

Interactive comment on “Weigh(t)ing the dimensions of social vulnerability based on a regression analysis of disaster damages” by Vincent David Corvin Heß

Anonymous Referee #2

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Summary

The author uses regression analysis to examine the relationship between proxy indicators of social vulnerability and per capita damages in municipalities across the Austrian federal state of Styria. The author then uses the results from the regression analysis to refine a principal components analysis derived social vulnerability index. The author concludes that in order to accurately develop and interpret a social vulnerability index, the index developers need to know the “place-specific influence of all indicators”. The author offers a number of useful insights in the discussion and conclusions of the paper that should have been applied in the analysis and interpretation of this work. I recom-

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mend reconsidering the formation of the social vulnerability index following the advice the author gives at the end of the paper: including reporting place and domain-specific circumstances (i.e., an analysis of the relevant features of the municipalities in Styria). Additionally, the paper requires a clearly bounded operational definition of resilience that aligns with theory and the selection of vulnerability indicators. While, the methodology for analysing the data seems sound the questions and interpretations need to be reconsidered to make this a much more valuable contribution to their field. More specific comments on how to remedy this below:

Specific Comments Page 1, line 21: You have framed the paragraph as tension between concepts of physical and social vulnerability, but this last sentence of the paragraph indicates that there are conceptual differences within current approaches to the way social vulnerability is defined. These are two separate issues and should be clearly separated.

Page 2, line 3: I think what prevents a “direct measurement of social vulnerability” is the fact that it is a social construct and therefore cannot be directly observed no matter how precisely or universally it is defined.

Page 3, line 24: How did the author come to the conclusion that there is no justification to use one approach over the others [weighting schemes]”? Did Vincent, Schmidlein et al., and Rygel et al. not make arguments for the benefits of their approaches? I think equal weighting is an understandable approach, but it needs to be better justified.

Page 5, line 25: The assumption that larger numbers of people in a household reduces vulnerability seem intuitive to me. This could mean that a single income is split between more people. It could indicate crowding.

Page 5, line 27-28: “fire station per citizens” is a response capacity indicator. There is no theoretical justification for why response capacity would increase or decrease exposure to physical damage from a disaster, unless fire personnel had an active role in disaster preparedness or mitigation.

Page 6, line 5: It would be interesting to know if women have the same degree of vulnerability in a developed European country compared to the USA where the Morrow and Phillips study (which is now almost 20 years old). It might be handy to supplement this variable selection with European based studies that have been conducted more recently.

Page 6, line 7: How are “foreigners” defined in this study?

Page 6, lines 23-24: Are the number of applications and pay-outs per capita or per household measures? As there is a significant variation in the population of the municipalities having the raw counts will cause misleading results.

Page 9, line 1-2: “These results indicate that few of the chosen indicators actually determine the differences in social vulnerability.” This observation is not adequately bounded. The results neither confirm nor disprove that the variables influence social vulnerability. The results show that few of the selected proxies reduce a population’s exposure to physical damage. There is no information about injuries, loss of income, the speed to some recovery proxy (e.g., people returning to households or meeting their previous level of productivity or health).

Page 9, line 4: It is difficult to interpret the population density result without knowing whether damages were adjusted for the municipality population. [This is clarified in the discussion Page 12, but should be made clearer in the methods and results sections.] Also, it is unclear if every areas had experienced a damage event within the four year assessment period. Both of these things need to be clarified before the reader can appropriately interpret the result.

Page 12, lines 18-19: The fact that social vulnerability indicators are only validated against monetary damages is a significant limitation of this paper and should be accounted for throughout the selection of the indicators to be evaluated.

Page 13, lines 9: Researchers need to develop clear operational definitions of vulner-

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ability when developing quantitative assessments. There are generally agreed upon general theoretical definitions for disaster vulnerability, but such definitions are not designed to provide adequate conceptual boundaries for specific analyses – this is the job of the research. I suggest the author remedy this issue in their own paper in the first section.

Page 13, line 25: The author should apply this very reasonable advice in this paper. Additionally, the author should clarify the disaster phase of focus and the types of impacts being analysed (i.e., social indicators that capture the exposure to physical damage and social indicators that may exacerbate physical disaster losses before a disaster strikes.)

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