Responses to Anonymous Reviewer 2.

Thank you for your helpful review. Please find our answers to each of your comments below.

A. General comments

>>"*The manuscript investigates a novel method for accounting for damage accumulation on large building portfolios exposed to sequential earthquake and tsunami hazards. This work is of great interest to the readers of NHESS. The paper is well-structured and clear, despite very minor typos. However, some contents of the current manuscript need to be clarified and a few sections to be improved before the paper can be considered for publication*".

Thank you for your nice comments about the structure of the paper and your suggestions. Following your advice, we have accordingly asked an editor (a native English speaker) to carry out a strict language review. The new version has been significantly been improved in that regard.

1. >> "*A.1 Lines 100-103. The purpose of this study is stated in these three sentences. The aim is relatively clear, however it remains unclear how the state-dependent fragility models are obtained. This is one of the critical parts of this methodology and should be briefly explained at this stage, as this would help the readers*".

    Thank you. We really appreciate your constructive comment. We agree that we had formerly mentioned the details about the derivation of the tsunami state-dependent fragility functions at a late stage. For instance, the citation to the open data repository Gómez Zapata et al., (2022a) was cited in line 775 despite the fact that the dataset was mentioned for the first time on line 325 where details about the specific selected model were provided (Sect. 3.3). In the updated version, the reference has been included at the end of Sect. 2.3.

More details have also been provided and the following lines have been added:

    "*For multi-risk assessment, such a damage-updated exposure model (i.e. building classes and damage limit states) requires to be associated with a set of tsunami fragility functions that can account for the pre-existing damage caused by the first event (shaking). In the application chapter of this paper, a set of tsunami state-dependent fragility functions are obtained through the use of simple ad-hoc scaling factors. Nonetheless, it is important to stress that these functions should be replaced by state-dependent tsunami fragility functions derived from more sophisticated methods as soon as they would become available*".

    We sincerely think that these new lines will provide a smother reading experience. Thank you.

2. >> "*A.2 Lines 125-130. Regarding the integration of a set of modular components, the authors should provide a clearer definition of "inter-scheme compatibilities" and "their related compatibility levels between inter-scheme damage states". I recognise that these concepts are described in more detail in the following sections, but this paragraph is quite difficult to read. It is not clear what these 'conversions' are… ".*

    We fully agree with the reviewer's comment. Although the method to derive the first set of these inter-scheme compatibilities i.e., between 2 sets of building classes, summarised in Sect.

2.1, was already formally presented in a separate publication Gómez Zapata et al., (2022b), we have now realized that the motivations to use such an approach were not clearly enough stated. Therefore we have included the following sentence:

"By reusing this approach, a building stock initially classified into earthquake-oriented typologies can now be probabilistically represented by tsunami-oriented typologies".

Furthermore, more clarity can be achieved if we state that these "conversions" are ultimately represented through two sets of *"compatibility matrices"*, which are probabilistically generated by the methods explained in detail afterward. We have also realized that the word "matrix" is clue herein and it was missing. We have therefore modified the sentence as follows:

"The two aforementioned conversions are ultimately represented through two sets of "compatibility matrices" that are probabilistically generated. The advantage of using these matrices is that through these conversions, the damaged updated exposure model resulting from the action of IM^A can be represented in the domain of the reference scheme attached to the second vulnerability to be analysed, and the damage updated exposure model can be directly used for a second risk computation, in this case for tsunami risk addressing the cumulative damage".

Reviewers continues *>> "…and how they make it possible for a cumulative assessment of the damage".*

In order to provide clarity in this regard, even at this early stage of the paper, we have decided to complement the former part of the same paragraph by adding the following explanation.

*"This is achieved by using **(3)** generic state-dependent tsunami fragility functions (i.e. with non-zero initial damage states made of new curves that represent the permissible damage progression). Since the resultant earthquake-induced damages are formerly expressed in the tsunami vulnerability domain (step 2), the non-zero damage limit states of this set of state-dependent tsunami fragility functions will implicitly account for such pre-existing damage. The joint ensemble of these three components can be ultimately used to calculate the cumulative expected damages after the triggered event with IM^B, while accounting for the preceding induced by IM^A (i.e., blue part in Figure 1b, developed in Sect. 2.3).*

3. *>> "A.3 Lines 160-177. The AeDES forms have been consistently used for post-earthquake damage assessment. The authors state that these can also be used for tsunami: why? Also, did the expert elicitation help to adapt these forms to tsunami damage?"*

We understand the reviewer's concern about this issue. However, we should emphasize some ideas that perhaps were not presented clearly enough.

One of the main current problems faced by scientists working on the derivation of empirical fragility functions for assets exposed to various hazards (i.e. that require damage reconnaissance campaigns) is the absence of a standard scale to assess the observable damage in a comparable manner across hazard-dependent vulnerabilities. The reviewer is

fully right that the AeDES scale (Baggio et al., 2007) had never been used to assess tsunami-induced damage before.We have considered that that aspect is not problematic, but it is rather an opportunity to demonstrate that the observable physical damage incurred by any kind of hazard can be transversally described by using "low-level taxonomic elements" (i.e. a set of individual building components).

The proposal of making use of the AeDES scale in the application section of the paper is not determinant nor compulsory for the usability of the proposed method. Actually, in point 3 of the discussion section (lines 553-556) we had already stated *"In this regard, although we have used the AeDES scale, other damage scales could be more suitable to describe the observable damage to some building classes than for others"*.

For instance, the EMS-98 scale (Grünthal, 1998) with 5 categories originally proposed for ground-shaking damage, had been the selected damage scale used to assess the damage extent on building types at the beginning of our study through expert elicitation. Notably, as we described in lines 53-55, the EMS-98 vulnerability classes have the great advantage that it has been already used not only to assess the likely observable damage due to seismic action, but it has been also used to classify likely ranges of vulnerabilities to other hazards (i.e. tsunami, floods, strong winds) based on the building's material types (Schwarz et al., 2019; Maiwald and Schwarz, 2019). However, since the scope of implementing a standard damage scale was ultimately to generate a synthetic dataset of observable damages for the various building types, the 0-5 scale of the EMS-98 was too limited. In fact, we noticed that using this scale, many of the damage features on some building components overlapped across damage limit states (i.e. they were not clearly distinguishable between adjacent damage states, e.g. infills and partitions for slight and moderate damage).

The selection of the EMS-98 damage scale resulted in a very limited number of produced data points in this synthetic collection, and hence not exhaustive enough to produce comparatively more robust probabilistic inter-scheme damage compatibility matrices. On the contrary, having used the AeDES scale, with a 0-9 scale and already related to the four selected specific building components (as we did in the submitted version of our paper), allowed us to generate a more exhaustive synthetic dataset (see Fig. 8 of the submitted version), and thus, we consider that this section also allowed us to produce better quality damage-related conversion matrices (Fig. 9 of the submitted version).

Details about the expert elicitation used in the AeDES form for tsunami-induced damage will be described in the answer to the next comment.

4. >> *"A.4 Lines 178-185. What are the details of the expert elicitation? Who took part in this exercise? Did the expert elicitation involve experts in earthquake and tsunami engineering?"*

The expert elicitation for the two hazard vulnerabilities of our interest, as explained in the paper, consists in the selection of the damage level (among the 0-9) scale that is likely to be observed for each damage limit state and building typology. In the submitted and present version we cite the open data repository Gómez Zapata et al., (2022c) which contains a *Jupyter notebook* and *Python scripts* where these scores and the subsequent process of using the derived synthetic dataset to classify their respective compatibility through machine learning.

Expert elicitation for earthquake vulnerability was supported by Dr. Nicola Tarque, co-author of our paper. He is also a co-author of the Villar-Vega et al., (2017) study that proposed the seismic fragility functions for the SARA building classes, upon we have based our calculations. Nicola Tarque is originally a Peruvian researcher, with extensive experience in deriving fragility functions for masonry, adobe and reinforced concrete buildings in Peru[1], and hence, he is aware of the damage features that can be expected from the damage limit states defined within this set of fragility functions for the local Peruvian building types contained in the exposure model we adopted.

On the other hand, considering that the building typologies contained in the Medina scheme have analytical tsunami fragility functions associated, the AeDES-based scores used to generate the resultant synthetic dataset were constrained through expert elicitation by thinking as if the tsunami forces alone were the cause of the described damage features (i.e. far-field tsunami). For this exercise, the authors of the models proposed by Medina et al., (2019), who are also co-authors of our paper (Dr. Juan Lizarazo and Sergio Medina) contributed to selecting the observable damage features for the four damage limit states of every fragility function associated with the selected set of six building types that they had defined in their previous study Medina, (2019). They are two recognized members of the tsunami-vulnerability community, who for instance have had numerous research exchanges in the framework of the Japanese-Colombian project SATREPS[2] about multi-hazard risk.

Reviewers continues >> *"What are Scheme A and Scheme B? Are A and B earthquake and tsunami, respectively? It is quite confusing"*

Scheme *A* and Scheme *B* are referring to the selected models for earthquake and tsunami vulnerability (i.e. in the application section: SARA and Medina, respectively). Such a definition had been presented in Figure 1-a as well as in several parts of the submitted version of our manuscript.

5. >> *"A.5 Lines 203-227. State-dependent fragility functions are developed for accounting for the cumulative damage. It is not clear the fragility function for a building type damaged by the earthquake and then by the tsunami is obtained. What are the scaling factors? How are these calculated, based on what ad-hoc calibration? The authors should clarify this key element, which remains quite obscure in the application as well".*

We deeply thank the reviewer for his/her comment.

It should be noted that we used and cited the open data repository Gómez Zapata et al., (2022a) where the assumptions to derive such ad-hoc state-dependent tsunami fragility functions are documented. This repository was and is also cited in the "Code and data availability" section (line 659) and it is accessible through the URL provided (line 775 of the originally submitted version). It is worth noting that this data repository has currently a temporary review link, and its designated DOI will be available once this paper has been published.

Therefore, we have done our best to ensure that involved assumptions and software are well documented and avoid the "obscurity" of "black boxes". Making available this information through the GFZ data service is a transparent practice that our team has been pushing for some time. Hence, the readers will be able to compare and possibly improve our approaches in the future.

However, the reviewer's comment has helped us to realise that some of the related ideas regarding these topics were not clearly expressed within the text. Since the application part of the method (Sect. 3.3) fully depends on the selected set of fragility functions that are linked to the building classes for tsunami vulnerability (i.e., Medina, 2019), no generalization was done in Sect. 2.3. However, as already stated in the response to comment "A.1", the open data repository open data repository (Gómez Zapata et al., 2022a) has been now cited at the end of Sect. 2.3 of the updated version. Then, the reader is able since an earlier stage to get more details about the derivation of the ad-hoc scaling factors.

It is worth noting that a part of Sect. 2.3 was formerly devoted to present the assumptions of our approach, which is largely based on the ideas of Mignan et al., (2014) (please see page 4, Fig. 6). Considering the former citation as well as the pertinence of having cited the auxiliary data repository, we have carefully evaluated the necessity of repeating some of the assumptions that the aforementioned paper already documents. Therefore, one of the main modification that we have made to the revised manuscript is having removed Equation 9. This is because this equation can be consulted in the aforementioned reference, it is simply a generic expression of a lognormal cumulative distribution and as the reviewer correctly noticed it was not sufficiently pointing out the assumptions we followed to derive state-dependent fragility functions. Hence, we have now included some of the theoretical assumptions we that we followed in the updated version of the paper, which have been always documented in the cited data repository (Gómez Zapata et al., 2022a), where the reader can find the specific example case we tested in Sect. 3.3. The following has been included:

*Only for the overall scope of this paper, we propose that state-dependent fragility functions can be simplified by using ad-hoc calibration parameters to modify these logarithmic mean values. For such a modification, we propose applying to them the exponential operator to obtain the physically accountable mean IM (hazard intensity measures). I.e., $\lambda_{q_0}(T_r^I)$ defines each damage state as: $\lambda_{q_0}(T_r^i) = e^{\mu_{q_0}(T_r^i)}$. Subsequently we propose to obtain their respective differences $\Delta\lambda_{q_0}$. For example, if a fragility function is composed of $q_{N_i} = 4$ damage states (excluding damage state 0, equivalent to no damage), there will be a set of damage states $\lambda_{q_0} = [\lambda_{q_{0_1}}, \lambda_{q_{0_2}}, \lambda_{q_{0_3}}, \dots \lambda_{q_{0_i}}]$ for which we should obtain the differences between all the possible top and bottom damage states and we must obtain six values: $\Delta\lambda_{q_0} = [\Delta\lambda_{q_{0_{1,2}}}, \Delta\lambda_{q_{0_{1,3}}}, \Delta\lambda_{q_{0_{1,4}}}, \Delta\lambda_{q_{0_{2,3}}}, \Delta\lambda_{q_{0_{2,4}}}, \Delta\lambda_{q_{0_{3,4}}}]$. In this example, these six state-dependent transition values are included within the $G_f = 10$ triangular number (i.e. 4 from 0; 3 from 1; 2 from 2; 1 from 3) given by **Error! Reference source not found.** Thereby, for each T_r^i, it is still necessary to determine the probabilistic representation (log mean and log standard deviation) of every damage state transitions $\Delta\lambda_{q_0}$. To do so, the $\lambda_{q_0}(T_r^i)$ values are proposed to be multiplied by the $\Delta\lambda_{q_0}$ factors, and reframing this quantity to a natural logarithm in order to approximate it back again to lognormal mean values. This is expressed as given by Eq. 1.*

$$\delta_{w|y} = ln(\Delta\lambda_{q_0} \times \lambda_{q_0}). \qquad\qquad Eq.\ 1$$

The reader should note that in this approach, the $\Delta\lambda_{q_0}$ vales are a set of ad-hoc calibration parameters or scaling factors that are applied directly to the IM λ_{q_0} for which each damage limit state was originally

derived. These values form the lognormal mean of the state-dependent fragility functions. A similar approach was followed by Rao et al., (2017). The fragility functions used to constrain the state-dependent fragility functions should have been derived only for the actual second acting hazard (i.e., far-field tsunamis). Thus, the use of those derived analytically is advised over empirical ones (which had implicit the damaged induced by ground-shaking in their derivation). Further details about this approach and model assumptions to find the ad-hoc calibration parameters are provided for the example case in the data repository Gómez Zapata et al., (2022a).

The methodological aspect of the former assumption is of course open for improvement, but as we stated in the discussion section (lines 540-542): the acquisition of "*real datasets that document observations on building components (even different from the ones presented in Eq. 3) could contribute to refine state-dependent tsunami fragility functions (…)*". Hence, the ad-hoc scaling factors are only place-holders within the method we propose and other, more sophisticated methods should replace this simplistic, but pragmatic working hypothesis.

6. **>> "A.6 Figure 11. I would present a similar work-flow for the general methodology earlier in the manuscript".**

We sincerely thought that figure 1-b was informative enough to present some basic concepts, and that a workflow only for the actual application case will only make sense once the punctual study area and the enclosed assumptions (as shown in Fig. 11) are first clearly presented. Another figure could be seen as redundant since the new figure would contain the sentences that are already presented in Fig. 11 (without the visual advantage of showing a geographically located earthquake rupture, exposure model, damage forecast, fragility functions). We do believe that the graphics within this figure are really helpful to the reader to understand the overall method. To take into account the reviewer comment we suggest that this figure can be the "selected main figure" of our paper. This is a great alternative that the NHESS journal offers to show the "most important figure of the manuscript" (please see **here**[3] how Fig 5 of this paper is shown as a "visual abstract" directly on the main web version of the paper).

B. Specific comments

1. B.1 Line 34: Indian Ocean Tsunami: *Corrected.*

2. B.2 Line 83: consider also Petrone et al., (2017) a relevant study on tsunami analytical fragility functions for a single building.
Ok. It has been added as suggested. Thank you. However, please note that this reference was already provided somewhere in the text for a similar context (line 649) to not be repetitive.

3. B.3 Line 96: "to" repeated twice: *Corrected*.

4. B.4 Line 111: "scenario" instead of "scenarios": *Corrected*.

5. B.5 Line 114. Please clarify the meaning of "vulnerability modes"

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[3] https://nhess.copernicus.org/articles/21/3599/2021/

We meant "vulnerability models". Sorry about it. It has been corrected.

6. B.6 Line 127. The authors refer to the "purple part" and then "red part" and so on – however the use of colours might be challenging if the paper is printed using greyscale. I would suggest to use a different way to identify the different components in Figure 1b.

   The use of colours in all of the figures is of our preference. The paper will be print in colour. We have experience publishing in the NHESS journal and we can ensure you that it will be that way. You should not worry about it. Thanks anyways.

7. B.7 Line 645. Amend typo "an\": Corrected.

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