

Interactive comment on “Social vulnerability of rural households to flood hazards in western mountainous regions of Henan province, China” by D. L. Liu and Y. Li

Anonymous Referee #2

Received and published: 28 January 2016

This article computes a household level social vulnerability index (HSVI) for eleven villages in Henan Province, China. The study is mixed-methods, as the authors use a combination of expert and lay judgment for variable selection, a household level survey as a primary data collection tool, and a principle components analysis (PCA) for the purposes of data reduction. While the development of a household level social vulnerability index is a novel contribution to the literature because of the lack of household focus within vulnerability indicators literature, the article has several conceptual and methodological shortcomings that must be resolved prior to publication.

First, the manuscript does not fully explain the theoretical value of constructing an in-

C3105

dex at the household level, nor do the authors identify an audience for the application of their findings. Demonstrating the novelty and utility of a household level index is of paramount importance, since this focus is what makes the study innovative. Is there a reason why households are more appropriate than the prefecture, county, or township levels in the Henan, China, context? What information might the household SoVI communicate that higher levels would not? Who specifically would find this information useful? Simply stating that “the household is the basic unit of social organization” and pointing out the lack of focus on household vulnerability is not enough. Is the interest in examining vulnerability at the household level purely theoretical, or does an initiative by a local government, the central government, or a non-governmental entity provide the basis for the household focus? Perhaps the household focus is more appropriate considering the autonomy of rural, farming households in this region? As written, the article leaves the reader to speculate. Hence, the authors should clarify the context and provide evidence to show the theoretical or practical value of a household level index.

Second, there are several gaps in data collection and analysis that need to be addressed:

(1) The abstract states that a principle components analysis (PCA) was performed to calculate the weights of the eight input factors. The manuscript should include the results from this PCA (i.e., component names, percent variance explained, loading scores, and details on rotations and eigenvalues cutoffs). Furthermore, PCA is a data reduction technique to decrease the number of factors being considered and simplify analysis by identifying patterns in multivariate data. A table of variables considered prior to the PCA run would make the process more transparent.

(2) In terms of the indicator selection, it is unclear who participated in the process. What do the authors mean by “multidisciplinary specialists?” How many individuals (specialists and farmers) were involved in indicator selection? Was this process carried out systematically through a series of semi-structured interviews, surveys, or focus

C3106

groups? The reader lacks detail at this juncture.

(3) How were local households sampled? Section 3.3 states that 100 households were selected based on local officials' advice. Without further details on selection, this procedure hardly seems systematic or repeatable.

(4) Section 4.2, lines 15-17, make reference to interviews about locals' flood risk perceptions. The reader lacks details on when and how these interviews were conducted.

(5) What is participatory rural appraisal (PRA), and how was this method integrated in the study? The authors reference PRA but do not cite any literature on this method or discuss its procedural steps.

Below are a few minor corrections:

(1) Page 6736, Section 4.1, lines 7-10: The text incorrectly states that the two Cutter et al. studies tested the validity of SoVI during Hurricane Katrina. A 2010 study by these authors did apply SoVI to the New Orleans context, but their social vulnerability index has been used as a descriptive algorithm, not as a validation tool.

(2) Page 6736, Section 4.1, lines 10-14: Correlation between the HSVI and casualty numbers from a recent flood is offered as a means to validate the current index. The calculated Pearson's r of 0.248 establishes only a weak positive relationship, not validation. The authors should consider rewording this.

(3) Page 6732, Section 3.2, lines 19-23: The eight indicators are organized into two groups: family characteristics and what seem to be coping abilities. An additional sentence or two explaining this two-type distinction would be helpful. As written it is unclear why there is a need for these two categories.

(4) Table 1: In reference to the hazard training, in what types of training did residents participate? Please clarify in the text.

(5) Table 1: It is also unclear which indicators came from expert judgment and which

C3107

came from outside literature consulted.

(6) Table 2: A tiered bar graph showing the proportion of high, moderate, and low HSVI with the 'n' superimposed on top of each category would be easier to interpret.

(7) Figure 1c should be remapped at a higher resolution to distinguish the symbol for town governments from the symbol for investigation sites. Labels on town names and water features would be helpful as well.

Spelling mistakes abound within the manuscript and are too numerous to list. Sentence structure and grammar are awkward in several locations: page 6730, line 17; page 6730, Section 2, line 5; page 6732, Section 3.2, lines 18-22; page 6736, section 4.2, line 16-17.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 6727, 2015.

C3108