

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.

Anonymous Referee #2

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Dear Authors,

I have now revised the discussion paper titled: “Improvement of an index oriented methodology for consequence analysis of natural hazards: application to the Upper Guil Catchment (Southern French Alps)”. In this discussion paper the authors seek to combine both methodologically and quantitatively elements and determinants of social and physical vulnerability. This effort is worth being undertaken, but not at the cost of a low prediction accuracy and an obfuscated representation of the cause-effect relations, whose profound understanding ultimately determine the prospects of success of

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any designed risk mitigation strategy. This methodological combination under a unifying umbrella is a complex task and its achievement should be as robust as possible against criticisms related to theoretical, methodological and computational aspects. In the light of this complexity and of the overall quality of the submitted discussion paper, I'm convinced that major revisions are needed. Here follows a set of recommendations:

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 analyze 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.

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 socio-economic 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 analyze 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 endeavors 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.

3) The last sentence of the abstract reads “GPDI 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 through the application of the employed methodology. This conclusion is really general and, at least in

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my opinion, is only marginally useful for any risk management endeavor. It would be beneficial to provide a more tangible concluding sentence.

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 string 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.

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 they approach.

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

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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.

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 including 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.

8) In relation to the construction and organization of the SIVI you present a synthesis

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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.

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?

10) In this review, I preferred to prioritize content related aspects to be improved. The text contains also several grammatical and orthographical mistakes and it would benefit from a thorough revision by a native speaker.

I hope that you find my comments useful.

Best regards Reviewer

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