

## ***Interactive comment on “Review Article: A Comparison of Flood and Earthquake Vulnerability Assessment Indicators” by Marleen C. de Ruiter et al.***

**Anonymous Referee #2**

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The article tries to highlight insights how earthquake and flood vulnerability indicators can be improved. This is generally laudable, to improve both types of indicators by achieving more integration and learning by examples from each other.

Overall this article is a bit problematic. It is a little worrying that it reiterates certain limited visions of vulnerability indicators and formula, focusing mainly on physical and exposure aspects, especially in the beginning of the article. The literature used is quite narrow for certain fields such as local level studies or social, economic or institutional vulnerability (and resilience) and while the article claims to be a review, it is quite limited in scope and missing insights from similar review approaches.

Content The authors distinguish social vulnerability into four groups. It is questionable

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to put economic indicators under social vulnerability. The examples and reasoning provided come too short and examples for instance for institutional indicators are not fully convincing.

Some chapters like these or 2.2.3 are so short that the impression remains that they could rather be skipped. Three lines about the aspect of scale under a heading are not sufficient, especially, the reference and thorough discussion and link to several indices discussed before, is lacking. Moreover, the function of chapter 2.2.3 is unclear, since in chapter 3 those aspects are discussed (again) in much more detail.

In terms of argumentation, the paper and logic of language is often hard to follow; certain contradictions seem to appear. For example, in lines 285 ff. There are rather unsupported claims that building codes have not been observed in flood vulnerability studies. What does this include? Building codes for earthquakes? Or specific design codes for physical stability against flooding? Do such standards exist? Which ones? And have they really not been analysed? But this is just an example of the argumentation style in this paper; claims made within one sentence and then not detailed anymore or supported merely by one source – in this case one of the authors of this paper and on earthquake not flood vulnerability. Some contradiction is also in this sentence with the following sentence “while for floods Nikolowski (2014) provides an overview” So is knowledge available or not, is a bit unclear.

Text from 285 to 315: well, the authors cited here (from the same institutions as the authors) use earthquake models also in flood studies. But this is not justifying the argument the authors make; that there would exist no flood vulnerability indicators that also analyse built environment or road infrastructure or else. In fact, there are even papers out by the same institution that specifically analyse road vulnerability, but are not mentioned here (Keller and Atzl 2014 International journal of disaster risk science) This again underscores the main impression that this article leaves; limited in scope and line of justification as based on own work of the authors and certain colleagues who have a strong focus only on certain aspects of risk or vulnerability. Their focus is

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fine, but this paper tries to be a review paper and should be much more balanced and informed by the diversity of approaches that exist.

State-of-the art: what about other review papers on vulnerability indicators such as Tate, de Sherbinin, or on similar resilience indicators etc. what did they find? What do UNISDR processes at the moment on indicators search for, demand, have achieved? The SREX report of IPCC and similar documents by Cardona and others have substantially contributed a joint understanding of vulnerability indicators on all types of hazards, and earthquake and floods are amongst the most prominent.

Method: it is not clear, how the table cells are justified – it is decisions by the authors to fill these cells and quite many of those appear to be based rather on assumptions and feelings by the authors, what should be emphasised or placed into a box. Is this 'method' the right approach? Some of the authors are really strong in quantitative data analysis or case study approaches – wouldn't it be much more compelling to provide those arguments for better indicators based on real data or on cases?

A theoretical underpinning is lacking as well; the cited work by Bruenau et al 2003 might serve as a starting point or an analysis of conceptual frameworks who tried to structure vulnerability dimensions already and provide insights that physical and social and cultural and economic etc aspects must be combined in indicators. Davidsson and Shah 1997 are a classic; but many who tried to apply it have struggled with the application since physical and social and exposure and hazard are often overlapping; where are the existing lessons learned studies here? A section also about the pitfalls and advances made?

Scientific language and style of argumentation needs major improvement. Sentences such as in line 326 are an example: "However, building age does not appear to be an important vulnerability indicator used in flood vulnerability assessments." They do not "appear to be": how do they come to this conclusion? How exactly is this to be derived from the previous sentence?

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Abstract: "In a cross-discipline study" please name the disciplines later on in detail and explain a bit how there might exist differences in focus.

Conclusion: I suggest a much more balanced differentiation and more caution. Sentences such as "Flood vulnerability assessments have generally used a higher scale of geographical aggregation compared to earthquake vulnerability assessments." are wrong, if they are generalised. A great number of household level flood vulnerability indicator studies exist as do aggregated indices at multi-national level. Overall, the paper runs the risk to be limited in scope to characterise vulnerability assessments per se as physical vulnerability assessments. Maybe it would help if the authors provide a better delineation of their scope – regarding content, ambition, and countries and disciplines covered

Minor comments: Line 54: Source is Davidson and Shah 1997 Line 380: Author is Rufat?

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