

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

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Thank you very much for your valuable and helpful comments regarding my research article. Your comments provide many helpful insights on where to improve my manuscript to increase its scientific value. I will address each point below and how I plan to improve an updated manuscript to incorporate your suggestions.

## *Summary*

The author uses regression analysis to examine the relationship between proxy indicators of social vulnerability and per capita damages in municipal-

ities 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 recommend 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.

Your main critique stems from a misunderstanding about the research goal of this paper. The goal was not to conduct a social vulnerability index for Styria as adequate as possible. Instead it should compare how common practices to calculate such an index can compare against an index based on real damage data. To enable this comparison we need to use a somewhat limited definition of social vulnerability and resilience that specifically addresses damages.

It is clear from your and the other referees comments that this goal is not clear enough defined in the current manuscript and a revised manuscript must clarify this goal upfront and throughout the manuscript. I will also improve the definition of social vulnerability in the introduction to achieve a clearly bound operational definition.

Page 2, line 3: I think what prevents a “direct measurement of social vulner-

ability” is the fact that it is a social construct and therefore cannot be directly observed no matter how precisely or universally it is defined.

I agree that this is an additional source of uncertainty and would suggest to include the following sentence at the end of the introductory discussion of vulnerability (page 1, line 23f):

“Ultimately, social vulnerability remains a social construct and therefore we cannot observe it directly, but only approximate it with proxy data.”

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, Schmidtlein 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.

That is a good point. While each author justifies his or her decision in some way or the other (sometimes just to compare it to equal weighting), no approach has been shown to be superior to the others. Again, this is in part because of the ambiguous definitions of vulnerability. However, not all of the algorithms are suitable for our later comparison with the RA index. In an updated manuscript I suggest to replace the sentence:

“As there is no justification to use one approach over the others, we opted for the simplest and most widely used algorithm, equal weighting.”

with the following:

“Since not all of these approaches allow us to compare a PCA based with a regression analysis based vulnerability index, we opted for the simplest and most widely used algorithm, equal weighting.”

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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.

Am I guessing correctly, that you mean counter-intuitive? While crowding could indeed increase vulnerability, we followed the more common assumption of a increasing vulnerability with single person households. While this is only partially covered by the average household size, I consider this to be more important than crowding. In an updated manuscript I will, however, add the information that it could also indicate the opposite (e.g. Cutter et al. (2003)).

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.

From my point of view, fire brigades can indeed have an active role in disaster preparedness and mitigation, e.g. by filling sand bags in time before the flood, evacuating people and assets from the ground floor and cellars, pumping out oil tanks, etc. All this can reduce experienced damages. I will clarify this in an updated manuscript version.

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.

Thank you for this advice. I will include more recent studies from a European context that assume higher vulnerability for female population. (e.g. Fekete (2009), Holand et al. (2011))

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

Foreigners are defined as citizens without Austrian citizenship, I will add this information in the updated manuscript.

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.

This is an important point, which we extensively discussed when we designed the research. In my dataset, the data is available per household. Damages were converted to damage per capita to assess the effect on the municipality as a whole, since the social indicators are also on a municipality level. Applications were used as a proxy variable for disaster intensity and therefore not converted to per capita applications. It should reflect the burden on a municipality, regardless if it is caused by stronger disaster events or by higher exposure (in larger municipalities).

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).

This is absolutely true in the current version of the manuscript. This issue will be partially solved by a more clear definition of social vulnerability (see comment to summary). I will also rephrase the cited sentence to

“These results indicate that few of the chosen indicators actually determine the *local* differences in social vulnerability *in Austrian municipalities*”.

Please also note that the phrase focuses on the *differences* and not the overall influence. This is also discussed in Section 5, page 12, lines 23-31. But I will keep it in mind to be emphasize it in an updated manuscript.

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.

Thanks you for this advice. I am not sure if I understand the first part correctly: Since the manuscript talks about per capita damages they must be adjusted to the municipality population. I will include the following sentence in Section 3.3: Damage data on page 6, line 24 to clarify this issues upfront:

“We only analyzed municipalities that experienced at least one disaster over this period. Although we miss some information about municipalities which successfully mitigated any damages, municipalities which are not even subject to natural hazards would otherwise distort our results.”

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.

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I fully agree to this statement. Again, the goal of the paper was not to create an adequate vulnerability index for Styria. We wanted to show that more focus should be given to the selection of indicators. We have shown that this is true for the case of monetary damages. It is very likely that this holds true also for other definitions of vulnerability. I suggest adding the following sentence to the conclusion to state this more clearly (Page 13, line 27):

“We have shown that when we define social vulnerability as vulnerability to physical damages not all indicators of our analysis were suitable. Although this will require further research, it is likely that this holds true for other definitions of social vulnerability.”

Page 13, lines 9: Researchers need to develop clear operational definitions of vulnerability 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.)

I agree to both comments. I will address this issue with an improved definition of social vulnerability and a clearer specification of the goal of the paper (see my main answer to your summary).

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