

Interactive comment on “A global comparison of community-based responses to natural hazards” by Barbara Paterson and Anthony Charles

Barbara Paterson and Anthony Charles

barbara.paterson@smu.ca

Received and published: 23 January 2019

Response to Reviewer 1: Thank you for your review of our paper. We have answered each of your points below. Specific comments

1. The subject of the article ‘A global comparison of community-based responses to natural hazards’ is interesting and relevant to the scientific interests of NHESS [. . .] it seems that no conclusions can be drawn that can be generalized as the authors would like. The main problem is that they attempt to produce indicators of the communities’ response to natural hazards based not on the results but on the frequency of previous studies per response category.

Response: We are not assessing “the communities’ response to natural hazards” using

[Printer-friendly version](#)

[Discussion paper](#)



“the frequency of previous studies.” Instead, we examine the number of communities for which responses are reported across the global set of studies. This distinction may appear subtle but is indeed important. Using content analysis, we are recording descriptions of responses actions, which are then categorised into types and classes. Our data set consists of examples of response actions – each example has taken place in a specific community at a specific time. For each type or class, we then count how many communities have reported this kind of response action. Although the data is gleaned from published articles, we are not counting the number of articles but the number of communities for which response actions of a particular category have been reported. Note as well that we are not attempting to evaluate community responses in a normative sense, but rather we are grouping reported response actions to assess their frequencies, i.e. “what has been done”. We agree with this reviewer that an evaluation of the success of the responses based on studies would be highly uncertain. We will need to provide a more detailed description of our methodology and the dataset to make this clearer.

2. But this way of analysis involves the risk of misleading conclusions, or at least of high uncertainty. Unless we accept that the trend of published studies on community response corresponds to the true trend of the communities’ responses, the results of the article are not solid.

Which is even more difficult to accept because the trend of publications is being examined globally and not for one region. But the number of publications on a topic and area is composed of many parameters that can hardly be taken into account.

The authors in some cases try to explain the number of publications by category and area taking into account relevant limitations, leaving the reader with the feeling that the frequency may not be significantly related to the actual trend.

Response: As we have explained above we are not suggesting that “the trend of published studies on community response corresponds to the true trend of the commu-

[Printer-friendly version](#)[Discussion paper](#)

nities' responses". What we are suggesting is that the sample of community-based response actions that is gleaned from articles is a subset of all response actions. What may be contested is whether this sample is representative. We collected our data from peer reviewed articles. Since the scope of our study is intentionally global, there is no other feasible method of collecting the data, at least not to our knowledge. If the scope of the study was smaller, say regional or national, then it would be feasible to utilise additional or different data sources, such as grey literature or community surveys. But these sources/methods are not appropriate for a global overview. On the other hand, a global study with intensive resources and participation by all nations of the world could gather more extensive data, but this has not happened. Thus we engaged in an extensive comprehensive review and extracted response actions from the articles which we are using as proxies for what is going on in the world. However, as is the case with any review there is potential bias, research and publication is influenced by many factors as pointed out by the reviewer. Also some geographic areas e.g. Africa are under reported, which is also true for Emdat. We are focussing here on analytical categories that, allows investigation of hazard response actions in a similar way as Emdat enables analysis of disaster consequences. We appreciate the insightful comments by this reviewer. In our paper, we will need to add more detail to the methods section and also include these considerations into the conclusions section.

3. The methodology followed for the categorization of responses seems appropriate, even though different topics (adaptation/emergency/recovery responses to hazards) are altogether included in the analysis.

Response: We agree that the different phases of disaster response, i.e. adaptation, emergency, recovery are important analytical categories, but not appropriate for the purpose of our analysis. We explain this in more detail below.

4. I suggest the authors to reconsider their point of view, perhaps looking also at the temporal trend of the response priorities. That is to look at the change in the scientific interest, which could reflect a certain shift in response priorities through time.

Response: This is an interesting suggestion; although a time series analysis is beyond the scope of the present paper, we will consider this for a future analysis of our data.

5. Or, to include other information in the aggregated Tables and the discussion section, e.g. ratios of the number of articles per number of the corresponding hazardous events for each world-region (EMDAT may have such data). This could show the low scientific interest or the low production of articles with such targets. Response: Such a table could be included if it is felt that this would add value to the paper. But again, please note that the focus of this paper is on local communities and their hazard responses, not on the authors' scientific interest as such, or the research described in the articles, which are just the source of the data.

Specific comments

6. The title refers to natural hazards; in the beginning of the Introduction, the reader assumes that the weather-related natural hazards will be addressed in the article, and particularly the ones threatening the coastal regions; in the beginning of Methods, geophysical hazards seem to be also included in the analysis. I suggest this to be clarified in the Intro and the abstract.

Response: We are including earth hazards in our analysis and therefore need to change the sentence in the introduction to “Natural disasters such as floods, storms and earthquakes”.

7. It seems that the authors include in their analysis responses that correspond to 3 different timings with respect to the disaster: Before – during – after disaster responses, which mean: responses to prepare/adapt– emergency response – recovery response. These are 3 different topics and I would expect this to be addressed. Aren't they related also to different attitude of communities against natural hazards? Is this aspect important for the classification of responses?

Response: We agree with the reviewer that timing of responses is indeed an important

[Printer-friendly version](#)[Discussion paper](#)

analytical category. However, we found that this analysis is rather complex. We found that the categories before - during - after do not always correspond with the 3 phases preparation – emergency response – recovery. For instance, adaptation responses can be viewed as actions that communities implement based on past experiences with hazards and in anticipation of future events.

8. Other issues with respect to policy implications have not been addressed: in addition to positive aspects of the responses, did the writers in the review also distinguish negative aspects? eg, emergency responses that led to opposite results?

Response: As we explained above, we do not evaluate any of the responses, but simply record actions that have been carried out, irrespective of outcome. We found that only few studies mention unsuccessful response actions. We decided to nonetheless include these in our sample because we do not have the means to evaluate the success of response actions across all the studies from which we gathered data.

9. In some cases it seems that conclusions may not reflect the tendency of the market. E.g. 3.3: green infrastructure for cities adaptation to climate change is however a growing sector. Could the authors look at the temporal trends of response priorities?

Response: This is an interesting suggestion. However, we feel that market considerations would not add to the analysis we are reporting here, but would be a different study all together.

Technical comments

10. I understand that the number of articles reviewed is very large; could it be, however, provided within a Table, eg having 1 column for the categories, or the hazard type, or the area, and 1 for the references separated by ‘;’?

Response: We would be happy to provide this data. However, including all articles would inflate the references section. Such a table may perhaps be more appropriate as supplementary data.

[Printer-friendly version](#)

[Discussion paper](#)



11. I don't really understand Figure 1 and the percentages written.

Response: As explained above, for each class of responses we are counting how many communities have implemented response actions of this kind. The percentages reflect these relative frequencies.

12. P8,l7: correction: in terms OF the total. . . P9,l6: correction: THEN we conclude. .
. Response: Thank you for these corrections – we will make the necessary edits.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2018-353>, 2018.

[Printer-friendly version](#)

[Discussion paper](#)

