

REBUTTAL LETTER

We thank the reviewers for their comments, which helped clarify our study. We detail below the changes made to address their comments.

Reviewer #2

The article focuses on the Upper Rhine Graben's seismogenic potentials. Significance of the article is to improve the modeling of the seismogenic zones by including various parameters related to the fault modeling and seismicity of the region. In this regards, the article helps on constructing a better seismic hazard model for the region.

The paper has a good quality but there are some aspects of the paper that requires improvements. They are given in below.

Major comments:

(1) Reviewer #2

- It would be better to provide a more expanded information about the truncated and tapered models. Maybe it is a very well known and used methods in the subject but it would help reader to understand the basic consent without reading another paper(s).

Michel et al.

We now include in the supplement a figure illustrating the two models (Figure R4 / S1).

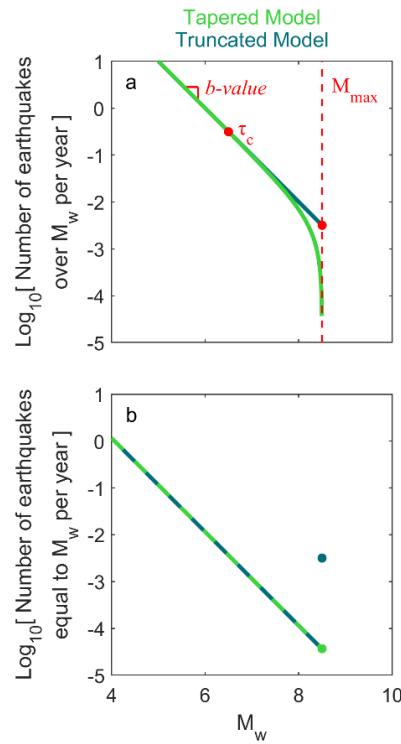


Figure R4: Illustration of the tapered and the truncated models in their (a) cumulative form and (b) non-cumulative form. M_{max} , the recurrence time of events of a certain magnitude (τ_c), and the b-value indicate the three parameters that control those models.

(2) Reviewer #2

- Magnitude estimations are given in the second decimals which requires a great knowledge of the almost all the parameters in the study. However, there are lot's of uncertainties in both data (eg. lack of knowledge about seismogenic depth) and method (eg. using a m_0 and A_{eq} scaling law which has its own standard deviations). In this regard, providing earthquake magnitudes in second decimals as if there is such a good resolution in the study is not realistic.

Michel et al.

We agree and rounded them.

(3) Reviewer #2

- In the thermal gradient part of the Section 3.1 the thermal features are expected to increase linearly with depth. Is there any reference to justify this linear gradient in depth? Neither Freymark et al. (2017) nor Guillou-Frottier et al. (2013) has any model for the depths that are deeper than the drilling depths. Even though Freymark et al. (2017) had a model for density variations, in thermal data the study limits itself except for the Fig. 10F which does not provide a detailed model for the region.

Michel et al.

Since we do not have enough data to constrain this point, we assume a homogeneous medium and steady-state conditions, resulting in a linear increase of temperatures with depth. Additionally, to first order, measurements of temperature down to 2 km-depth in the URG (Guillou-Frottier et al., 2013) seems to indicate a linear thermal gradient (Figure S2). We just extrapolated. We now state in Section 3.1 that we assume such linear gradient.

(4) Reviewer #2

- In the Section 3.1 seismogenic thickness of the southern par of the URG is defined by using temperature and salt basin assumptions. However, in the paper it is stated that in the southern URG there is seismicity. Can the authors add seismicity for the thickness modeling?

Michel et al.

The depth uncertainties in our seismicity catalog are unfortunately very large, if not undefined, and we prefer not to use them to infer the depth of the seismogenic zone. The variable quality of the depth determination is due to both the sparsity of the network at the beginning of the

instrumental period, the lack of stations (records) close to the event epicenters, and to the proximity to the German border, which sometimes requires the mixing of different national seismological networks to reduce the epicentral gaps. We have checked the ISC-EHB catalogue containing well-constrained hypocentral depths (level L1), or the IASPEI Ground Truth GT5 events, but the number of earthquakes in these catalogues is unfortunately too small for our region of interest. Furthermore, we believe that the depth of small-magnitude events, even if well recorded, may be not representative of the depth of larger events.

(5) Reviewer #2

- In Section 3.1 and Section 3.3, relation between the section and their effect on the model parameters are explained by referring the variables in sections. It would be good to provide same information for Section 3.2.

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We now changed the beginning of Section 3.2 to:

“To constrain the MFD of the long-term seismicity models with an observational seismicity catalog, as described in Section 2.3, we need to evaluate from the observational catalog the number of events per magnitude bin $n_{obs}^{M_i}$ over a period of time $t_{obs}^{M_i}$ (Section 2.3). We use the earthquake catalog from Drouet et al. (2020). This catalog was built from multiple former catalogs. ...”

(6) Reviewer #2

- In line 267-268 what is the reason for choosing second peak in truncated model instead of the first one?

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It's the largest peak, thus the mode of the PDF. But indeed we could have done it also for the second peak. Nevertheless, we prefer not to do it as the reading would become cumbersome.

(7) Reviewer #2

- In line 149, $P_{barries}$ is not defined.

Michel et al.

Thank you for pointing this out. It's a typo and we removed P_{barrier} from the equation.

(8) Reviewer #2

- To sum up the study, a Table can be added into the conclusion part which shows the changing results for changing parameters. They are more or less presented in the text of conclusion. A table show the overall results which helps readers to summarize the study.

Michel et al.

We now added such table in the conclusion (Table 2).

(9) Reviewer #2

- As mentioned in Introduction and Figure 5, results of this study can be useful for nuclear power plant constructions. However, in the paper there is no indication of which model is better and should be preferred on hazard assessments. In a scientific point of view, all the variations in results are important but for the engineering perspective there must be a decision to make to choose a number to move forward. In the conclusion there is no such indications.

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We added the following sentence in the conclusion:

"In contrast, when strike-slip kinematics are considered as described in Section 5.3 and the Black Forest Fault is taken into account, there is a 99% probability that M_{max} is less than 7.6 and 7.5 for the tapered and truncated models, respectively. This is our preferred scenario as it is based on recent findings for strike-slip mechanisms, although the assumptions made in this analysis are debatable (i.e. strike-slip/dip-slip ratio evaluated on a fault just north of our zone of study and applied to all faults; Section 5.3). "

(10) Reviewer #2

Minor details:

- Line 58 – "w" of M should be subscript to be in agreement with the other M_{ws} .

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We changed the subscripts accordingly.

(11) Reviewer #2

- Line 128 – No need to say “according to global earthquake statistics”.

Michel et al.

We prefer to keep it.

(12) Reviewer #2

- Line 136 – No need to say “finally”.

Michel et al.

Done.

(13) Reviewer #2

- Line 138 – "since" is a conjunction, so it should always join two clauses.

Michel et al.

Thank you for pointing this typo out. We modified the sentence accordingly:

“Since we consider only mainshocks, we define the likelihood of the observed seismicity catalog ...”

(14) Reviewer #2

- Line 170 – “aforementioned” can be moved before the “other”. “... by the aforementioned other faults”.

Michel et al.

Done.

(15) Reviewer #2

- Line 231 – What does percentage of the mean and variance of the scaling factor in numbers?

Michel et al.

We apologize for the confusion, we changed the sentence to:

“... we assume the PDF of α_s is a Gaussian distribution with $\mathcal{N}(0.9, 0.25)$...”

(16) Reviewer #2

- Lines 275-278 – Word “thus” used in 3 consecutive sentences. Different wording can be chosen to text more readable.

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We changed to three sentences to:

“In this respect, we also provide results if events are selected within 20 km of the faults (Figures S16 and S17). Under these conditions, the seismicity rates of the observational earthquake catalogs are higher and constrain the long-term seismicity models to cases that produce higher moment release rate

(17) Reviewer #2

- Please provide the DOI for all the references.

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Done.

(18) Reviewer #2

- In Figures 4, 7-8 it would be better to put a y-axis for $P_{m\max}$ and τ .

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See Answer to comment **(5) Reviewer #1**.