Reply to RC1

We would like to thank the anonymous reviewer for his evaluation of our work, whose suggestions will certainly contribute to improving the quality of our manuscript. We have considered each suggestion thoroughly and have taken steps to implement them in a meaningful way. In this response, we provide detailed explanations and revisions that reflect our efforts to address the reviewer's comments. In addition, where necessary, we have carefully incorporated these improvements into the revised version of the manuscript to ensure that the final document reflects our commitment to excellence and responsiveness to feedback.

1) The number of citations is extremely small with respect to the argument. Seismic hazard assessment is a complex procedure that consider the previous experiences and the many data used for building the model; many affirmations made by the authors without an adequate reference must be referred to the authors themselves, but often this is not the case. In the following some examples.

   In the new version of the manuscript, we have included new references to existing literature, also in accordance with the following suggestions.

2) The previous model for this area has been released more then 10 years ago (in 2012 the EMCA project released its model in the frame of the GEM activities). As the authors refer, after 2012 many studies have been realized in the single countries: in the period 2018-2021 all the country involved in this project were studied by several authors, in some cases with applications of the model to the building code. So, it's not clear why a new study is necessary and what are the main criticalities of the previous works, if any. Please, add a comment on this issue.

   The SFRARR project was led by a consortium of international scientists from various private and public research organisations, including representatives from the Central Asian countries mentioned in this study. Some of these representatives were also involved in the national initiatives mentioned in the introduction. The consortium directly incorporated their experiences into the model presented. The feedback from these national experts influenced many modelling decisions, including the zonation of the sources (six versions were progressively developed and discussed during the project), the homogenisation of the data, the tectonic regionalisation and much more.
   Among the benefits of the present study, it should be noted that unlike the national initiatives, which were primarily carried out on an individual basis, the aim of the SFRARR project was to harmonise these contributions into a single regional model. This approach was intended to build on and improve previous efforts, such as the EMCA model published in 2012, by bringing together different expertise and data sources into a comprehensive and harmonised framework.

3) On row 107, the authors comment that the hybrid approach allows a more realistic representation of the seismicity. In my opinion, in general this is true, but it depends on the practice of design large seismic source area due to the poor knowledge about the seismogenic processes.

We agree with the reviewer's comment on the general usefulness of the hybrid approach to seismic hazard modelling. This approach is particularly valuable when the delineation of higher resolution source areas proves difficult due to limited seismogenic constraints or, alternatively, when very large regions are considered. In any case, it is fair to point out that accurate zoning can work as well or even better than hybrid modelling under certain conditions, provided that the local process of earthquake generation is sufficiently understood. We have emphasised this consideration more clearly in the manuscript.

4) Section 3 contains very few information about the definition of the source models. As far as I understood, the definition of seismic source zones (figure 1) is based only on seismic information. There is not any seismotectonic consideration. Is it correct?

   Unfortunately, important details on the development of the model were not included in the manuscript for reasons of space. To counteract this limitation, the most important project results have been divided into two accompanying publications within this special edition by the same team of authors. However, further information can be found in the World Bank's online project report, which is freely accessible on the World Bank's documentation platform.
   Regarding the definition of the seismic source areas shown in Figure 1, we confirm that our approach has taken into account all available seismotectonic information and has not only analysed the distribution of seismicity. These considerations were discussed in detail in several project workshops with local experts during the construction phase of the source model, where several versions were proposed and iteratively refined.
   While it is not possible to discuss all seismotectonic considerations in detail in the manuscript, we have added a clarification to address this aspect more explicitly (see also answer to question n. 6).

5) Row 132: what does it means that this is the accepted version of the source model? From whom? Is it this information useful or necessary?

   We agree with the reviewer that the current definition lacks context and could be misleading. The acceptability of the proposed model version refers to the entire creation process, which aimed to reach a reasoned consensus among the consortium participants. It is important to emphasise that scientific partners in Central Asia and local representatives from different countries actively contributed to both the development and review of the model. Therefore, it was necessary to reach a certain level of consensus considering the different scientific views on certain controversial issues.

6) Also, the definition of the tectonic groups it's not fully explained. When the authors write that the groups "are assumed to have comparable behavior…" on what basis their judgment is based? Only the earthquake catalog or other data? It is also missing any comment or comparison with previous source model.

   We understand the reviewer's concerns. Unfortunately, due to space constraints, it was not possible to summarise the numerous considerations that led to the development of

the zonation model. As briefly highlighted in the manuscript, the grouping was done by combining the analysis of seismicity data with seismotectonic considerations.
For example, statistical data on the distribution of focal mechanisms and the empirical magnitude frequency distributions were analysed together with the characteristics of the main active fault systems (presented in the companion paper to this study) in the context of regional tectonic structures and boundaries.
To provide a practical example of the construction process, Zone D was found to encompass a tectonic domain that is clearly separated by the stable features of the West Siberian craton (Zone E). As also indicated by the available source mechanisms, Zone D is characterised by a mixed regime, albeit with a dominance of large transpressive fault systems (e.g. Talas-Fergagna fault, Irtysh shear zone) that have influenced the southeastern evolution of the Tianshan Massif (Chen et al. 2022). Towards the south, a change in seismotectonic style becomes evident (Zone G), where the main reverse mechanisms increasingly dominate and large trust systems develop along the suture zone with the former cratonic terrains of the Tarim region (Angiolini et al., 2013). Here, seismic productivity is increasing and large magnitudes have occurred in the past. Further south, a mixed tectonic style is again present (Zone C), while seismicity becomes typical of continental collision (Zone F), with larger and deeper events along the Pamir thrust system (e.g. Murodov 2022). Towards the west, a clear separation between the tectonic styles of the systems at the boundary between the Turan Platform (Zone B) and the Karakum terrains (Zone A) has also been noted along the ideal southwestern extent of the Pamir suture zone (see Ghassemi and Garzanti, 2018 for a comprehensive review).
We have now expanded the discussion in the manuscript, although an exhaustive description of the entire argument supporting the construction of the zonation model cannot be included due to the limited length.

7) About Section 3.2 (Deep seismicity zones), in figure 2 the position of the letters L and H are over the same zone. I understand that the two zones are overlapping, but from the caption I assume that the letter H refers to the wider area with the pale color.

   Thank you for pointing out this inconsistency, which is definitely confusing for the reader. Zone L is indeed the deeper and smaller zone. We have corrected the problem in the new version of the manuscript.

8) At row 147, I suggest the use of the term “deeper” instead of “less”. Even if English is not my mother tongue, as written I understand the deep earthquakes occur at 20 or 30 km.

   The suggestion is well received. We have replaced “less” with “deeper” in the new version of the manuscript.

9) Section 4.2 (Occurrence rate model). The definition of seismicity rates is crucial in any seismic hazard model and object of many assumptions and operational choices by the modelers. In this field, it is normal to refer to analogue experiences. On the contrary, in this section there is only one reference about the Mmax estimation. I would like ask to the authors what is the approach adopted for the declustering; most used approaches (Gardner & Knopoff, 1974 or Reasemberg & Jones, 1985, among many others) lead to numbers of removed events very different.

We appreciate the reviewer's comment on the importance of declustering in probabilistic hazard analysis, which we also consider crucial. Although this topic was discussed in detail in section 2.7 of the companion article in the same special issue, focusing on the input datasets compiled for the analysis, we acknowledge that additional clarity is needed on the declustering approach used in our study.
In our analysis, we used well-established window-based declustering approaches, including those proposed by Gardner and Knopoff (1974), Uhrhammer (1986), and Grunthal (1985). These methods were chosen because of their suitability for the data set and their widespread use in similar studies. We have now included a clear reference to section 2.7 of the companion paper in the new version of the manuscript to provide readers with further insight into the employed declustering strategy.

10) The determination of b-value in two steps was adopted in many studies. Anyone to mention?

    Even if we call it two-stage, the strategy of setting a regional b-value for large zones in advance is quite common and has been used several times in research studies and industrial applications. This is usually necessary when the recorded seismicity is not sufficient to perform a more detailed evaluation.
    Some relevant published examples are from Vilanova and Fonseca, (2007), Ullah et al. (2015), Ghasemi et al. (2020), Ghione et al. (2021). The first author of this study has also applied the same methodology in different regions of the current GEM Global Earthquake Hazard Model, e.g. in the East African Rift (SSA, Poggi et al. 2017), in North Africa (NAF, Poggi et al. 2020) and in Russia/Mongolia (NEA, Pagani et al. 2020).

11) At row 184 it is reported a sentence that I have to dispute: “It should be additionally noted that the width of the non cumulative magnitude bins is not required to be uniform”. In my opinion, based on more than 30 years of expertise, It’s the first time that I read something like this. The bin width is a delicate point of the analysis, since it determines the b-value (Marzocchi et al., 2020; doi:10.1093/gji/ggz541). Even more so, the variable width is not acceptable. Let’s suppose that in the bin for magnitude 7 +- 0.5, all the events reported in the catalog have magnitude greater than 7: if you use 2 bins (with width 0.5) instead of 1 (with width 1), you will obtain 2 points with the same value in the cumulative curve, and this change the resulting fit. For me the assumption made by the authors it’s not acceptable.

    We are not sure that we understand the example given by the reviewer. Indeed, a linear fit using a least squares approach for a single data point cannot be reliably performed due to the imbalance between data points and parameters in the model.
    From a least squares perspective applied to incremental (non-cumulative) rates, each point that needs to be fitted represents the average frequency in a given magnitude interval. When minimising the squared error over the prediction, the adjustment is calculated for each bin for that specific interval, providing a mean of normalisation. In this way, multiple non-overlapping magnitude intervals can be fitted together, each with its own extension, without loss of generality. Of course, several bins (>2) are required to converge to an unbiased solution.

On the contrary, it is important to note that the choice of different bin lengths may affect the robustness of the associated occurrence rates, especially in regions with low seismicity. In general, we would recommend using non-uniform binning, where the intervals become progressively larger with increasing magnitude, e.g. following a logarithmic scheme, to ensure a comparable amount of calibration data for the calculation of rates.
Nonetheless, we recognise the reviewer's initial point, which is undoubtedly relevant when maximum likelihood approaches and cumulative distribution functions are considered.

12) Row 187: It's true that most of rates models start at magnitude 4.5, mainly for completeness reasons. In some cases, we know damaging earthquakes for magnitude 4 or less (as an example in volcanic areas with very shallow hypocenters) Could you quote any papers that affirms what you are saying?

    The reason for the introduction of a lowest truncation in the rate models lies indeed in the need to avoid unnecessary integration steps in the hazard integral. From an engineering perspective, severe damage has been occasionally reported from events with magnitudes less than 4, but for specific cases with high frequency accelerations associated with the effects of site conditions and on highly vulnerable buildings. In most standard cases, however, only light to moderate and non-structural damage is to be expected. When damage levels D4-D5 (severe damage up to collapse) are considered with average exposure and rock conditions, 4.0-4.5 is usually considered a reasonable choice that prevents calculations from being performed that do not directly affect the outcome (in term of impact). Furthermore, magnitudes <4.0 generally do not contribute significantly to the hazard controlling scenario for the most commonly used exceedance probabilities in engineering practise, such as 10% in 50 years.
    Relevant publications include Bommer and Crowley (2017), Azarbakht (2024), which have now been included in the new version of the manuscript.

13) I understand that Mmax is based only on the information reported in the catalog, i.e., the maximum observed magnitude. Why was the maximum geological magnitude not considered? One example is contained in Woessner et al., 2015 (doi:10.1007/s10518-015-9795-1). Or do you think that magnitudes larger than the observed events are not possible?

    We are of the opinion that events that are greater than those observed can certainly be expected. For this reason, when defining the Mmax of each zone in our model, we always take into account a conservative premium on the maximum observed magnitude (and also consider its uncertainty). In addition, we have considered the epistemic variability of Mmax in the log-tree of the source model.
    The direct use of geological constraints must be carefully considered. For studies focussing on specific known structures, we would agree to consider the maximum extent of the rupture. However, in the present case, the mapped seismogenic structures are generally not constrained in such detail. Individual fault lines could represent one or more complex systems and information on the actual segmentation is generally lacking, which could lead to a dramatic overestimation of the expected maximum magnitude. For example, when using the AFEAD dataset, many mapped faults may yield unphysical magnitudes if scaling

relationships are applied to their entire extent to convert the rupture area to a moment magnitude, as was done in SHARE's FSBG model.

14) Row 270: the smearing effect due to the adoption of seismic areas depends on the approach adopted to design the areas: smaller are the areas and more the hazard is concentrated on the epicentral areas. The design of areas should contain a sort of "prediction" for those zones with poor knowledge about the historical seismicity.

    We agree with the reviewer's comment, which also agrees well with our answer to question 3.

15) Figure 10: I wonder why the tectonic regions are different with respect to the groups of figure 1. As an example, source zone 5 in figure 1 has a different classification in figure 10 if I consider the other zones of group A. There is an explanation?

    Yes, the difference in grouping between the zonation of the source model and the tectonic regionalisation is due to the fact that they represent different aspects of the earthquake phenomenon. The source zone grouping was based on similarities in the process of earthquake generation at the source level, while the grouping for the tectonic regionalisation type (TRT) was based on the expected differences in the general attenuation behaviour along the path from the source to the site and aimed to differentiate the ground motion prediction equations (GMPEs). Although there are some similarities between the two classification schemes, they are not equivalent as they are based on different assumptions and constraints.
    We have clarified this aspect in the new version of the manuscript to provide a better understanding of the reasons for the different groupings in Figures 1 and 10.

16) Row 371: what are the considerations that allow you to say that in stable continental crust zone an intermediate behavior between active shallow crust and stable crust is expected? I don't say that it is not true, but I would like that you support this sentence with a reference or your comment.

    The assertion regarding the expected intermediate behaviour in stable continental crustal zones was based on operational considerations of the authors. We came to the conclusion that a purely cratonic attenuation behaviour is unlikely to be expected for regions such as the Kazakh Shield. Assuming that tectonic regionalisation type 2 (TRT2) exhibits less extreme attenuation behaviour compared to standard regions with active shallow crust, it can therefore be inferred that buffer tectonic regions surrounding large active structural systems such as the Tianshan Massif and Turkmenistan would behave in an intermediate manner. This operational choice is partially supported by the associated seismicity patterns, as shown in Figure 4 of the accompanying paper, but can only be verified if sufficient ground motion records are available for comparison with the selected ground motion models.

17) Section 8. I don't find any description in the manuscript about the 3 options for the assignation of b-value (b, b+0.5, b-0.5). With regard to Mmax, on contrary, in the manuscript I found only a sentence about the branch with Mmax+0.1. I think that the

whole logic tree has to be described together with the strategies adopted for assigning weights.

We take note of the reviewer's suggestion. In the new version of the manuscript, we have included additional explanations of the strategies for implementing the logic tree.

18) Row 415: this is a clarification. The results of the calculation are only the hazard curves (not only in OpenQuake engine). Maps and UHS are possible representations!

    We fully agree with the reviewer's clarification. While the hazard curves are indeed the primary results of the calculation, it is important to point out that maps and Uniform Hazard Spectra (UHS) are derived products obtained from the hazard curves representing certain exceedance probabilities and observation times.
    In the manuscript, we only wanted to emphasise the output product that we made readily available to the reader in OpenQuake format. These data can indeed be downloaded from the World Bank's data portal and will also be included in the journal's repository.

19) Section 10. I don't understand why the presentation of the model expressed in terms of macroseismic intensity is in this section and it's not in ad hoc section.

    This decision results from the editorial decision to summarise the information from the original project report in the form of an article. In the original report, a separate section was indeed devoted to the discussion of macroseismic intensity, as expected by the reviewer. However, in order to reduce the overall length of the manuscript, we decided to integrate this alternative presentation directly into the discussion section. This approach allowed us to integrate the comparison with existing models in a natural way and to facilitate the corresponding discussion.

20) Row 434: "Comparison with previous PSHA studies shows general agreement". I don't see the comparison! In the Supplement it is reported the map of GSHAP, released 25 years ago. Probably this is not the best test… In my opinion the comparison has to be performed with EMCA project (most recent study for the same area) or with recent national projects. Not only: I expect a quantitative comparison, not only a comparison of two figures.

    Dear Reviewer, we have reproduced the maps of GSHAP (for PGA) and EMCA (MSK intensity) in Section 3 of the Appendix (see Supplementary Material) in Figure S4 and S5, respectively. The two maps were created using the same colour scale, sample, metrics and extension of the hazard maps from the present study to facilitate quantitative comparison for the reader.
    We intentionally did not include a map of differences as this would have overemphasised the significance of the discrepancies. It is important to clarify that our aim in comparing these models was not to assert the superiority of one over the other. Instead, we have sought to understand the differences and similarities between them, recognising that all models, including ours, have their own limitations. As George E. P. Box famously said, "All models are wrong, but some are useful." Our comparison therefore aimed to emphasise the usefulness of each model in different contexts, rather than ranking them.

As for the GSHAP, we recognise its age and the limitations associated with using this model as a benchmark. However, it is worth noting that GSHAP is still considered in many technical studies. During project development, we were repeatedly asked by partners and reviewers to perform such a comparison, which is why we included it.

21) Regarding the intensity maps, at row 464 you write: "All intensity maps are consistent with a shear wave reference velocity of 800m/s". This is a strong statement and I ask you to cite a paper or discuss it. Most localities are built near rivers for access to water; this means soil conditions other than rocky ones.

    The use of a shear wave reference velocity of 800 m/s is a common practise in seismic hazard assessment and is often used as a standard for comparison purposes. It provides a standardised basis for the evaluation of seismic hazard in different regions. Furthermore, this choice is in line with the guidelines that recommend 800 m/s as the reference shear wave velocity for seismic hazard assessment in engineering applications. Thus, the use of a standard reference rock is a common abstraction and does not necessarily reflect actual site-specific conditions. While site-specific models could provide more accurate assessments, their applicability may be limited if they are based on the assumptions of regional models, especially in regions where soil conditions vary widely, such as near rivers.
    Nevertheless, in our study we also calculated a site-specific model, which is discussed and provided in another article by Salgado et al. in the same special issue (presently under review). This model provides a more detailed assessment of seismic risk for the Central Asian countries and takes into account site-specific soil conditions and local geological features.
    Overall, while we recognise the importance of considering site-specific conditions, the use of a standard reference rock in our intensity maps allows for consistency and comparability with existing studies and provides a basis for further analysis and interpretation.

22) In the conclusions, again, very few reference, but paper by Poggi et al.. When you talk of the strategies for assess seismic hazard at national scale, for example, you could quote Gerstenberger et al., 2020 (doi:10.1029/2019RG000653). For the international project, also, the references for CCRIF and ARC projects are missing.

    We appreciate the reviewer's suggestion to include additional references in the conclusions section. We believe that these additional references will enrich the discussion and provide readers with further resources to critically analyse the results of this comprehensive project.