

## ***Interactive comment on “Integrated tsunami vulnerability and risk assessment: application to the coastal area of El Salvador” by P. González-Riancho et al.***

**Anonymous Referee #2**

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The manuscript addresses relevant scientific questions within the scope of NHESS. The authors state that the objectives of the paper are to (1) propose a methodological framework for integrated tsunami vulnerability and risk assessments, (2) establish a clear connection between vulnerability and risk assessments with risk reduction measures, and (3) apply this methodology to the El Salvador coast. I believe the authors are successful in the third objective but not the first two objectives. The manuscript has value for publication in the NHESS as an interesting case study of community vulnerability to tsunami hazards in El Salvador. The authors are encouraged to focus on that application in a revised manuscript and reduce the text that attempts to show the

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uniqueness of the first two objectives. Text on pages 2884 to 2902 could be greatly condensed. Also, the discussion of the study area results could be better framed to help international readers understand lessons learned from the experience, instead of simply stating which communities have higher sensitivity values. The typical NHESS reader will have more interest in what they can learn and apply for their area of interest, and less interest in the details of the El Salvador results.

The authors are not completely successful with their first objective of proposing a new methodological framework. They devote a considerable amount of text to summarize the literature on vulnerability conceptual frameworks but end up using a framework that is not very different from already published work, such as that from Turner et al., Birkmann's MOVE framework, or Polsky et al.'s Vulnerability Scoping Diagram (Polsky, C., R. Neff, and B. Yarnal (2007). "Building comparable global change vulnerability assessments: the vulnerability scoping diagram." *Global Environmental Change* 17(3-4): 472-485). The framework discussed in this manuscript is not a substantial advance in new concepts, tools, or methods. The section summarizing the vulnerability literature could be greatly condensed.

The manuscript lacks an adequate discussion of the difference between risk and vulnerability assessments. The authors are encouraged to read the Sarewitz et al. (Sarewitz, D., Pielke, R. and Keykhah, M., 2003, *Vulnerability and risk: some thoughts from a political and policy perspective*, *Risk Analysis*) discussion of these differences as a starting point. Sarewitz and others would likely disagree with the authors' assertions that "risk can be mitigated by reducing the vulnerability" (p. 2893, line 6). In addition, although the authors correctly define risk as the likelihood of negative consequences over a certain time period, they do not implement this definition in their indicators. Subsequent analysis is based on aggregating hazard indicators and exposure/sensitivity/coping indicators. Risk analysis is a conditional probability based on the probability of an extreme event and the probability of asset failure/damage/injury/etc. given the extreme event. The hazard zone was defined by

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a deterministic scenario with no description of occurrence likelihood for the various sources in Figure 6. There is also no description of likelihood of failure for the various societal assets (people, buildings, habitats). It seems that failure/damage/loss is assumed to be 100% for any asset in a hazard zone, which itself is a composite of multiple sources. That is fine from a vulnerability perspective, but is inadequate from a risk perspective. In summary, the article attempts to provide an integrated vulnerability assessment, but cannot really be considered a risk assessment.

The manuscript is also not successful at its attempt to provide an integrated approach for coupled human-ecological systems. The analysis presented in the manuscript is fairly reductionist, in that it offers a set of indicators and then combines them for a final, aggregated value. The paper lacks discussion on feedback loops and interrelationships between the environmental setting and societal sensitivities or coping capacities. Related, the authors assert that a coupled system perspective is required for risk management. I don't completely agree with that assertion. Some vulnerability issues may not require a holistic assessment. For example, minimizing life loss from future tsunamis through evacuation training doesn't really require an understanding of the habitat health or GDP exposure.

The manuscript lacks an adequate discussion of how indicators and variables were generated. The text summarizing index creation in the literature (p.2895) could be greatly condensed. In its place, the authors can devote more time to describing how they developed their indicators. The text and Table 2 lack sufficient discussion of how variables were chosen, how they were defined (e.g., literature? Stakeholder opinions?), data sources for the variables, and how indicator classes were designed. Table 2 has a long list of variables that are organized into indicator groups but there is inadequate discussion to justify this organization or choice of variables. Also, the list of variables and indicators mix together many different risk issues, such as life safety, ecosystem services, economic loss, and the authors should discuss the advantages and disadvantages to this mixed indicator approach. With regard to the resilience variables, the

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authors present a fairly simple view of the topic. They could benefit from reading from works such as Cutter et al., 2008 (A place-based model for understanding community resilience to natural disasters, *Global Environmental Change*, 18, 4) as a starting point for this field of research. The yes/no/partially responses to the resilience questions seem highly subjective and not very robust. The authors need to provide more justification based on the literature or stakeholder engagement for the questions being asked. Also, the authors state that multiple people within a jurisdiction were asked to answer the resilience. How were conflicting answers within a jurisdiction resolved? Finally, it is unclear how figures 11 and 12 vary in terms of vulnerability calculations for national and local assessments.

The authors seem to vacillate in their use of term sensitivity in their analysis. It is defined as intrinsic quality of an asset (e.g., age of exposed population), but is later used to describe a community that has a high number of people in a hazard zone (p. 2904, line 23). The authors need to be consistent when discussing demographic sensitivity or community sensitivity, because they are different concepts. In that same paragraph, the authors state that a community is more sensitive than others because of the presence of mangroves. However, some believe mangroves are good because they may reduce wave energy. This is an example of feedback loop that could be discussed more in a revised manuscript.

With regard to the second manuscript objective, I don't feel the authors were successful. They state that want to establish a clear connection between vulnerability and risk assessments with risk reduction measures. However, the brief text in section 4.3 and Table 7 are not successful. Table 7 has an extensive list of recommended risk-reduction measures but there is not a strong connection to the results of the vulnerability assessment. The proposed measures are all good but are generic and don't really require a detailed vulnerability assessment to realize their potential. Table 7 includes a great amount of detail of vulnerability results but their explicit connection to the risk-reduction measures are tenuous. For example, on p. 2925, the authors state that 30% of the ex-

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posed population are below 10 yr or above 65 years and that is used as the basis for evacuation planning. However, evacuation planning could proceed without this level of demographic detail.

Other points to consider:

- p. 2884, lines 16-17 – The authors need to provide more support for their assertion that (1) tsunamis are rare phenomena, and (2) they are greater threats than earthquakes, hurricanes, and tornadoes. I disagree with both assertions.
- There is a great deal of redundancy in the first half of the manuscript related to the authors stating the need for integrated/holistic assessments that address coupled human-ecological systems. They are encouraged in their revision to condense that language and not to repeat this assertion.
- Table 1 is not effective. It is too general and the structure implies that everything on similar rows is related.
- Tables 4 and 5 and Figure 5 are fairly generic and more appropriate for a textbook.
- Figures 1, 2, and 4 all show basically the same concepts. They could be combined.
- Figure 3 summarizes a vulnerability assessment, but not really a risk assessment due to the lack of discussion of conditional likelihood of event occurrence and asset damage.
- Figure 8 – what are the ranges on the various bar graphs?
- Figures 11 to 16 seem more appropriate for a project report written for local officials. This level of detail may not be needed for international readers not invested in El Salvador issues.

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