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Response to the respected reviewer #2

Dear professor Telesca

From the outset we would like to convey our appreciation for the thorough, critical and fair review of our manuscript. You raise several important points and we believe that we can address all of them in a satisfactory manner. Moreover, we can identify that in doing so that our manuscript will have considerably been improved, which we appreciate also greatly.

Here is a point-by-point response to your comments and concerns.

Reviewer' Comments to the Authors:
The paper proposes to change the classical frequency-magnitude distribution (the GR scaling law) in the PSHA with the non-extensive frequency-magnitude distribution derived from the SCP model. The aims of the paper fall within the scope of the journal. The study is clearly written and structured. However, some points need to be clarified.
Authors' reply: Thank you for reaching out and providing us with valuable feedback. We found your comments extremely helpful and have revised accordingly.

1. Page 5, the authors say "In this equation, unlike the non-extensive expression of Telesca (Telesca, 2012) in which the catalog completeness magnitude is used, we include the minimum earthquake magnitude of engineering significance". How the "minimum earthquake magnitude of engineering significance" is defined? If the completeness magnitude is not a fundamental parameter, why the authors at page 7 say "In order to have a reliable estimate of the seismicity parameters, a homogeneous and "complete" earthquake catalog is required."

Authors' reply: The completeness magnitude is a key factor in estimating the seismicity parameters. In this statement, we do not mean that this parameter is less important. This merely corresponds to the fact that in the evaluation of PSHA integral (not in seismicity analysis), mainly the minimum earthquake magnitude of engineering significance is used. This parameter is defined as "the smallest magnitude of earthquake that is capable of generating potentially damaging levels of ground shaking" (Bommer and Crowly, 2017). However, we have modified our statement in this section to avoid misleading information.

Bommer, J. J., & Crowley, H. (2017). The purpose and definition of the minimum magnitude limit in PSHA calculations. *Seismological Research Letters*, *88*(4), 1097-1106.

2. The authors just say that the GR parameters were calculated by using the SEISRISK II software. However, Fig. 2 shows that the GR law does not fit at all the ECDF, which can be easily fitted by a straight line whose slope gives the estimate of the b-value that should be smaller than that indicated in Table 1.

Authors' reply: We thank you for pointing out this problem. You are absolutely right. This is because we have mistakenly reported the α and β values (i.e., $\alpha=a_{GR}\times\ln(10)$ and $\beta=b\text{-}value\times ln(10))$ instead of a_{GR} and *b-value* in this figure (and also in the Table 1). This mistake has led to the incorrect drawing of the GR curve. Accordingly, this figure was modified.

3. The epicentral distribution of the earthquakes needs to be shown.

Authors' reply: Based on your comment, the epicentral distribution of the earthquakes has been added to figure 1.

4. The declustering is performed by using the Gardner and Knopoff method with Uhrhammer window. It is known that this method can also be used with the Grunthal window (van Stiphout et al., 2012, doi:10.5078/corssa-52382934. http://www.corssa.org). Why did the authors use Uhrhammer?

Authors' reply: In this example, we aim to illustrate the difference between PSHA and NEPSHA results. The use of Uhrhammer window here for declustering does not mean that it is superior to other methods. We used this window because it is a known method and has been used in many seismicity studies.

5. Recently, Mizrahi et al. (Seismol. Res. Lett. 92, 2333–2342, 2021 doi: 10.1785/0220200231) concluded that "declustering should be considered as a potential source of bias in seismicity and hazard studies", since the GR parameters depend on the method of declustering. Thus, I think, the paper would be improved if the authors discuss and compare the results obtained after applying also another method of declustering besides that cited in the paper.

Authors' reply: Thank you for your comment and suggestion. Generally, the main purpose in this work is to develop an efficient scheme to PSHA based on the fragment-asperity (SCP) model instead of the Gutenberg-Richter (GR) scaling law. In this paper, the computational framework of the proposed NEPSHA method is presented. We provide here a simple example only to describe and evaluate the proposed framework. Obviously, in practical applications, there are some issues that can be investigated and evaluated (e.g., effect of ground motion prediction equation selection on PSHA results, effect of the catalog selection on seismicity parameters, and sensitivity of PSHA results to the declustering methods). In this context, the valuable results provided by Mizrahi et al. can be discussed and investigated. But we believe that detailed dealing with these issues can overshadow the main purpose. Undoubtedly, the detailed examination of these cases can be a research topic itself. But we are worried that addressing them in this manuscript will mislead the readers. Finally, if the honorable referee considers it necessary, we are ready to add it to the present work.