

A Probabilistic Seismic Hazard Model for North Africa

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INTRODUCTION TO REVIEW REPLY

We would first like to thank the two reviewers and the editor for their time in reviewing this manuscript and for the appreciation of our work. We found their comments very useful and fair; these gave us the chance to improve the manuscript, clarify some aspects of our research and trigger new ideas for future developments. As requested, a minor revision of the manuscript has been undertaken. All the proposed suggestions have been carefully considered and, where necessary, applied. Controversial issues have been explicitly addressed with a (hopefully) comprehensive and detailed reply, highlighting our thoughts and motivations.

All minor comments not directly addressed in this reply, are accepted and implemented by the authors without further discussion. Identified mistakes have been fixed. Please note that all corrections are marked in red on the manuscript, to facilitate the reviewers in the referencing.

Legend:

R1 – Original comments from Reviewer #1

R2 – Original comments from Reviewer #2

AR – Author's reply to comments

Reply to Reviewer #1 Comments

R1: The paper represents is an important part of the global seismic hazard model developed by the GEM Foundation. The paper is generally well written (apart from some 40 missing references). I have suggested some improvements to the grammar and style in the attached annotated manuscript.

AR: We express our gratitude to the anonymous reviewer for the appreciation of our work. Suggested corrections have been applied to the manuscript. We apologize for the missing references, which is indeed a remarkable mistake. Possibly, some information went lost when assembling the contributions from co-authors. All missing references have been added and carefully checked.

1. Active fault database

R1: This part of the study is particularly contentious. I am aware that some North African scientists are aggrieved that they were not consulted by GEM and are of the opinion that the authors of the NAF model have little knowledge of the region. I am also aware that GEM tried to establish a relationship with scientists from the region, but this broke down. Thus I think that it is important that the authors indicate the degree to which their active fault database is based on field studies, especially those conducted by local scientists.

AR: We have added a paragraph to the introduction to section 5 describing our reliance on published studies of faults, particularly by local experts:

“Of the ~125 active structures, about half were based directly on published work, typically field studies by local scientists. Fault characterization based on topographic, geophysical and geodetic data was only performed when no studies for that structure were available. The majority of faults that are not based on published studies are along strike or otherwise close by the faults that have received direct investigation, and clearly share some characteristics of the studied faults. The reference information for each fault is given in the fault database.”

(Please note that we removed 10 faults from the fault database during the model construction because we were unable to estimate slip rates on them, so there are 125 faults in the seismic source model rather than 135, although we forgot to propagate this change to the originally submitted manuscript.)

We have also added a paragraph that describes that we wanted collaborations but were unable to secure them:

“We acknowledge that involvement of local scientists in the creation and maintenance of the fault database is ideal; unfortunately, we were not able to establish these collaborations within the time constraints of this project. However, we stress that the NAF seismic hazard model presented here is simply the first version, and that local scientists who wish to be involved in future iterations are strongly encouraged to contact the GEM Secretariat (hazard@globalquakemodel.org).”

Because we work globally and often with short time windows for each project, it can be very difficult to identify local scientists from each region on Earth who are both willing and able to work with

GEM. In the case of North African faults, we reached out to 10-15 individuals (both North African scientists and European/American scientists with decades of work in the area), most of whom simply didn't respond, and the others stopped responding after some time. [IT SEEMS A BIT PROVOCATIVE, DO WE WANT TO INCLUDE THIS?]

2. Magnitude occurrence relations and Mmax

R1: I would like greater clarity on definitions and procedures.

(a) Explain the relationship between 'minimum magnitude' and 'magnitude of completeness'. Is 4.5 greater than the magnitude of completeness for all zones?

AR: Our manuscript was definitely unclear on the matter, as a similar issue was raised also by the second reviewer. In Section 6.1.2 we now better describe the difference between the minimum magnitude from completeness analysis (thus variable between zones), used for the calibration of the GR, and the minimum magnitude threshold, used for hazard calculation.

R1: (b) The text states '... the maximum magnitude ... is generally derived as the size of the largest observed event plus 0.5 magnitude units. However, Figure 4 seems to deviate from the Mobs+0.5 rule in half the cases, for example Groups 3, 4, 7 and 9. Please explain.

AR: We apologize for the lack of clarity. Actually, the GR of the different Groups in figure 4 have been calculated using the largest M^{\max} ($= M^{\text{obs}} + 0.5$) of the zones within each specific group. Values are therefore consistent with those in Table 7. We have better described this information in section 6.1.2 and in the caption of table 7 and figure 4.

R1: (c) I recommend that the number of events exceeding M4.5 and the maximum observed magnitude in each source zone and group of source zones is listed in Table 7 and included in the Legend in Figure 4.

AR: On this only point, we do not agree with the reviewer suggestion. In our perspective, providing just the number of events larger than M4.5 without completeness information would be misleading. Also, we would prefer to not include this information in the paper for the sake of conciseness. It must be noted, however, that the interested reader can obtain the whole model calibration information (catalogue, completeness table, etc.) directly from the GEM foundation (reference in Data and Resource section), who is currently preparing a public repository where model build-up will be openly available.

Similarly, the indication of M^{obs} might be here redundant, as it would simply be the M^{\max} (already shown) minus 0.5 units. Therefore, we would prefer to avoid including this.

R1: (d) Provide more information on the selection of the sizes of the magnitude bins. For example, is there a minimum number of events in a bin? How is the magnitude of non-cumulative number of events in each bin calculated? For example, the mean of the magnitudes. This is important when fitting the curves and determining the rates.

AR: To better describe the procedure we used to calibrate the GR, we added the following description in section 6.1.2:

“The bin size is defined while performing completeness analysis and is progressively adjusted after several iterations to provide an optimal solution of the GR. In general, the size is set to be increasing with magnitude (e.g. 0.25 magnitude units from 4.5 to 5.25, then 0.5 above 5.5), but in case of too few events (e.g. group 5), grouping might be necessary. The process requires nonetheless a level of personal interpretation based on expert judgement.”

As is visible from figure 4 (white squares) the reference magnitude of the non-cumulative (incremental) bins is defined as the mid-point of the bin. We have thus clarified this information in the caption of Figure 4.

REFERENCES

R1: There are many inconsistencies in the reference list. The authors must adhere to the prescribed style. About 40 articles cited in the text that do not appear in the reference list. For example:

AR: We apologize for the mistake. Missing references have been included.

TABLES

R1: Table 5: Define the magnitude types in the caption e.g. MS, Ms, MSZ, mb, mb1, MbLg, mbLg mL, ML, MI, MD, md

AR: Magnitude types are defined from ISC using the IASPEI recommendations. This is now described in the caption of Table 5, with direct reference to the IASPEI (2013) Working group.

R1: Table 7: List the number of events exceeding M4.5 and the maximum observed magnitude in each source zone and for each group of source zones.

AR: Please refer to our previous reply to Tale 7 and Figure 4, where we clarify our point of view on the matter.

R1: Table 8: The probabilities for Group 2 add to 0.8, not 1.

AR: This was a typo. The first probability of group 2 has been corrected to 0.4, as it is properly implemented in the source model.

Reply to Reviewer #2 Comments

R2: The paper describes the development of a hazard model covering North Africa as part of the worldwide mosaic of models being developed by the GEM Secretariat.

The paper is well written, pleasant to read, concise and with high quality illustration. The only remark on the form would be the absence of line numbering for review.

To my opinion, there are a couple of details and sensitivity tests missing which would help to clarify a number of hypothesis and choices made by the authors.

Remarks and comments in the order of appearance in the manuscript are given below.

AR: The reviewer raised a number of important issues that are certainly relevant to improve the clarity and transparency of the present work to the scientific community. We are grateful for that. It is our hope to have exhaustively addressed all the relevant issues in this reply and within this new revision of the manuscript.

Section 2 Seismotectonic settings

R2: A seismotectonic map of the region with the names of the main structures and regions would help to follow the description of the context given in the whole section.

AR: Although we understand the rationale of the reviewer, it is our opinion that a seismotectonic map would definitely overload the article, already quite dense of information. We believe that a number of appropriate literature references were provided to facilitate the reader in identifying the major structures, if necessary.

We have nonetheless added a second panel to figure 2, according to reviewer suggestion below, to also show the modelled tectonic features of the North-Eastern part of Africa.

Section 2.1.1 Morocco

R2: Third paragraph, second line: “Aboran Sea” should read “Alboran Sea”

AR: Corrected

Section 4 Catalogue

R2: While the introduction states that the catalogue has been assembled from globally and locally available sources, the actual sources are mainly global and regional (EMEC). The only local source is the IGN catalogue from Spain. There are a couple of relatively recent publications related to earthquake catalogues for Algeria (Harbi, A., Peresan, A. & Panza, G.F. *Nat Hazards* (2010) 54: 725. <https://doi.org/10.1007/s11069-009-9497-6>; M. Hamdache, José A. Peláez, A. Talbi, C. López Casado; A Unified Catalog of Main Earthquakes for Northern Algeria from A.D. 856 to 2008. *Seismological Research Letters* ; 81 (5): 732–739. doi: <https://doi.org/10.1785/gssrl.81.5.732>). Have these kinds of local sources been considered?

AR: Yes, we have considered these studies in our preliminary investigation, as well as other minor local studies for Algeria, Tunisia and Egypt, which have been discarded for various reasons (e.g. licensing, unavailability or unusability of the published results, redundancy or concurrency of the input information to other considered sources, extension of the investigated area etc.)

Section 4.2 Hypocentral location selection

R2: End of first paragraph Table 2 is cited while Table 1 comes in the next paragraph. Usage is usually to number in order of appearance.

AR: Corrected. The reference to Table 1 was indeed missing from Section 4.1.

Section 4.3 Duplicates

R2: Second paragraph: To address the trade-off between identification of duplicate events and the fraction of independent events erroneously identified as duplicates, the authors state that window size (space and time) is manually adjusted to obtain the best trade-off. What is the method actually used to select the “best” window? Is it based on manual review of the identified duplicates? Are there any measures of the trade-off eventually based on known aftershock sequences? In the following the authors state the erroneously identified duplicates are mostly aftershocks which would in any case be removed by the declustering, however, their removal at the duplicate identification stage will impact the declustering. This is more a comment than a remark.

Regarding the size of the windows for the different periods: for historical seismicity, are all the events known by month and day in the catalogue? Since the time difference is set to 2 days, they have to be. For the instrumental period, 120 second time difference seems large. Origin time is relatively well constrained, much more than location for instrumental events. More details on the selection of the numbers presented in Table 3 would be welcome.

AR: Unfortunately, we did not have a more objective and/or automated procedure to calibrate the window size, and therefore a progressive adjustment was manually performed while inspecting the distribution (spatial and temporal) of the identified duplicated and the removed events. Certainly, as suggested by the reviewer, a more robust approach based on the analysis of previous well-known aftershock sequences would be advisable. This is, however, more a topic of a separate and focused study. For the purpose of our analysis, we believe that rather satisfactory results were obtained.

Regarding the matching of the historical events, month and day information is available for the large majority of the events. Nonetheless, for the remaining uncertain events we performed a manual review, artificially assigning the arbitrary date of the 1st of January (only for event matching purposes). This is now clarified in the manuscript.

Section 5.1 Hig and Middle Atlas

R2: End of second paragraph: “Meghraoui et al. 1999” should read “Meghraoui et al. 1999”

AR: Corrected.

Section 5.5 North Eastern-Africa

R2: Figure 2 does not cover this region. The discussed structures and regions might be identified in a regional seismotectonic map for the whole region as suggested above.

AR: We added a second panel including the active faults for the North-Eastern Africa region.

Section 6.1.2 Occurrence Model and Maximum Magnitude

R2: First line “double truncated” should read “doubly truncated”?

AR: That is interesting. We found both denomination in literature, as it seems they are equally valid. We have nonetheless modified “double truncated” in “double-truncated”, uniformly.

R2: “Minimum magnitude has been arbitrary assigned to 4.5 for all zones”. Minimum magnitude has a strong impact usually on the computed activity rates. Also, the catalogue has been developed from $M=4.0$, instinctively one would compute activity rates with as much data as possible starting from $M=4$. I suggest explaining a little bit the rationale to select $M=4.5$. Sensitivity tests might also be performed which could be used to justify or discuss the uncertainty on the b -value included in the logic-tree (as explained in section 9).

AR: The decision of using a calculation threshold of 4.5 is a rather standard engineering choice, as it is generally accepted that events below such threshold are not capable to produce a “significant” level of damage. For the development of a previous model for the East African Rift (Poggi et al. 2017), we have preliminarily tested a lower threshold of 4.0, but the results were not particularly different for the considered return periods (although we expect some impact on shorter return periods, which are nonetheless not considered).

It must be noted that catalogue is compiled from a magnitude lower than the minimum magnitude used for the calculation in order to:

- a) Better constrain the GR relation (if local completeness allows to)
- b) Account for the uncertainty when selecting and converting between magnitude pairs during homogenisation

R2: Second paragraph: The authors mention the completeness analysis. Please provide details on the analysis: method used, unique completeness for the whole region or local specificities (based on the discussion on the catalogue, it seems that completeness may vary across the region under study).

AR: The variability of the completeness between zones is now better indicated at the beginning of section 6.1.2, also stressing the relation with minimum threshold magnitude (as also requested by the first reviewer). For details about completeness analysis we now refer in the same paragraph to Poggi et al. 2017, where the procedure is more exhaustively presented. We decided to not include this information in the paper for the sake of conciseness. It must be noted, however, that the interested reader can obtain the whole model calibration information (catalogue, completeness table, etc.) directly from the GEM foundation, who is currently preparing a public repository where model build-up will be openly available. Reference is added to the “Data and Resource” section.

R2: End of second paragraph: “with rather short of incomplete earthquake records” change “of” by “or”.

AR: Corrected

Section 6.2: Spatial Variability of Earthquake occurrence

R1: Last paragraph: the smoothing approach is usually used as alternative to the area source with spatially constant activity within each source. The application of smoothing within area sources is interesting, however, I would have expected one branch of the logic-tree populated with the traditional area source approach. Maybe the case with the large decay parameter is close to the “classical” approach?

AR: This is correctly described in the sentence “A theoretical infinite value of λ would produce a homogenous area source”. A value of 100 (upper bound of model uncertainty) produces a smoothed model that is nearly a homogenous area source model. Therefore, using an additional branch for

area sources would have represented a sort of “duplication” of the smoothed model with large decay factor, de facto introducing an over-weighting of this interpretation.

Section 7

R1: Sixth paragraph (end of page 20): “The maximum magnitude of the MFD is derived by applying the Leonard (2014) scaling relations”. Can the author justify the use of these specific relations amongst the existing ones?

AR: The use of Leonard (2014) seems to be more appropriate than other scaling relations we tested, as its constraints help avoiding the development of rather unrealistic (elongated) fault geometries at large depth.

R2: Sixth paragraph (end of page 20): “with the additional constraint of not exceeding the maximum magnitude realistically expected for the source group”. How is the maximum magnitude realistically expected computed?

AR: As described in section 6.1.2, the expected maximum magnitude for the source group is assumed to be the maximum observed plus a conservative 0.5 magnitude units. We simply imposed the model to not exceed this value. We agree that the term “realistically” might be misleading, as these are just modelling assumption, and therefore we remove it from the sentence.

R2: Last paragraph: The construction of the background model is not very clear.

AR: The procedure we used for the construction of the background model is now described more in detail at the end of section 7.

Section 8 Ground-Motion

R1: Third paragraph: Group C for the ground-motion model has been introduced as a transition zone between active and stable parts of the region to avoid abrupt variations of ground-motion. Looking at the maps, it seems that there are transition zones in northern Libya and Northern Egypt. However, there is no transition in the North-South direction between northern Morocco and the central-southern part. Also, it seems that the area source groups shown on Figure 3 have not been defined taking into account, at least partly, the tectonic classification shown in Figure 9. Can the authors elaborate on this?

AR: The inconsistency identified by the reviewer is due to a mistake in Table 10, where the characteristics of zone B (AS+SC) and C (SC) have been accidentally swapped. The table is now fixed in agreement with the implemented the source model. Therefore, the zonation is now logically compatible with the proposed classification methodology. We apologize for the mistake.

To answer the reviewer question, the original zonation presented in figure 3 was actually done taking in consideration also the results of Chen (at least, to some extent), although the subsequent source grouping was performed with a different rationale (and for a different purpose) than that discussed in Section 8 for the definition of ground motion models.

R2: Last paragraph: there is no argumentation for the selection of the 4 GMPEs used for calculation amongst all the GMPEs available. The argumentation is also not provided in Poggi et al. (2017). As

stated in the discussion, the GMPE selection has a large impact on the hazard results. Some more arguments to justify the ground-motion model is needed.

AR: As described in the second paragraph of the Section 8, given the unavailability of earthquake records for the calibration, we had to rely on indirect selection criteria, as those suggested in Cotton et al. 2006. In practice, from the GMPEs available in the hazardlib library, we have first identified those models ideally more compatible with the target region, and then ranked the selected GMPEs in relation to their overall performance and robustness (based on literature review). An additional clarification has been added to end of the section.

Section 9 Source Model Uncertainties

R2: Third paragraph: The relative errors on the maximum magnitude and b-values are given without rationale. Please justify these choices (there is a link with the previous comment regarding the choice of the minimum magnitude used to compute the seismic activity parameter in Section 6.1.2). Regarding the fault model, no uncertainty on either the slip rate, b-value (which is based on the computed area source b-values) or maximum magnitude are included. Please justify.

AR: Additional considerations regarding the uncertainty on Mmax and b-value have been added to the second last paragraph of section 9.

Regarding the fault model uncertainty, we were initially considering to include it, but we had to face some technical limitations. So far, the only possibility to do that for faults is to manually create a separate (thus different) xml source model for each logic-tree end-branch. Unfortunately, this would have led to a source model complexity too difficult to handle. For that reason, we limited the epistemic variability of the model to what proposed. Nonetheless, we are confident that in future implementations, the OQ engine will allow a more appropriate handling of the fault uncertainty (particularly when slip rates are used). These considerations are now summarized at the end of the second last paragraph.

Section 10.1 Calculation settings

R2: First paragraph: Please indicate the Vs30 used for calculation (760 or 800 m/s).

AR: Changed to 800m/s

Section 10.2.1

R2: Second paragraph: Looking at the hazard curve, in addition to the particular shape observed at PGA for Rabat highlighted by the authors, the hazard curve at PGA for Cairo also shows a particular shape, the decrease of probabilities with increasing acceleration is not as fast as observed for Algiers and Tunis (in those case, the PGA curve crosses the SA(0.5) curve which is not observed for Cairo). Do the authors have an explanation for this? There seem to be no fault close to Cairo in the model. Is this an effect of the Grong-Motion model?

AR: We don't have a definitive explanation for that. The ground motion model is likely not the cause, as both Tunisi, Algiers and Rabat are falling in the Active Shallow Crust domain, using therefore the same combination of GMPE. In our opinion, this might be due to differences in the b-values across the region, which makes large magnitudes impacting differently the longer return periods.