

# ***Interactive comment on “Improvement of an index oriented methodology for consequence analysis of natural hazards: application to the Upper Guil Catchment (Southern French Alps)” by Benoît Carlier et al.***

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Comment n° 1: Strictly speaking a real consequence analysis is not performed. It is advisable to stick to the main objective related to the fusion of "physical" and social vulnerability". Otherwise, using consequence analysis as a key term, it is mandatory to analyse in detail the impacts generated by the hazard process and constantly refer to a specific set of hazard scenarios which should reflect the perturbation of the exposed system.

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Response to comment n°1: We have taken the comment into account and addressed the lack of information on hazard type and scenario. The scenario considered is now cited in the abstract (line 18) and in the introduction (line 116-117). Detailed information is provided in section 2.3: “Flood hazard mapping” (line 254 to 268).

Comment n° 2: You claim that “the social approach in which vulnerability is viewed as a combination of socio-economic variables determining people’s ability to anticipate before a catastrophic event, to react during it, and to recover after it”. This statement is confusing, at least to me. First, describing the social approach simply as a combination of socioeconomic variables is rather reductive either in scope and also in epistemic terms. Second, it is not clear what people should be able to anticipate. If people should be able to anticipate the consequences, then it is also essential to analyse the process impacts. Rigorously, this is possible only by mirroring a certain set variables quantifying the intensity of the underlying process. If this deduction holds, then hazard analysis, conceived as an ancillary element of vulnerability assessment from a natural science perspective, comes again decisively into play, perhaps not as the only starting point of vulnerability assessment endeavours but, in any case, as one essential component. In my view the hazard analysis is only rudimentarily addressed throughout the manuscript and I really suggest to reinforce this part.

Response to comment n°2: We have taken the comment into account and developed the description of the social approach (line 66 to 71). However, we don’t understand the second part of the comment. The ability to “anticipate” a catastrophic event refers to risk knowledge, risk memory and more widely, to preparedness. Preparedness is commonly admitted as an essential component of risk management. For us, a resilient population should be able to anticipate a hazard. In accordance with the referee comment, we developed our hazard analysis (line 254 to 268).

Comment n° 3: The last sentence of the abstract reads “GPDl scores are globally lower than PDI scores indicating that resilient population may qualify results obtained for physical consequences” and is meant to summarize the specific insights gained

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through the application of the employed methodology. This conclusion is really general and, at least in my opinion, is only marginally useful for any risk management endeavour. It would be beneficial to provide a more tangible concluding sentence.

Response to comment n°3: We have taken the comment into account and modified the text accordingly. A discussion on the interest of combining social and physical vulnerability together is made in the introduction (line 52 to 81) and the conclusions and perspectives (line 366 to 378). We also developed the advantages and limitations of our method in the “Conclusions and perspectives” part (line 366 to 399). 7

Comment n° 4: In the introduction you state that in mountain areas local communities are particularly exposed to natural hazards due to some characteristics inherent to the physical and socio-institutional environment. Since you build your case upon these characteristics it is necessary to unveil them. The second sentence starts with “This leads to important costs for communities...” Also this sting of argumentation remains unspecified until you do not clearly state the characteristics that determine exposure to natural hazards. To conclude this line of thought I suggest to better convey the reasons that moved you in trying to improve the index based methodology for vulnerability assessment.

Response to comment n°4: We have taken the referee comment into account and modified the text accordingly. We developed our argumentation line 36 to 52.

Comment n° 5: You contend that reducing susceptibility to hazard and create disaster-resilient communities, necessitate to combine the “engineering approach” and “socio-economic approach”. I completely agree with this statement. The problem resides in how these approaches are combined. Personally I think that the engineering approach is rigorous but is only partially capable to assess the broad spectrum of consequences of a natural hazard event. On the other side, the social views on vulnerability tend to dismiss the importance of the frequency, intensity (and magnitude) of the perturbation as well as its spatio-temporal dynamics. Hence, a certain margin of vagueness still

risks to persist. This may be somewhat detrimental for an efficient risk management process. Why compress hazard impact analysis into a set of qualitative indicators if modelling approaches allow for a reliable spatial representation of the hazard scenarios? Hence, to be concise, I'd rather retain the strengths of the engineering view and would prefer to look for ways on how to embed these methods in a broader methodological architecture aimed at contextualizing all relevant and concurrent determinants of social and economic vulnerability. Given these premises I'd like to invite the authors to expand the rationale of their work and explain at their best the suitability of their approach.

Response to comment n°5: The referee comment was taken into account and the text was modified accordingly.

Comment n° 6: The first subsection of the section study area is called “Physical context”. I miss, however, relevant physical information about the mentioned hazard events. It could be insightful to provide a description of the main events and to display the inundated areas. Moreover, I suggest to highlight, through an appropriate set of symbols, the damages caused by, at least, the most severe among the mentioned events.

Response to comment n°6: We have taken the comment into account and addressed the lack of information on hazard type and scenario. The scenario considered is now cited in the abstract (line 18) and in the introduction (line 116-117). Detailed information on the scenario used is provided in section 2.3: “Flood hazard mapping” (line 254 to 268). In accordance with the referee comment, we produced a figure showing the damage caused by the 1957 and 2000 flood events (Fig. 2).

Comment n° 7: Section: Methods and data: I have several concerns in relation to this section: You explicitly state that “in the context of the French funded ANR project SAMCO (Society Adaptation for coping with Mountain risks in a global change Context), you applied these principles to set up a systemic analysis of mountain risk in-

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cluding elements of all the components of vulnerability (i.e. structural, functional, social, economic and institutional). It is not clear to me which are the principles applied. Please describe them clearly. Moreover, you say that you include elements of all the components of vulnerability. In relation to the hazard process, for example, you propose the distance to the torrent as main “indicator”. In my opinion this indicator is rather unsuitable. This choice signals a weakness in incorporating the hazard process dynamics and indicates, as a reflection, a potential failure in successfully combining the different views on vulnerability. It is not intelligible how hazard processes of different intensity affect the built environment. As a consequence, it is not clear to me, how the damage generation occurs and how the direct and indirect consequences could materialize. With respect to the employed methodological apparatus I miss a coherent weighting of the single factors (indices) contributing to the overall vulnerability of the system. Several mentioned criteria concur to determine a plurality of different indices at different hierarchical levels. In aggregating the weight of these indices serious troubles of consistency may arise. More theoretical background on the employed methodology is necessary backbone your methodological structure. In relation to the selected criteria and in particular looking to figure 4, it is not understandable what torrentiality means associated to several criteria (e.g. land-use, land cover, transport and energy systems etc.). To sum up, in its present form, the methodological workflow is rather unintelligible. Please put efforts in significantly enhancing the clarity of the description of the single methodological steps.

Response to comment n°7: The referee comment was taken into account and the text was modified accordingly. In the context of the Samco project we adopted a systemic point of view by studying different elements of the risk system (processes, hazard, risk perception, physical vulnerability, climatic and socio-economic changes etc.). The present paper is only focused on the development of a method to combine social and physical vulnerability. The indicator “distance to the torrent” was deleted and replaced by a new criteria based on the areas potentially impacted by different scenarios of flood (EAR-H area affected by floods; Fig. 3 and 4). This paper is an upgrading of an ex-

isting method. The description of how hazard process affects buildings is discussed in the following paper: Puissant, A., Malet, J.P., Maquaire, O.: Mapping landslide consequences in mountain areas: a tentative approach with a semi-quantitative procedure, SAGEO, 1-16, 2006.; Puissant, A., Van Den Eeckhaut, M., Malet, J.P., Maquaire, O.: Landslide consequence analysis: a region-scale indicator-based methodology, Landslides, 1-16, DOI: 10.1007/s10346-013-0429-x, 2013. We made significant modifications in our method. Re-writing the paper, we made an effort to be more intelligible in the description of our method.

Comment n° 8: In relation to the construction and organization of the SIVI you present a synthesis table of the criteria usually employed, citing several papers where these criteria were proposed and used. I invite you to rigorously check the reference list for completeness. Response to comment n°8: Following the referee comment, the literature review was completed with recent papers.

Comment n° 9: Results, conclusions and perspective: You claim that the organization of the obtained results, and, hence, also their representation form (i.e. maps) is easily interpretable by risk managers or local decision makers. Can you corroborate this statement? What exactly can be deduced better in terms of an enhanced elaboration of risk management strategies? Ideally, an assessment procedure should also provide opportunities to understand how to increase cost-effectively the resilience of a system by design. Your systemic analysis seems to be a bit weak in this respect. What should be done? What should be prioritized? What should engineers know in order to improve their design?

Response to comment n°9: In accordance with the referee comment we clarified the objectives of the study. A first indication on the use of the produced map is given line 116. Other information is given in the “Conclusions and perspectives” part (line 366 to 378 and 395 to 399).

Comment n° 10: In this review, I preferred to prioritize content related aspects to be

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improved. The text contains also several grammatical and orthographical mistakes and it would benefit from a thorough revision by a native speaker.

Response to comment n°10: Following the referee comment, the text was reviewed by a professional translator.

Please also note the supplement to this comment:

<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2017-323/nhess-2017-323-AC4-supplement.zip>

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Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2017-323>, 2017.

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