Integrated Tsunami Vulnerability and Risk Assessment: application to the coastal area of El Salvador

P. González-Riancho, I. Aguirre-Ayerbe, O. García-Aguilar, R. Medina, M. González, I. Aniel-Quiroga, O.Q. Gutiérrez, J.A. Álvarez-Gómez, J. Larreynaga, F. Gavidia

Author's response to Anonymous Referee #2

First of all we would like to thank this Referee for accepting to review this paper and for the constructive comments provided.

According to the suggestions made by the Referee, we have carried out an extensive revision of the paper and we proceed in this document to answer all the comments. As many of them required extensive, explanatory and detailed answers, the referee comments are presented in black bold font followed by the authors' answers in blue font.

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 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.

Following the Reviewer's suggestion, the revised manuscript has been greatly condensed, specifically the text regarding the literature review and methodological aspects. The discussion of the study area has been reframed in order to focus on lessons learned from the experience. The objectives of the paper have been consequently adapted to the revised document and scope.

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.

Following the referee suggestion, the literature review and methodological sections have been greatly condensed in order to provide more space for the justification of the indicators and other aspects. Literature on tsunami risk assessments has been maintained.

The wording of the paper has been revised to better express the purpose of the work, as the main expected contribution is to provide a straightforward method to facilitate the implementation of concepts provided by existing theoretical frameworks and approaches such as the MOVE framework (Birkmann et al., 2013), Turner et al. (2003) or the BBC conceptual framework (Bogardi and Birkmann, 2004; Cardona, 1999, 2001). The implementation of these theoretical concepts to case studies is sometimes complex due to site-specific problems, lack of data or the lack of information about particular methodological aspects.

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).

A justification for the components of the model is provided in the revised paper (Section 2), paying special attention to those that are slightly different to the ones suggested by official definitions, i.e. risk, vulnerability and resilience, the revised paper being consistent with these definitions. More precisely, regarding risk and vulnerability:

(i) Risk is expressed in terms of the specific impacts analyzed (e.g. loss of human lives), and depends on the characteristics of the threat (e.g. flooding), the exposure of the studied elements (e.g. people in urban areas) and their vulnerability (sensitive groups and resilience).

(ii) The vulnerability conditions are here understood to be of two types, internal (unchangeable individual conditions, such as the age of the population) and external (changeable community conditions, improvable through learning and experience, such as the risk preparedness within the communities), the improvement of the latter being a possible countermeasure to reduce the vulnerability of highly sensitive areas. Accordingly, *sensitivity* refers to the intrinsic characteristics of the exposed elements that make them potentially affected; while *resilience* is the ability of a system, community or society exposed to hazards to resist, absorb, accommodate to and recover from the effects of a hazard in a timely and efficient manner.

Concerning risk mitigation, if risk depends on the hazard and vulnerability conditions of the exposed elements, and the tsunami hazard magnitude, frequency, location, intensity, duration and probability cannot be modified, the only option available to mitigate the risk of some areas must be by reducing the vulnerability of the exposed elements. The authors believe that advances in the understanding of the vulnerability conditions can help reducing the risk of tsunami prone areas. The authors do not find any

contradiction with Sarewitz et al. (2003) as, according to them, *reduced vulnerability always means reduced outcome risk, but reduced outcome risk does not always reduce vulnerability*. Besides, he states that *is politically difficult to justify vulnerability reduction on economic grounds* and provides the example of an earthquake event where *the insurance industry believed that an unfunded mandate to cover catastrophic losses in the future –i.e., hypothetical losses- was more politically palatable than a mandate to pay real dollar, in the present, to reduce those hypothetical losses.* The authors agree with this assertion. Sarewitz et al. (2003) highlights that *vulnerability reduction can be sometimes in conflict with policy, politics and economics*, in terms of insurance for example, but not in other type of initiatives such as community information and awareness, designing of evacuation plans or developing and early warning system. Assessments like the one presented here permits to identify different management options; however, what the decision-makers choose according to policy, politics and economics is out of the scope of this work.

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 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.

According to this comment, the wording of the paper has been revised to better express the risk concept, assessment and results of this work.

The authors acknowledge that the fact of using a deterministic analysis does not allow providing the risk results in terms of a probability of negative consequences; instead it permits the identification, location and quantification of the **expected negative consequences or impacts for the worst possible credible scenario** (as carried out by Jelínek et al., 2009, within the TRANSFER Project). These results are common in risk assessments and, despite not offering information about probabilities, provide very useful for decision-makers to organize and manage the risk according to the most protective situation.

The calculation of the extent of the negative consequences (damage levels) varies in this work according to the available information and methodologies, not being defined in this work in a homogeneous way for every dimension or exposed element. The expected human impacts has been analyzed based on the tsunami drag (understood as the likelihood of human instability based on the water depth and velocity) considering the ranges proposed by Jonkman et al. (2008). The expected damage in buildings has been developed according to the materials and the water depth (based on SCHEMA methodology). The expected damage in the various types of ecosystems, infrastructures and socioeconomic activities has not been calculated, instead the exposed ecosystems and socioeconomic area as well as the precise location of the exposed infrastructure are provided together with the different hazard information (depth, velocity, area, runup, etc). The information provided is the following: (i) area and location of ecosystems and related ecosystem services that would be affected by a potential event, as well as the local communities depending on them; (ii) area, number of jobs and economic contribution to be lost for the different

socioeconomic activities exposed to the hazard; and (iii) number, location and percentage of specific infrastructures that would be somewhat affected due to their presence in the exposed area. These local analyses, although slightly different if format and scope, provide essential knowledge for risk management and the formulation of adequate risk reduction measures.

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 authors acknowledge the relevance of the concepts of coupled social-ecological systems and related feedback loops suggested by the Reviewer, which have been explicitly included in the revised paper. Regarding the comment about the holistic assessment, partial vulnerability results (for each dimension) may not require the assessment of the other dimensions (as the example provided by the reviewer, stating that minimizing life loss from future tsunamis through evacuation training doesn't really require an understanding of the habitat health or GDP exposure). In other words, individual risk, hazard and/or vulnerability assessments and management can be partial, sectoral or specific. However, risk management requires, in our opinion, an integrated and holistic understanding of the coupled human-environmental system dealt with; otherwise management options can produce unexpected and sometimes undesired results. The authors believe so as focusing only on minimizing life loss could lead to a lack of preparedness on the social and economic impacts after the event the will greatly affect the population, for example the one associated to the destruction of the livelihood-related natural resources or the lack of economic resources at the national level to support the affected areas in the very long-term. The importance of the integrated approach is exactly this, understanding the global system to reduce the vulnerability in a sound way without under-considering the resources and community development capacities. The more information is collected and assessed the best management options can be applied.

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.

According to this comment, the literature review and methodological sections have been greatly condensed in order to provide more space for the justification of the indicators and other aspects. Information about the generation of the set of indicators has been included in the revised paper, justifying the aim of each single indicator, data sources and classification method. Index creation in the literature has

been greatly condensed. The definition of variables is usually a trade-off between the desired assessment and the available information, this tradeoff has been applied in this work.

Advantages and disadvantages of the mixed indicator approach have been identified and included in the revised paper. Vulnerability focuses on the expected impacts by municipality on the different dimensions and their potential worsening implications for the populations due to existing feedback loops (for example, the loss of household income due to loss of livelihood-related natural resources, the loss of recovery capacity of the country due to the loss of area of specific socioeconomic activities, or the lack of long-term water resources due to the affection of coastal wells, among others). This is the main justification for the mixed indicator approach. One could think that a partial human analysis is enough for reducing life losses for example; however, understanding all the potential implications of a tsunami event in a specific area (human, environmental, socioeconomic impacts) will help in promoting awareness and preparedness for example. On the other side, this global understanding of the system has the disadvantage of sometimes resulting into a shallow analysis on some of the impacts analyzed.

With regard to the resilience variables, the 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?

According to **Cutter et al (2008),** *there has been a noticeable shift in the rhetoric about hazards, moving from disaster vulnerability to disaster resilience, the latter viewed as a more proactive and positive expression of community engagement with natural hazard reduction*. She also defines resilience as *a system's capacity to absorb disturbance and re-organize into a fully functioning system*, *including not only the capacity to return to the state (or multiple states) that existed before the disturbance, but also to advance through learning and adaptation (Adger et al., 2005; Klein et al., 2003; Folke, 2006).* She also distinguishes between *pre-event and post-event phases through preparedness and mitigation initiatives,* this approach being consistent with the work carried out in this paper. According to Cutter et al (2008), *rapid onset events such as hurricanes and tornados require an immediate response and the time for change or modification in behaviors and practices in the preparedness (pre-event) or post-event (mitigation) phases. In this context, some indicators of resilience could be community evacuation plans*. This approach, in the context of tsunami hazard, can be translated into evacuation plans, evacuation shelters, early warning system, etc. The kind of resilience assessment applied by the authors perfectly fits in the social and institutional resilience proposed by Cutter et al. (2008). According to her, *social resilience can be increased through improvements in communications, risk awareness and preparedness, disaster plans, the purchase of insurance and the sharing of information to aid in the recovery process*. *Institutional resilience* includes the following indicators: *hazard reduction programs, hazard mitigation plans, emergency services, emergency response plans, etc*. The other resilience dimensions (*ecological, economic, infrastructures, and some social indicators*,) are covered in this work through the vulnerability assessment, which make sense within the Cutter's DROP framework, as *the antecedent conditions (social systems, natural systems and built environment) can be viewed as a snapshot in time or as statistic state*.

The indicators proposed in this work perfectly fit within Cutter's et al framework even if calculated through qualitative data.

In addition to this, the variables selected by the authors (information and awareness, warning and evacuation, emergency response, etc.) and the approach applied are consistent with the resilience framework proposed by the **US IOTWS program** in the document *How resilient is your coastal community? A guide for evaluating coastal community resilience to tsunamis and other coastal hazards, US IOTWS Document No. 27, 144 pp., Bangkok, Thailand, 2007*.

The authors agree that the resilience assessment is based on subjective qualitative information as it is based on questionnaires. However we believe that qualitative information can also be used for vulnerability indicators when no quantitative data exist. An analysis of a single municipality maybe does not require to create a resilience index (i.e. numerical); however, when a comparison between municipalities is required (which is the aim of the national assessment) the resilience index seems to be a possible approach to have a general idea of the state of each municipality in terms of their preparedness and emergency management. The results at the national level allow understanding in a general and preliminary way the main weaknesses in emergency management, in order to design further detailed analyses to propose weakness-oriented site-specific corrective measures. Besides, in case of contradictory answers ("yes/no") the intermediate value ("partially") has been finally assigned, the incoherence between authorities' and society's perception about the preparedness of the municipality being automatically identified as a critical issue for resilience improvement measures.

Regarding the justification based on the literature or stakeholder engagement for the questions being asked, the resilience questionnaire applied to understand the resilience level by municipality is based on the assessment of the level of ICZM implementation in Europe, proposed by Pickaver et al. (2004) and carried out through a questionnaire with three possible answers (yes/no/no answer) against each ICZM action and for three spatial levels. This simple approach permits to identify the main existing gaps in ICZM implementation and a trend through time.

Finally, it is unclear how figures 11 and 12 vary in terms of vulnerability calculations for national and local assessments.

Regarding the comment about Fig.11 and 12, we do not fully understand the question. Fig.11 shows the risk results at the national scale (hazard, vulnerability, risk) while Fig.12 shows some examples of hazard results at the local level (wave height, water depth, drag). Therefore, both figures don't vary in terms of vulnerability calculations for national and local assessments because they are not comparable. Nonetheless, the revised paper does not include these figures according to the suggestions of this Reviewer #2.

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.

The definition, references and explanation of the sensitivity term has been clarified in the revised paper in order to avoid the suggested misunderstanding. The difference between demographic or individual sensitivity of the exposed elements and the sensitivity results of the municipality has been clarified in the revised paper, the results being better expressed to be consistent along the document (the obtained result is not that *a community is more sensitive than others because of the presence of mangroves,* but that *a municipality is environmentally more sensitive than others due to a bigger extension of mangroves* ***exposed to the hazard***).

The sensitivity assessment focuses on the **expected impacts by municipality on the different dimensions and their potential worsening implications for the populations due to existing feedback loops** (for example, the loss of household income due to loss of livelihood-related natural resources, the loss of recovery capacity of the country due to the loss of area of specific socioeconomic activities, or the lack of long-term water resources due to the affection of coastal wells, among others). What we are measuring at the national level is the vulnerability of each municipality regarding the different dimensions to understand the expected human, environmental, socioeconomic and infrastructural impacts.

Accordingly, the Environmental Sensitivity indicators (S7-S10) aim to assess the potential **environmental impacts** by municipality in terms of loss of ecosystems and the subsequent loss of livelihoods-related ecosystem services. Thus, the loss of relevant ecosystems (S7, S8, S9), the potential permanent destruction of ecosystems (S10), and the loss of livelihood-related ecosystem services such as coral reefs and mangroves (S8) is assessed. The potential capacity of mangroves to mitigate the hazard is included in this work through the hazard assessment as a higher roughness coefficient was assigned to mangrove areas,

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 exposed 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.

According to this comment, additional information to connect risk assessment and management is included in the revised paper. Table 7 has been removed.

Regarding the example provided by the reviewer, of course evacuation planning as well as other type of measures can proceed without information, but the more information is collected the best management options can be applied. One can propose evacuation routes and shelter areas, but knowing the evacuation speed of your population (which can depend on the age, disabilities etc.) will allow you modeling the evacuation in order to identify critical areas where people would not be able to reach the safe area before the tsunami reaches the coast. Knowing where your sensitive population in terms of evacuation is located will allow you planning alternative measures for them.

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.

This sentence has been removed as both reviewers didn´t agree with it.

– 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.

The text has been greatly condensed to avoid redundancy.

– Table 1 is not effective. It is too general and the structure implies that everything on similar rows is related.

Table 1 has been removed

– Tables 4 and 5 and Figure 5 are fairly generic and more appropriate for a textbook.

Table 4 and Figure 5 have been removed.

Table 5 cannot be removed as it shows the precise structure of the risk assessment carried out in El Salvador, including every single aspect analyzed.

– Figures 1, 2, and 4 all show basically the same concepts. They could be combined.

Figures 1, 2 and 4 have been 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 3 about the risk matrix has been removed.

– Figure 8 – what are the ranges on the various bar graphs?

There was a mistake in that figure. The revised paper has the correct version. The range is 0-1 for every figure as they represent the indices values.

– 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.

Figures 11 to 16 show the case study this Reviewer suggests the authors should focus on. Some of the have been removed in the revised paper according to this comment, although others have been maintained in order to present some results of the case study.