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Interactive Comment

Interactive comment on "A multi-scale risk assessment for tephra fallout and airborne concentration from multiple Icelandic volcanoes – Part 2: Vulnerability and impact" *by* C. Scaini et al.

Anonymous Referee #2

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The analysis is surely of great interest and worth publishing. However, I think that a general revision and a better explanation of terms and definitions is required, in order to avoid dangerous mis-interpretation of the results and their applicability. In few words, I do believe that the presented analysis represents a very important basic assessment of the risk due to tephra fall, which may help focusing the attention to specific issues to be investigated through more quantitative analyses, before being usable in practice. To clarify this, several major changes are required throughout the manuscript. Such changes are, in my opinion, very important, but they can be accomplished rather easily with careful re-writing.

My main concerns are:

1) The analysis of impact is essentially qualitative. This should be underlined in the different part of the analysis, since it may introduce important biases in the results, which may also affect the effectiveness of long-term mitigation actions. In particular, the vulnerability is not based on quantitative fragility assessments (e.g., Douglas 2007, NHESS), which assess the probability of damage for all potential intensities of the hazard. Only in this way a probabilistic risk assessment is possible (e.g., Cornell and Krawinkle 2000, PEER Center News), since both low intensities (with higher probability of occurrence, but low probability of damages) and high hazard intensity (with low probability of occurrence, but high probability of damage) may contribute to the probability of damage. To select a single critical value for the intensity, and report the probability of exceedance of this threshold is, in my opinion, limited and sometimes misleading (at very least). For example, in section 3.3 it is discussed: "The accumulation of 5–10 mm of ash can produce tephra-induced insulation flash-over, while a > 10 cm fine ash fallout has a medium to high probability of causing electrical network failure ". How such a "medium to high probability" is accounted for here? How would this impact the results?

2) The potential applicability of the results is largely influenced by the fact that the hazard is essentially scenario based and limited to few potential volcanoes, neglecting most of the variability of potential sources (all the volcanoes) and potential eruptions (variability in eruption styles, volumes, etc., for all the volcanoes). This must be stressed, since it may greatly influence the results, and it must be discussed in all the parts of the manuscript in which the applicability of the results for long-term mitigation plans is proposed.

3) In different part of the manuscript, it is underlined the "systemic" character of the analysis. However, the systemic vulnerability analysis here is, in my opinion, very limited. In particular, several systemic vulnerability assessments demonstrate that spatial correlations for damages (induced either by physical and/or functional inter- and intra-

dependencies among components, or simply by the spatial correlation of the hazard) may lead to very important consequences in systems (e.g., Adachi and Ellingwood 2009, Computer-Aided Civil and Infrastructure Engineering). To model such spatial correlations, single realizations of the hazard (that is, single scenarios that, in this case, are represented by single eruptions and single wind conditions) should be systematically analyzed, and then aggregated considering the probability of occurrence of each of these scenarios (e.g., Cavalieri et al. 2012, Earthquake Eng Struct Dyn). If hazard curves (aggregating many potential single events) are considered as input, these spatial correlations are completely lost (e.g., merging all potential wind fields). To neglect this may introduce important biases, potentially strongly underestimating the expected losses. This is particularly important for the analysis of the road networks, as well as, the electric power network. On the opposite, in the manuscript, the "systemic" character is essentially related to the selection a priori of specific components that are important to the system performance, rather the identification of critical components a posteriori as a result of the systemic analysis. This choice is simpler, but again rather limited, and it should be stressed.

4) All the statements reporting probability must include the time frame and/or the conditions under which probabilities are assessed, otherwise they are meaningless. For example, sentences like "The electricity network is the most exposed element to an Askja OES 1875- type eruption, resulting in a 10 % probability of 655 km of the network being impacted" or "Based on all eruption scenarios, there is a 10 % probability of affecting 1–10 km2 of croplands. " do not mean anything, without reporting either a time window (15 yr?, 10 yr?, 10000 yr?) or a condition (in case of eruption at Askja? In case of an eruption at Askja of 1875-type only? If so, what the the probability of an eruption of this type?). Essentially, this problem exists in many sentences of this kind throughout the manuscript.

5) (minor) Most (if not all) the results have a meaning only in a relative sense, since the qualitative character of the analysis makes practically impossible to compare the

risk due to volcanic tephra with the one due to other hazards, in a multi-risk prospective (e.g., Grunthal et al. 2006, Nat Haz). Ultimately, the decision about mitigation actions should be based on multi-risk results, since we are not interested to damages due to tephra, but to damages due to whatever reason. This should be discussed case by case, whenever the applicability of the results for long-term mitigation plans is proposed.

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