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Comment on nhess-2022-214

Luciano Telesca (Referee)

Referee comment on "A non-extensive approach to probabilistic seismic hazard analysis"
by Sasan Motaghed et al., Nat. Hazards Earth Syst. Sci. Discuss.,
<https://doi.org/10.5194/nhess-2022-214-RC2>, 2022

First of all, by mistake I posted my review as CC rather than as RC, so I re-write my
comments below.

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.

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."

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.

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

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?

I would add further comments:

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.