this manuscript a stand-alone, independent paper. It would be useful to include the size of the spatial cells as indicated in the response file.

We have added the following description:
“The smoothed seismicity model is developed by optimizing the adaptive kernel bandwidth, the smoothing parameters and the declustering parameters. Training and validation sets are used to determine the optimal combination of parameters. Details are provided in the EFEHR report (see, Danciu et al. 2021).”

Line 108: Move “recurrence models” after Pareto.
Done

Line 122: Replace “at the scale of Europe” with “at the European scale”.
Done

Line 127: Add a colon after “inverse problem” (without any space after “problem”).
Done

Line 132: Add a full stop before “however”.
Done

Line 134: Add “, i.e.” after “categories”.
Done

Line 136: Delete finally.
Done

Line 142: Leave a space between 142 and km. A space between a number and km should be checked throughout the manuscript.
Done

Line 143: Change “the radius is increased, the” with “the radius increases, the”.
Done

Line 144: Add “is” after “this radius”.
Done

Line 147: Change “a number of decisions are required that may impact” to “a number of required decisions may impact”.
Done

Lines 149-150: Quadratic and Azimuthal should not have capital letters.
Done

Formula 3: Explain what n is.
Thank you for your careful review. This was indeed an error; *ncell* and *n* represent the same value in Equation 3. We have revised the equation accordingly.

Line 167: Remove the comma before “uses”.
Done

Line 172: remove the comma before “propose”.
Done

Formula 7: What is AX?
This was a misunderstanding: we had written MAX, not AX. We have adjusted the formula to enhance clarity.

Line 172: remove the comma before “uses” and remove the s in “uses”. As indicated in my previous review, the use of the comma should be checked more carefully.
Done

Line 179: Remove “focused” and add “of Cg” after two values.
Done

Line 180: Replace “consider two values,” with “consider two Cg values,”. It is unclear how the selected values of 2 and 2.6 were computed for dip = 25 and 65. Since one of them is Cg =
2 as in Stevens and Avouac (2021), I assume that the dip angle should be also the same, i.e. 45, and not 25. Something here seems to be incorrect.
Done

Lines 182-183: It is not enough to cite the work of Dziewonski and Anderson (1981) to justify the alternative values of the shear modulus. A brief explanation should be added here.

We have included Burov (2011) to support the 30 GPa reference alongside the 33GPa proposed by Dziewonski and Anderson (1981). We have modified the text as follows :
“The uncertainty on the shear modulus is also taken into account, including two alternative values proposed for continental crust : $3.3 * 10^{10} N.m.yr^{-1}.km^{-2}$and
$3.0 * 10^{10} N.m.yr^{-1}.km^{-2}$ (e.g. Dziewonski and Anderson, 1981 ; Burov 2011) and widely used in the literature (e.g. Stevens et Avouac 2021; Working Group on California Earthquake Probabilities, 1995; Mazzotti and Adams, 2005)”

The shear modulus μ is commonly considered as the bulk shear modulus for the crust. For continental regions, two standard reference values—33 GPa and 30 GPa—are commonly used (Dziewonski and Anderson, 1981; Burov, 2011). These values are those used in the literature (e.g., 33 GPa Stevens and Avouac, 2021; and 30 GPa in Ward, 1998a; Working Group on California Earthquake Probabilities, 1995).

Burov, Evgene B. (Aug. 2011). “Rheology and strength of the lithosphere”. In: *Marine and Petroleum Geology* 28.8, pp. 1402–1443

Lines 187-194: For the selection of the seismogenic thickness, using the case study of eastern North America does not seem correct for the highly seismic South Europe. Also, how to justify a thickness between 5 and 15 km for the seismicity with hypocentral depths of 20-25 km, which is present in Europe?

We mention the work of Mazzotti et al. in Western Canada for introducing the concept of effective seismic thickness, which is smaller than the seismogenic depth. The effective seismic thickness is not the total thickness over which earthquakes occur.
We then provide examples of the thicknesses used in different published works similar to our work, in Western US (15km), in Italy (10 km, 3 and 8 km), in the India-Asia collision zone (15km). All the studies mentioned are using strain rates to infer the moment that is released in earthquakes.
The thicknesses we use are within the range of the thicknesses used by previous studies in similar tectonic contexts.
To complement the paragraph, we have added the following sentence:
"Using strain rates to forecast earthquakes in the Italian seismic hazard model, Visini et al. (2021) assume elastic thickness equal to 7 and 13 km throughout Italy). As there is considerable uncertainty on this parameter, based on this literature review, we use three alternative values (5, 10, and 15 km) and propagate this uncertainty up to the geodetic moment rate estimates."

Line 204: Replace "the most" with mainly or mostly.
Done

Line 215: Remove the comma after "variability".
Done

Line 223: Replace "substantial" with "strong".
Done

Lines 230-231: What is the impact of using different equations to calculate the geodetic moment since it is non-negligible? This sentence should be expanded.
We have modified the sentence as follows:
"The results also highlight that the equation used to convert surface strain into scalar moment rate can have a significant impact (in blue in Fig. 5). The uncertainty on the choice of the equation contributes to the overall variability of the moment rate."

Figure 5: In the caption replace "Full" with "Full distribution" as indicated in the x-axis. The labels in the y-axis of the top plots are still missing.
Done

Line 245: Replace "realistic the model is" with "realistic ESHM20 is".
We are addressing only the source model of ESHM20, not the hazard results.
We correct the sentence: 'how realistic it is.'

Line 257: Replace "stay" with "are" or "lie". Furthermore, this sentence is unclear because 1) the brackets are misplaced since it seems to be related to low-seismicity regions; 2) the sentence "seismic moment rates go down to much lower values" seems to be incomplete [than what??] and which regions are these seismic moment rates related to?
"stay" has been replaced by "lie".

We have suppressed the sentence "Besides, we observe that in low-seismicity regions, geodetic moment rates lie between ≃10^{11} and 10^{12} $N.m.yr^{-1}.km^{-2}$ (in blue and green, in mainland Spain and France, northern Europe and Fennoscandia) whereas the seismic moment rates go down to much lower values."
Figure 8 is much more relevant for discussing what is observed in low seismicity regions.

Lines 274-275: This sentence is unclear so the authors should rephrase it for better readability.
We have re-written the sentence : "The distribution for the seismic moment is built by exploring the ESHM20 source model logic tree, taking into account the weights associated to each branch."

Line 299: The beginning of the first sentence should be rephrased because beginning with Let's is not suitable for a manuscript.
We changed the sentence into 'In eight area sources, the geodetic moment rate is, on average, significantly lower than the seismic moment rate.'

Line 305: " The source zones 305 CHAS071 (Switzerland), DEAS113 and DEAS109 (Germany) are not as active" [as WHAT??]. This sentence does not seem to be complete.
We added 'as FRAS164' for more clarity.

Line 311: Replace "is inferred both from the larger macrozone and from the number of earthquakes" with "is inferred from both the larger macrozone and the number of earthquakes". Also, the citation should be (Danciu et al., 2021) and should be replaced with the updated citation Danciu et al., 2024.
We have re-written the sentence, please see the next comment.

Lines 310-312: How was the final a value computed? Using the macrozones or the individual zones? This sentence could be improved to make clearer how the activity rate was estimated.
We have re-written the sentence:
"There are too few data to constrain the model, the b-value is inferred from the larger macrozone, whereas the seismic activity is estimated by re-scaling the occurrence rates as a function of the number of complete earthquakes (the scaling factor is the ratio between the number of complete events in the area source and the number of complete events in the corresponding macrozone, Danciu et al. 2021)."
It is important here to keep the EFEHR report reference, because this level of detail is not in the peer-reviewed article.

Lines 331-332: Include references for this sentence.
The sentence you referred to is as follows:
"GIA generates a viscous asthenospheric flow and a large scale flexure of the overlying elastic lithosphere that results in rather large wavelength deformation (i.e. the strain is distributed over a large area)."
The citation of Mazzotti et al. (2011) and Piña-Valdes et al. (2022) has been added.

Mazzotti, S., A. Lambert, J. Henton, T. S. James, and N. Courtier (Dec. 28, 2011). “Absolute gravity calibration of GPS velocities and glacial isostatic adjustment in mid-continent North America: AG CALIBRATION OF GPS AND PGR”. In: Geophysical Research Letters 38.24.

Lines 332-334: Include references for this sentence.
The sentence is as follows:
“It should also be noted that the postglacial rebound is a phenomenon that is not representative of the long term (a few Myrs) tectonics, but that it is a transient mechanism that started after the last glacial maximum, ≈ 20,000 years ago.”
The citation of Steffen and Wu (2011), has been added.

Steffen, Holger and Patrick Wu (Oct. 1, 2011). “Glacial isostatic adjustment in Fennoscandia—A review of data and modeling”. In: *Journal of Geodynamics* 52.3, pp. 169–204.

Line 370: Remove one bracket in “part))”.
Done

Lines 389-390: replace “As a consequence, and as seen at the scale of macrozones (Fig. 11), this discrepancy is reduced at a larger scale because of a spatial smoothing of the signal.” with “This discrepancy is reduced at a larger scale because of a spatial smoothing of the signal as seen at the scale of macrozones (Fig. 11).”.
Done

Line 425: Replace “within” with “in”.
Done

Line 430-431: Why does FRAS164 behave differently from the surrounding zones?
The case of the FRAS164 zone is discussed in Section 3.1.3.
We have modified the sentence as follows:
“There are exceptions, such as FRAS164 in the Western Pyrenees, a small zone with a high seismic activity with respect to the rest of the Pyrenees (as explained in Section 3.1.3).”

Conclusions: Report examples of the regions when describing the results, for example, “in areas with small characteristic distances, such as XXX” in line 440; “In some of these areas, such as XXX-XX” in line 447; “whereas in others (e.g. XX-XXX)” in line 448; etc.
We have re-written the conclusion and provide examples of regions.

Lines 445-446: Include references for this sentence.
The conclusion has been rewritten and we don’t go into this detail in the new version.

Lines 456-457: I don’t find this sentence correct. In regions of slow deformation and low seismicity, it is often difficult to include the tectonic structures as fault sources in the seismic source model because the information on their geometry, the rupture behaviour, and the maximum magnitude they are capable of generating is incomplete or unknown. Furthermore, although the overall deformation rate in the region may be known, it is difficult to partition it among the active tectonic structures and thus estimate the activity rate of the individual faults.

The sentence you mention is “the inclusion of active faults may therefore strengthen the earthquake recurrence model in areas that are characterized by both a slow deformation rate and rare seismic events.”
We agree with you, we have suppressed this part of the conclusion.

References

Danciu, L., Giardini, D., Weatherill, G., Basili, R., Nandan, S., Rovida, A., Beauval, C., Bard, P.-Y., Pagani, M., Reyes, C. G., Sesetyan, K., Vilanova, S., Cotton, F., and Wiemer, S.: The 2020 European Seismic Hazard Model: overview and results, Nat. Hazards Earth Syst. Sci., 24, 3049–3073, https://doi.org/10.5194/nhess-24-3049-2024.

RC3

The reviewer asks for minor revision: “the article must include the most recent studies”
In his/her original review, the following studies were listed:
ART1 : Nakamura, M., Kinjo, A. Activated seismicity by strain rate change in the Yaeyama region, south Ryukyu. Earth Planets Space 70, 154 (2018).
ART2 : Pappachen, J. et al (2021). Crustal velocity and interseismic strain-rate in the Garhwal–Kumaun Himalaya. Scientific reports, 11(1), 1-13.
ART3 : Zeng, Y. et al (2018). Earthquake potential in California-Nevada implied by correlation of strain rate and seismicity. Geophysical Research Letters, 45.
In the paragraph 1.4 Focus in Italy, please produce a comparative and critical analysis with the following previous studies, focussing on the difference of the applied methods and the conclusion:
ART4 : Riguzzi, et al (2012). Geodetic strain rate and earthquake size: new clues for seismic hazard studies. Physics of the Earth and Planetary Interiors, 206.
ART5 : Farolfi, G., et al (2020). Spatial forecasting of seismicity provided from earth observation by space satellite technology. Scientific reports, 10(1), 1-7.
ART6 : Piombino, A. et al (2021). Assessing current seismic hazards in Irpinia forty years after the 1980 earthquake: Merging historical seismicity and satellite data about recent ground movements. Geosciences, 11(4), 168.

In the first version of our revised manuscript, we had added the reference Zeng et al. (2018). These papers are not directly related to our work, we are surprised that we are asked again to include all of them. Because we are in the second round of reviews, we have added two more articles, Riguzzi et al. (2012) and Farolfi et al. (2020), in the introduction. Please find below our explanations.

There are a large number of studies published studying the relation between strain rates and seismicity. We would like to underline that our study is different. We never compare the strain rate based moments with observed seismicity. We compare the strain rate based moments with seismic moments inferred from a probabilistic seismic hazard model (ESHM20). The seismic moments are obtained by integrating long-term magnitude-frequency distributions. These distributions are calibrated on observed seismic

rates (instrumental and historical earthquakes) and often extrapolated up to a maximum magnitude.
We have included the references that are directly related to our work, such as :

- Meletti et al. (2021) and Visini et al. (2021) are using a strain rate model for establishing a source model for probabilistic seismic hazard, we describe and cite this work.
- Carafa et al. (2017) developed a method to include strain rate in the building of a fault model for probabilistic seismic hazard assessment, the article is mentioned and cited.
- Jenny et al. 2004
- Stevens and Avouac 2021
- Mazzotti and Adams 2005

The 6 articles that the reviewer asks us to include are not comparing strain rates with long-term magnitude frequency distributions:

Nakamura and Kinjo (2018) evaluate the long-term strain rate by using GNSS data and compared it with seismicity activation during a 10 yrs period in the Ryukyu trench. They conclude that the long-term seismicity near Iriomote Island is strongly affected by changes in the crustal strain rate. This paper could have been cited in the Pina-Valdes et al. 2022 article, but we don't know what would be the reason for absolutely including it in the present study ?

Papachen et al. (Scientific Reports 2021) analyze the interseismic strain rate using GNSS data in the Himalaya, to identify zones where large earthquakes could occur. They identify high compressional zones, extensional deformation zones, equal strain rate zones and low strain rate zones. They analyze crustal accommodation processes through the strain rate patterns. This study is only remotely close to our work.

Zeng, Petersen, Shen (2018). They correlate GPS strain rates with seismicity in California. They show that earthquakes of $M > 6.5$ are collocated with regions of highest strain rates, whereas smaller magnitude earthquakes of $M \geq 4$ show clear spatiotemporal changes. They show that seismicity is closely related to the strain rate, and that, as the deformation field evolved out of the shadow in the late 1980s, strain has refocused on the major fault systems entering a period of increased risk for large earthquakes in California.

Riguzzi et al. (PEPI 2012) perform an analysis of the background strain rate in Italy and comparison with seismicity over a 5 years period. They conclude that the strain rate map may be a powerful tool for identifying the areas prone to the next earthquake.

Farolfi et al. (Scientific Report 2020) aims at understanding what controls the distribution of the seismicity. They use a strain rate field determined from GNSS data and satellite radar interferometry over a 20 years period. They study the correlation with $M \geq 2.5$ earthquakes that occurred in the same period. They found that earthquake occurrence probabilities are linearly related to strain rates. They conclude that strain rates can be used to forecast seismicity.

Piombino, Bernardi and Farolfi (Geociences 2021), entitled “Assessing Current Seismic Hazards in Irpinia Forty Years after the 1980 Earthquake: Merging Historical Seismicity and Satellite Data about Recent Ground Movements” use a strain rate map in Italy and show that there is a link between the strain rate and the shallow earthquakes, with their epicenters being placed only in high strain rate areas. The article analyzes the strain rate map with respect to the occurrence (or absence) of historical earthquakes.