

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 #1

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The paper is very relevant to NHESS and presents new data from a case study area in El Salvador. Conceptually, although the authors note that an improved framework is presented, the advances are modest (see further comments below). The authors have gone at length at looking into the vulnerability and risk literature, but have omitted the concept of coupled social-ecological systems in their analysis, thus not accounting for important feedback loops that operate between subsystems and process at various scales, which is a limitation of the classical approach that they used. It would be important to justify this in the text as social-ecological systems (as coupled systems) are central to most contemporary risk and vulnerability assessments (you also talk of

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systems in page 2885).

The approach used is classical and I am not certain that it is an advance per se from existing frameworks. The framework in Figure 1 is, from my perspective, rather simple, e.g. when comparing it to the Turner et al framework reviewed in this manuscript or the MOVE framework described by Birkmann et al (2013, in Natural Hazards). Feedback loops are not considered as well as e.g. policies enacted outside the place of vulnerability. In this sense the work is close to that of e.g. the World Risk Report or that of Damm (2010 - <http://www.ehs.unu.edu/file/get/8056>). This is not strictly problematic but my suggestion is that the review sections (1 & 2) and the more general methodological considerations (e.g. section 3.2.1 and 3.2.2) could be condensed in order to provide more space for the justification of indicators and other aspects I discuss below.

What is missing in the paper is a justification for the various components of the model. For example, why use resilience under vulnerability as, as is discussed in the paper, some see resilience as the flip side of vulnerability while others consider these as two different concepts? This is a particular interesting discussion point because when you derive indicators (Table 2), these are capacity indicators (coping, recovering). So why use “resilience” terminology and not simply stick to “capacities” which is also used in other frameworks? I appreciate that resilience is a trendy word/concept, but in this case, I do not think you are really talking about resilience (in the sense of e.g. the resilience alliance or even of the UNISDR definition you quote on page 2890).

As indicated above, regarding the spatial dimension, what is missing in my opinion is the consideration of factors shaping risk at various scales, e.g. policies at the international or national level increasing (or reducing) the vulnerability of communities at the local level (cf. the Turner model, for example).

With respect to environmental sensitivity, I am not sure I understood the argumentation completely. In table 5, you address losses of protected ecosystems, etc. Looking at impact variables in a vulnerability assessment is problematic – indeed from Table 5

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and from your discussion on this specific section (you mention damages to mangroves, to specific species), one could infer that a simple policy measure to reduce the sensitivity in the environmental sphere would be to cut all mangroves and eliminate the various endangered species. I am of course here being provocative, but an area with cemented beach front would turn out as less vulnerable (after aggregation) than one with mangroves, if I followed your assessment correctly. However, I might have missed something important in your argumentation and misunderstood your analysis. When considering the environmental sphere, I would infer on the contrary, that an area with mangroves might reduce the overall social-ecological system vulnerability (I admit this is debated scientifically) even if the mangrove itself is destroyed in the process, which bring me back to the lack of “systems thinking” for the proposed framework. In the end we do not know who’s or the vulnerability of what you are measuring.

Another aspect that should be discussed in the paper is that of uncertainty and critical assessment of the approach. These indicator based approaches are very useful, but have also some shortcomings which need to be discussed. An example is the need to quantify everything – this is particularly evident when you compute resilience (Table 6) and provide a resilience index on what is basically qualitative information. The approach also assumes that we are considering some sort of continuous domain whereas I would argue that risk assessment might actually consist of discontinuous functions. Taking resilience as an example, you might have a situation where municipalities would say they have everything you propose in Table 6 underway, except e.g. early warning systems and evacuation routes. Yet, the municipality would end up with a “good” resilience score, whereas it is clear that lack of tsunami early warning and evacuation routes put communities at extreme risk with respect to this hazard, regardless of anything else being done positively in other domains. These limitations should, in my mind, be critically assessed in your paper.

Specific comments:

P2884: the sentence “but they nonetheless represent a greater threat than earth-
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quakes, hurricanes and tornadoes” needs to be justified. I do not necessarily agree with “greater”, in particular when considering earthquakes. Furthermore, tsunamis are often triggered by earthquakes, as you note in the paper.

P2887: I suggest you abstain from naming all the frameworks, particularly since these are extracted from Birkmann (2006). A reference to this review paper would be sufficient, in my opinion.

P2887: you mention that several gaps in science have been identified. Please indicate by who (reference).

P2888: I would replace “bewildering” by “large”.

P2893 L15: what do you mean by “climate change hazard”?

P2893 section 3.1: do you consider modeling uncertainties and if so how? Also, I suggest you avoid using “shall”.

P2896 L3: I do not think that you mean that your indicators were selected based on the general OECD guidelines step by step procedures. This raises the question as to how you decided on the proposed set of indicators presented in Table 2 – this is not discussed at all in the paper. Did you consider indicator cross-correlation? It seems to be the case, but there is no evidence for this in your paper.

P2896 L24: who was questioned through the questionnaire?

P2897 section 3.2.2: this section is too detailed and reports already exist for these step descriptions. I suggest you reduce the length of the explanations by simply providing the adequate references.

P2900: you could mention explicitly ecosystem-based measures within you non-structural measures (you have them in Table 4). These are critical in coastal settings, and brings me back to the issues related to environmental sensitivity discussed above.

P2902 L16: replace “affection” by “impacts”

P2904: I would suggest you replace “are highlighted” with relevant sensitivity qualifiers. L23 you mean “human sensitivity graph” or “social dimension graph”? Same comment for “environment”.

P2907 and Figure 15: you provide here quite exact figures on economic losses and it is not clear from your paper how you achieved this.

Figure 2: I would suggest you explain all the symbols in the caption).

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