Response to the comments of Reviewer 3

The authors would like to thank the reviewers for their constructive and positive reviews of our manuscript. We would like to highlight several essential points that all three reviewers commonly ask:

First, our paper is a manuscript that proposes a new approach integrating the multivariate Bernoulli earthquake occurrence model and stochastic source modelling. As such, a natural scope/application of the new method is the time-dependent Probabilistic Tsunami Hazard Analysis (PTHA). We do not take a strong position as to which time-independent and time-dependent models are appropriate. However, we have noticed that the wordings in our original manuscript in supporting the time-dependent model for PTHA were relatively strong, and hence, once we are invited to revise the manuscript, we will soften our statements and clarify the motivation of the paper.

Second, the justification for the choice of the time-dependent PTHA model was not well presented in the submitted manuscript. We have prepared additional texts both in the introduction and the results and discussion sections (see details comments below) to highlight some more recent studies (i.e. Williams et al., 2019; Griffin et al., 2020; Moernaut 2020). Those studies suggest that the global paleoearthquake records provided empirical support for weakly quasiperiodic earthquake recurrence. Therefore, it can be used to justify the use of renewal models (i.e. Brownian Passage Time (BPT) model) for seismic/tsunami hazard assessment.

Third, this manuscript presents a new alternative to carrying out a time-dependent PTHA as a kind of sensitivity analysis using Indonesia (Sunda) applications. As such, we should recognize the limitations and challenges to be resolved. In our submitted manuscript, we did not provide the limitations of our study highlighted by the three reviewers. These include the 1D approach in earthquake rupture modelling, BPT parameter estimation using the Bayesian approach, and space-time earthquake rupture modelling. Subsequently, we will dedicate a new section explaining our limitations in the revised manuscript.

Fourth, a combination of multiple earthquake occurrence models can be included as a logic-tree approach. However, we did not explore this in this study as it is out of the scope. We stay on our objective: presenting a new method that combines multivariate Bernoulli and stochastic source modelling methods, which we hope to promote many applications in future.

Fifth, the valuable suggestion from the reviewers to re-do the analysis, specifically in the earthquake rupture modelling and Bayesian parameter estimation, is essential. We will re-do the earthquake rupture modelling and Bayesian parameter estimation and include the updated results in the revised manuscript:

For the earthquake rupture modelling:

- The instrumental events (i.e. the 2007a, 2007b and the 2010 events) will be excluded in modelling the time-dependent earthquake rupture.
- The earthquake catalogue data to develop a magnitude-frequency model for the time-independent approach will include the paleogeodetic records provided in Philibosian et al. (2017) by excluding the instrumental events (i.e. the 2007a, 2007b and the 2010 events). Therefore, both the time-dependent and time-independent approaches use similar earthquake data. An updated frequency-magnitude model will then be developed for the time-independent approach by fitting the Gutenberg-Richter (GR) relationship using Weichert (1980) method to treat the varying completeness magnitude. Moreover, the number of low-magnitude scenarios (i.e. < M 7.625) will be reduced by considering only the magnitude of ≥ M 6.0 (in the submitted manuscript, we use the magnitude of ≥ M 5.0).

For BPT parameters estimation using the Bayesian approach:

- The prior setup needs to be updated and adopt the uninformative priors, e.g. as used by Fitzenz et al. (2010).
- The BPT parameters estimation using the Bayesian approach will be further improved to use both the maximum a posteriori (MAP) and full posterior parametric uncertainty in defining the final BPT parameters to consider the uncertainty in BPT parameter estimation.

Furthermore, to respond to the reviewer's comments, we have copied the reviewer's comments and in italicized text.

Major comments

Justification of the choice of a time-dependent approach. A number of recent studies of global paleoearthquake records (Williams et al 2019; Griffin et al 2020; Moernaut 2020) have, to varying degrees, provided empirical support for weakly quasiperiodic earthquake recurrence as a general model, which can be used to justify the use of renewal models for hazard assessment. That said, the Mentawai record of Philibosian et al (2017) looks to be more random than quasiperiodic in the analysis presented by Griffin et al (2020), although perhaps a different result might be obtained using the segmentation model presented here. The posterior BPT parameter estimates given for each segment are also relevant – some give values of alpha ~1 (segments 2, 3 and 4), implying random recurrence (i.e. Poisson), while others are ~0.6 (segments 1, 5 and 6), implying moderately quasiperiodic recurrence. So, I think some comment needs to be made here that:

1. **At a global scale there is empirical support for weakly quasiperiodic earthquake recurrence as a general model (see Griffin et al 2020);**

2. **Excluding the hypothesis at the individual fault level is difficult, particularly for short records (Williams et al 2019; Griffin et al 2020)**
3. **The data from Philibosian et al (2017) is somewhat equivocal about whether earthquake recurrence here is truly time-dependent, and the Poisson hypothesis cannot be confidently excluded using these data. But the global studies mentioned above suggest it is not unreasonable to assume time-dependence as a hypothesis.**

The discussion section of the paper could then discuss the implications of this assumption in light of the different values of alpha obtained for each segment.

Thank you very much for these valuable comments. We will revise the manuscript by adding the following text in two different sections to justify the chosen model (i.e. BPT model) once we are invited to revise it.

In the Introduction section:

In general, the earthquake rupture can be modelled using two approaches: Poisson and non-Poisson. The Poisson approach employs a memory-less Poisson process for long-term hazard assessment and is commonly adopted for earthquake rupture modelling (e.g. Burroughs and Tebbens, 2005; Tinti et al., 2005; Orfanogiannaki and Papadopoulos, 2007). However, assuming a lack of memory between major earthquake occurrences is often viewed as a first approximation, inconsistent with the physics of elastic rebound (Reid, 1911; Anagnos and Kiremidjian, 1984; Berryman et al., 2012). As a result, many studies adopted a renewal model of earthquake occurrence, i.e. non-Poisson model (Matthews et al., 2002; Zhuang et al., 2012; Field et al., 2014; Williams et al., 2019; Griffin et al., 2020) to carry out a time-dependent earthquake rupture modelling. More recent studies using global paleoearthquake records (i.e. Williams et al., 2019; Griffin et al., 2020; Moernaut 2020) showed that large earthquakes in the subduction zones recur more regularly than expected from exponentially distributed interevent times (i.e., a Poisson process). Specifically, the earthquake recurrence in the Mentawai Sunda subduction zone is categorized as more like a supercycle type (i.e. a combination of large gaps and clusters), demonstrating that successive large earthquakes are dependent on each other (Salditch et al. 2020). Therefore, this study adopts the renewal model (i.e. BPT distribution) for earthquake rupture modelling in the Mentawai segment of the Sunda subduction zone.

In the results section of 'Bayesian parameter estimation':

Error! Reference source not found.** illustrates the MCMC results with the priors and posteriors of μ, α, and γ for all segments, whilst the final parameter estimates are taken from the maximum a posteriori (MAP; Table 3). The figure shows that, in general, the available earthquake data can effectively reduce the parametric uncertainty of the priors, in particular for the μ parameter in* *segments 2 to 4. The median interarrival times of the central segments (i.e. segments 2 to 4) are about 40 years, while the interarrival times in the remaining segments are greater by more than 50%. The uncertainties of the parameters for the interarrival times of segments 1, 5, and 6 are large because few ruptures have occurred in those segments (see* ***Error! Reference source not found.*). Moreover, the data dispersion of the central segments is greater than in others, resulting in a higher coefficient of variation. Moreover, the values of* α *for each segments are varied depending on the segments. Segments 2, 3, and 4 have an* α *of ~1 implying random recurrence (i.e. Poisson). On the other hand, the values of* α *for segments 1, 5, and 6 are ~0.6 showing a moderately quasiperiodic recurrence. Such results are generally consistent with the findings from the recent studies using global paleoearthquake records, including the data for the Mentawai segment of the Sunda subduction zone (Williams et al. 2019; Griffin et al. 2020; Moernaut 2020). The study suggests that the earthquake recurrence for the Mentawai-Sunda zone can be time-dependent, but the Poisson hypothesis cannot be excluded to model the future earthquake rupture.*

In estimating parameters for the BPT distribution, the authors use the data to estimate the prior distribution of mu, before then using the same data to calculate the posterior probability distribution of mu. This is incorrect. I would suggest using an uninformative prior (e.g. as used by Fitzenz et al 2010). An alternative approach could be to use an informative prior for mu based on the slip rate (e.g. as determined from geodesy), but this may become complex (e.g. due to having to estimate coupling of the fault). The 450 year long record is short for accurately estimate model parameters. This is, of course, what a Bayesian approach should be helping with, but needs more care about the choice of priors.

We agree with the reviewer's comments and will re-analyze the Bayesian estimation by updating the following steps:

1. *The prior setup needs to be updated and adopt the uninformative priors, e.g. as used by Fitzenz et al. (2010).*
2. *The BPT parameters estimation using the Bayesian approach will be further improved to use both the maximum a posteriori (MAP) and full posterior parametric uncertainty in defining the final BPT parameters to consider the uncertainty in BPT parameter estimation.*

I am also concerned that fitting the model parameters to each segment individually is problematic. Later you consider multi-segment ruptures, and it is not clear how all this fits together. Do the recurrence statistics obtained from the sum of all synthetic ruptures across all segments match the recurrence statistics from the sum of all historic/paleo ruptures in your data? Checking this could be a good test for your model.

Thank you very much for raising this issue.

In our study, the rupture is modelled based on the integration of temporal and spatial interaction shown by $\boldsymbol{p_t}$ *and* Σ *in Equation (5) of the manuscript.* $\boldsymbol{p_t}$ *is a vector of the marginal probability of rupture on the* k*-th segment in year* t *given the time since the last rupture (*T_t*).* Σ *is a 6-by-6 covariance matrix describing the spatial correlation of ruptures on the segments. Subsequently, the rupture is modelled for each segment each year, where the spatial correlation, Σ, constrains the extension of the rupture. Such a spatial correlation allows us to model the rupture of the whole segment.*

Moreover, we will check the consistency of our rupture modelling results regarding the recurrence statistics once we are invited to revise the manuscript. Currently, we have not explored such a result consistency because we will re-do the rupture modelling as described in the introduction of our response.

Also related to parameter estimation, some of the posterior histograms seem a bit spiky; does this improve if the number of samples is increased beyond 10,000?

The posterior distribution does not change much even when we increase the number of samples. The spiky at the posterior distribution is generally found at the segment (e.g., segments 1 and 6) where the past earthquake data is insufficient (only one or two events rupture in those segments. Once we update the Bayesian parameter estimation in the revised manuscript (if we are invited to revise it), we will update the evaluation of the posterior distribution and the BPT parameter estimation.

Spatio-temporal completeness of the paleo record compared with the instrumental record is an issue that I think could lead to biases in the parameter estimates. It is very unlikely that events similar to the Mw 7.8 2010 Mentawai event would be visible in the coral record; this event occurred near the trench and caused <4 cm subsidence on the Mentawai Islands as measured with GPS (Hill et al 2012). Related to the above, the Mmin of 7.6 (L129), while reasonable from a tsunami hazard assessment perspective, would mean that you are modelling events that are unlikely to be present in the paleoearthquake record. I am unsure of how the frequency of these events could be determined in the time-dependent approach. Therefore it seems likely in your current approach that smaller events are missed in the paleoearthquake record, therefore affecting the recurrence model parameters.

Thank you very much for these valuable suggestions.

We realized that we have a few technical weaknesses in developing the earthquake rupture modelling, including:

(1) Use different catalogue data to model the magnitude frequency relationship between the time-dependent and time-independent models. The time-independent model adopts only the earthquake catalogue data from 1970, whilst the time-dependent data consider solely the paleogeodetic records from the sixteenth century.

(2) Consider a large number of small earthquakes (i.e. M 5.5 – M 6.0) for the time-independent model that is well below the minimum magnitude considered in this study (i.e. M 7.625).

(3) Include the instrumental events (i.e. the 2007a, 2007b and the 2010 events) for the time-dependent model that the paleogeodetic records can not detect.

Subsequently, the following revisions will be conducted in the revised manuscript once we are invited:

First, the instrumental events (i.e. the 2007a, 2007b and the 2010 events) will be excluded in modelling the time-dependent earthquake rupture.

Second, the earthquake catalogue data to develop a magnitude-frequency model for the time-independent approach will include the paleogeodetic records provided in Philibosian et al. (2017) by excluding the instrumental events (i.e. the 2007a, 2007b and the 2010 events). Therefore, both the time-dependent and time-independent approaches use similar earthquake data. An update frequency-magnitude model will then be developed for the time-independent approach by fitting the Gutenberg-Richter (GR) relationship using Weichert (1980) method to treat the varying completeness magnitude. Moreover, the number of low-magnitude scenarios (i.e. < M 7.625) will be reduced by considering only the magnitude of ≥ M 6.0 (in the submitted manuscript, we use the magnitude of ≥ M 5.0).

Consequently, we perform the initial modelling of the magnitude frequency relationship by considering the new earthquake catalogue (i.e. integration catalogue) to understand the difference in the magnitude frequency distribution before and after the integration of paleogeodetic records. The results show that the b value produced from the Weichert (1980) approach is far less than the previous model, i.e. $b = 0.72$ *for the Weichert model vs* $b = 1.05$ *for the catalogue excluding the paleogeodetic record. Furthermore, we plot the probability distribution of magnitude from the non-integrated (excluding the paleogeodetic records) and the integrated catalogues (including the paleogeodetic records) as shown in Figure R3-1. The figure clearly shows a significant change in magnitude probability for low (< M 8.0) and high (> M 8.3) scenarios. The probability of low magnitude in the integrated catalogue is almost twice smaller than the non-integrated catalogue. In contrast, the probability of the high magnitude of the integrated catalogue is twice higher than that of the-non integrated catalogue. Such a change will definitely influence the final hazard curve of the time-independent model.*

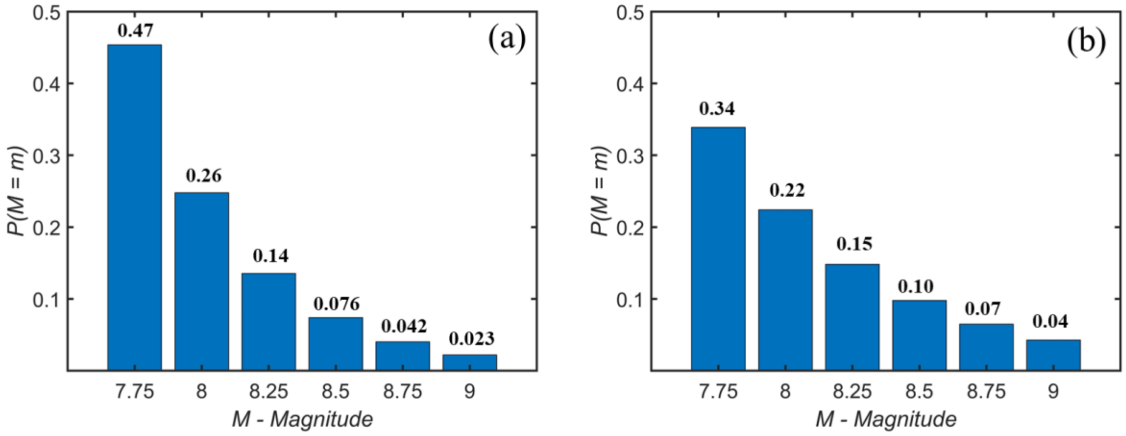

Figure R3-1. Probability magnitude distribution produced from the non-integrated catalogue (a) and the integrated catalogue (b).

The modified results for comparing time-dependent and time-independent PTHA will be included in the revised manuscript, specifically in the methodology, results and discussions sections. We will run the new earthquake rupture modelling for the time-dependent case by excluding the instrumental events and update the results accordingly once we are invited to revise the manuscript.

The 1D rupture segmentation is a problem for tsunami hazard assessment, as the resulting tsunami size depends so significantly on the depth of rupture. Compare the 2007 Bengkulu earthquakes (Mw 8.4 and 7.9), that were down-dip of the trench and did not generate a significant tsunami, with the 2010 Mentawai earthquakes (Mw7.8), which occurred near the trench and did generate a significant tsunami. It is not clear whether such events are discriminated by the stochastic modelling approach with 1D segmentation – it seems they probably aren't, but I may not be understanding correctly.

Thank you very much for your valuable comment.

The 2D segmentation can also be possibly represented by our current setup. The first direction is along strike segmentation whereas the asperity region can be regarded as a along width segmentation as large concentration of slips is allowed in this area. This was initially considered to accommodate notable observations from the recent tsunami events, including 2011 Tohoku – large concentration of slips along the trench line. In reality, this may capture various geological environments – such as outer wedge rupture. However, such an explanation has not been included in the submitted manuscript and we will include this in the revised manuscript once we are invited to revise it.

On the other hand, In our setup, the strike and dip angles of the fault-plane of tsunamigenic source models are typically 296^{o} to 326^{o} and 7^{o} to 19^{o}, respectively. These values are comparable to the slab models for the Sunda *subduction zone produced by the USGS (Hayes et al., 2009, 2012). The top edge of the fault plane is located at a depth of 3 km. This depth is consistent with the past Mentawai finite-fault models developed for the 2010 Mentawai tsunamigenic earthquakes and the twin events of the 1797 and 1833, which have the top edge depth between 2 km and 5 km (Newman et al., 2011; Satake et al., 2013; Philibosian et al., 2014; Yue et al., 2014).*

We have not explained this clearly in the manuscript, and we will include it in the manuscript once we are invited to revise the manuscript.

A related problem is low-rigidity near the trench and its tsunamigenic potential, as in the 2010 Mentawai tsunami? How might the assumption of constant (and relatively high) rigidity (L309-310) bias your tsunami hazard results?

In our approach, we allow large concentration of slips in the asperity region. This was initially considered to accommodate notable observations from the recent tsunami events, including 2011 Tohoku – large concentration of slips along the trench line. In reality, this could capture various geological environments – such as outer wedge rupture. The large slip 'could' reflect low rigidity of the outer wedge of the accretionary prism or others.

Moreover, It is also important to note that all inversion studies in this modelling framework are typically done by considering a (hard) constant ridigity, to match the predictions with the observations. Consequently, the slips along the trench are large. Therefore, the stochastic earthquake source modelling used in this study with a constant rigidity and allowable large slip concentration in the asperity region may still capture realistic tsunami hazard levels in the region of interest.

The maximum magnitude of 9.0 seems too low, which seems related to the segmentation model. If the potential for ruptures connecting with other segments of the Sunda Subduction Zone is considered, then larger Mmax values are justified. Significantly larger Mmax's were used in Horspool et al (2014). Even if the paleoearthquake record for the past 450 years suggests events haven't exceeded Mw 9.0, we also don't expect these magnitude events to occur all that often. So allow for the possibility that they are missing from the record.

Thank you very much for your comments.

In this study, to integrate the spatio-temporal time-dependent earthquake rupture modelling, we adopt a 1D along-strike distance discretized into six segments. Each segment represents the smallest area that may rupture in tsunamigenic earthquakes. Such a 1D along-strike model is developed based *on the past fault rupture, and the total distance of these 6 segments (i.e. 600 km) is used to represent the maximum magnitude scenario.*

The maximum magnitude scenario was selected based on geodetic, paleo-geodetic, and paleo-tsunami studies (Zachariasen et al., 1999; Natawidjaja et al., 2006; Sieh et al., 2008). Those studies indicated that the accumulated slip in the Mentawai segment of the Sunda subduction zone might generate tsunamigenic earthquakes ranging from M 8.8 to M 9.1. Specifically, coral microatoll samples from 21 sites along 600 km of the Mentawai region were used to constrain the dates, spatial extents and approximate earthquake source models for tsunami generation (Natawidjaja et al., 2006; Shieh et al., 2008; Philibosian et al., 2014, 2017). In our study, the tsunami simulation can not be extended into more than six segments (600 km) due to the availability of the coral microatoll samples beyond these six segments. However, this 600-km segment with a total area of 150,000 km^2, i.e. 600 km (length) × 250 km (width) is still consistent with the scaling relationships (e.g. Goda et al. (2016) and Thingbaijam et al. (2017)). Figure R3-2 presents the relationship between the fault areas and magnitude based on the Goda et al. (2016) and Thingbaijam et al. (2017) scaling relationships. The figure shows that the maximum fault areas used in this study represent the maximum magnitude of between M 9.0-9.2. Moreover, we allow the model to have a maximum magnitude up to about M 9.1 (+/- M 0.1 of M 9.0) in developing the stochastic earthquake source model, and hence, such a number is still sufficient to represent the maximum magnitude event that may occur in the Mentawai segment of the Sunda subduction zone. Therefore, it can still be adopted to constrain the boundaries of finite fault models developed in this work.

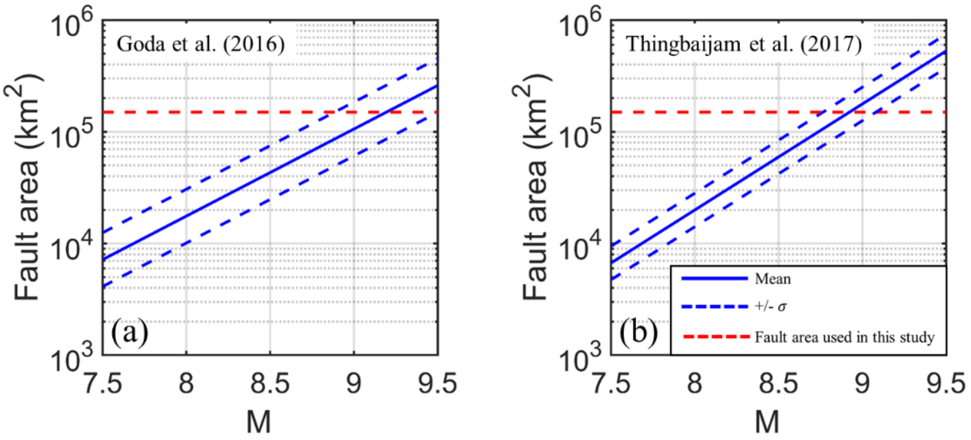

Figure R3-2. Fault areas and M relationships based on the Goda et al. (2016) and Thingbaijam et al. (2017) relationships.

More importantly, we are aware that this is one of the limitations of our current work. Therefore, we will emphasize this issue in the following revised manuscript once we are invited to revise it.

Some area of the coast of Padang show zero probability of inundation (Figure 17), while in others the potential inundation extent extends quite a way inland. This raises some significant concerns for me about the quality of the inundation modelling and/or the elevation data used, given how low-lying the coast is in this area. If only SRTM data was used, this could significantly underestimate inundation extent (see Griffin et al 2015, Figure 8). Are buildings included in the elevation model?

Thank you very much for your comments.

The building is not included in our model, and we don't solely use the SRTM data. For Padang areas, we use the local DEM5 and Bathy5 for the DEM and bathymetry data (Taubenbock et al., 2009; Schlurmann et al., 2010). Outside the Padang region, we adopt the SRTM dataset.

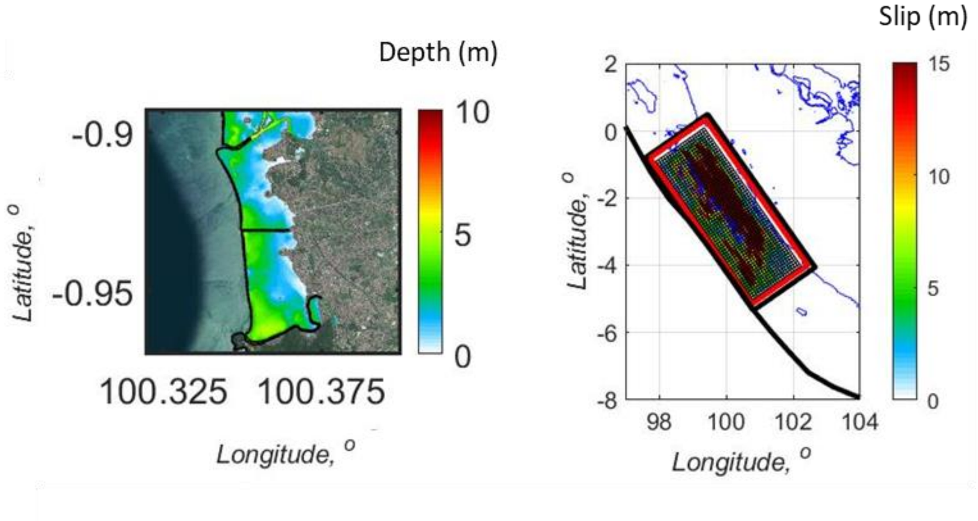

Figure R3-3. Tsunami hazard map (right panel) in Padang produced from one of the M 9.0 stochastic source models (i.e. model 9; left panel).

The inundation results in Figure 17 are developed based on the time-dependent and time-independent methods for two different probabilities (10% and 2%) in 50 years. Using such an approach, we take the tsunami hazard at an interest probability level of 0.02 and 0.1. Therefore, the extension of inundation inland is zero in some of the Padang's areas compared to our previous tsunami hazard maps in Padang (i.e. in Muhammad et al., 2017 and 2018). Suppose we consider only the tsunami hazard maps from one example of generated stochastic source models. We may see the extension of the tsunami hazard maps to be further inland, as shown in Figure R3-3.

Detailed comments

L15: Suggest change 'A total of >' to 'More than'

> *We will update the manuscript based on your suggestions.*

L18: Forecast periods begin in what year?

> *The forecast began in 2021. We will add such an explanation in the revised manuscript.*

L136: Choice of BPT is fine, but hasn't really been justified here. Why is this chosen over lognormal, Weibull or Gamma? Some of your justification seems to be presented later in Section 2.2.

> *In the revised manuscript, we will add the following texts (in the introduction) once we are invited to revise the manuscript:*
>
> > *In general, the earthquake rupture can be modelled using two approaches: Poisson and non-Poisson. The Poisson approach employs a memory-less Poisson process for long-term hazard assessment and is commonly adopted for earthquake rupture modelling (e.g. Burroughs and Tebbens, 2005; Tinti et al., 2005; Orfanogiannaki and Papadopoulos, 2007). However, assuming a lack of memory between major earthquake occurrences is often viewed as a first approximation, inconsistent with the physics of elastic rebound (Reid, 1911; Anagnos and Kiremidjian, 1984; Berryman et al., 2012). As a result, many studies adopted a renewal model of earthquake occurrence, i.e. non-Poisson model (Matthews et al., 2002; Zhuang et al., 2012; Field et al., 2014; Williams et al., 2019; Griffin et al., 2020) to carry out a time-dependent earthquake rupture modelling. More recent studies using global paleoearthquake records (i.e. Williams et al., 2019; Griffin et al., 2020; Moernaut 2020) showed that large earthquakes in the subduction zones recur more regularly than expected from exponentially distributed interevent times (i.e., a Poisson process). Specifically, the earthquake recurrence in the Mentawai Sunda subduction zone is categorized as more like a supercycle type (i.e. a combination of large gaps and clusters), demonstrating that successive large earthquakes are dependent on each other (Salditch et al. 2020). Therefore, this study adopts the renewal model (i.e. BPT distribution) for earthquake rupture modelling in the Mentawai segment of the Sunda subduction zone.*

L174: Several thousand years

> *We thank you for this comment and will revise the manuscript based on your suggestions.*

L185: Perhaps rephrase as 'reflects the expectations of elastic rebound theory', or similar.

> *We will revise the manuscripts based on your suggestions.*

L192: Should probably cite others who've used Bayesian approaches to fitting time-dependent models to earthquake records, in particular Rhoades et al (1994) and Fitzenz et al (2010).

> *We will cite the mentioned literature in the revised manuscript.*

L197 and Table 1: These should not be referred to as tsunamigenic. For half of them we have no information on whether a tsunami was generated; coseismic deformation on the Mentawai Islands observed in coral paleogeodetic records suggests they probably were, but we don't actually know.

> *In the revised manuscript, we will update the term of tsunamigenic to earthquake event.*

L324. Please give a link or citation for DEM5 and Bathy5.

> *We will add the following citations in the revised manuscript: Taubenbock et al., 2009 and Schlurmann et al., 2010.*

L332: Might be a typo here – Griffin et al (2016) used a Manning's roughness of 0.036 as a conservative minimum for land (grassland; for the Mentawai Islands). For the urban context here, 0.06 may be reasonable, e.g. Griffin et al (2015) suggested a Manning's roughness of 0.08 for the city of Padang. See also Kaiser et al (2011) for a discussion of choice of Mannings n.

> *Thank you very much for noticing such a typo. We will update the typo and include some essential references here in the revised manuscript.*

Ling 501-502: The time-independent model has too low an Mmax (9.0) to be considered worst-case. See earlier comments about choice of Mmax.

> *Thank you very much for the comment. We have answered this in the major comment point 8.*

Table 1: Change Shieh to Sieh.

> *We will revise the manuscripts based on your suggestions.*

Figure 3, and also in the text. I do not think the term 'occurred' scenarios is the best terminology. These are modelled scenarios that have not actually occurred.

> *Thank you very much for this comment. The term 'occurred' scenarios will further be updated to the 'simulated scenarios' in the revised manuscript.*

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