

Interactive comment on “Conceptualizing community resilience to natural hazards — the emBRACE framework” by Sylvia Kruse et al.

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This paper is well-structured and well-written, covering an important topic which is relevant to this journal. The manuscript displays good use of apposite mixed methods, presenting solid interweaving of theory with empirics. The text is somewhat sparse on many of the methodological details, but this approach to the writing is understandable given the nature of the paper and what it aims to represent. The manuscript is also sparse on verifiable and specific results, but again this approach is understandable given the nature and approach of the paper.

Overall, this paper is not a basic science piece with hypotheses-methods-results-discussion. Instead, it is mainly conceptual while being methods orientated, bringing together a large volume of work which is synthesised into an applicable framework.

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The paper is important and successful in this approach, hence is publishable as such. The empirical contribution is especially useful, despite the lack of details, because details are given in other publications cited and because the idea of this manuscript is to meld theory and empirics to develop the framework presented. As such, the empirics contribute what they should to this form of paper.

The main concern with the paper, requiring major revisions, is the theoretical baseline and theoretical approach, leading to questions about the final framework.

The paper explicitly accepts from the beginning that both 'resilience' and 'community' are poorly theorised and are contested concepts. This approach is impressive and helpful, which led to a promising start to the manuscript. Unfortunately, the paper then does not add substance to either concept while perpetuating the retrogressiveness of certain classes of literature which the paper initially (and insightfully) critiques.

For example, for 'resilience' why would the authors 'broadly follow the definition of resilience proposed by the Fifth Assessment Report of the Intergovernmental Panel on Climate Change (IPCC)' when the critical resilience literature points out substantive flaws in this definition and provides ways to improve it? A golden opportunity is available—with the rich data and thorough investigation done in this study—to do better than the IPCC and to build on much better, existing work. The authors do not even cite a seminal paper published in this journal <http://www.nat-hazards-earth-syst-sci.net/13/2707/2013/nhess-13-2707-2013.pdf> The authors do note a few of the key critical resilience publications, but do not acknowledge those publications' critiques of the IPCC and its definitions.

I can possibly guess why the authors wholeheartedly embrace the IPCC approach: because so many others do so. Which is exactly why this component of the paper lacks substance: because so many others do the same. If we do what everyone else is doing, where is the originality and what is the point? When so many critiques exist of the approach taken, a paper such as this one could be bold, applying the alternative

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approaches to the empirical case studies to judge whether or not the critical social science literature on resilience really stands up to empirical scrutiny.

Similarly with 'community', where is the literature by Terry Cannon explaining difficulties with the concept of 'community' in exactly the contexts which this paper covers? Again, I appreciate that the authors acknowledge the theoretical challenges and this is needed. But there is no point in producing a framework when those theoretical challenges are not overcome and are actually reinforced. The point of critical approaches is to do better, not to perpetuate the same difficulties which the authors have explicitly acknowledged.

Even for the statement 'only a few approaches have tried to characterize and measure community resilience comprehensively', which is fair, the authors do not cite the best work available: that of Karen Sudemeier, James Lewis, and John Twigg. When the baseline literature review has missed key material, the paper's aim cannot be entirely fulfilled and, in fact, the paper's approach and framework do not progress the field from what is already published—and published too many times. Conversely, the literature which this paper misses contributes to filling in the gaps which the authors correctly identify and rightly seek to rectify. This situation does not preclude the need for further work, as outlined by this paper, nor does it claim a lack of the gaps which these authors identify. It simply means that no paper can ever be comprehensive regarding citations, so references need to be selected much more carefully than is evident in this manuscript.

Continuing along this same theme, the paper relies on the concept of 'adaptive capacities', but it does not explain how this concept is different or provides any more than the concept of 'capacities'. As it turns out, the concepts which are reasonably well described and quite well applied in the paper are straight from the 'capacities' literature from long before the 2011 citation given—and this manuscript is strong for it. But it is important for any current work to recognise the baseline of literature which came before it. In focusing on adaptive capacities rather than capacities, this paper does not

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do so.

There is useful discussion of 'individual resilience' and 'survivors' perceptions of their own resilience'. This material has been extensively discussed in psychology literature (e.g. Johnston, Lewis, Ronan, Paton), none of which is cited in this paper, apart from one Paton reference to a conference piece rather than to his detailed work in peer-reviewed journals. Interestingly, page 2 refers to this cohort of psychology research but no citations are given. Consequently, its rich history and material are absent from the paper, again indicating that citation choice could be improved.

As such, the 'iterative process building on existing scholarly debates' claimed by the paper and the literature review are inadequate. Substantial revisions would be required on the theoretical part of this paper.

Other theoretical components which require further thought and exploration:

(a) Financial and monetary are seen as being synonymous. This assumption ought to be interrogated.

(b) Phrases such as 'civil protection' and 'social protection' have multiple meanings and debates. The 'disaster management cycle' concept has been eviscerated in the literature with alternatives provided. It is puzzling why the cycle approach is presented without recognising why a almost a generation of literature has been explicitly trying to move away from it.

(c) The term 'transformation' appears sporadically throughout the paper, without definition, without critique, and without acknowledging the critiques of contemporary transformation literature. Perhaps it would be easiest not to refer to 'transformation', because a single manuscript cannot cover everything.

(d) The authors could take more care in reading and applying their own writing. As one example, consider 'It is an ever emergent property of social-ecological and technological systems coproduced with individuals and their imaginations'. Why not use your

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'imaginations' to explain why 'social-ecological and technological systems' should not be separated and why 'socio-ecological systems' is inherently problematic? This is not the only example of jargon subsuming reality, which would be so easy to avoid.

(e) 'Disturbances' is a term from ecosystem science, imposed on social-ecological systems theory discourse. It is not clear that this word has relevance to society in reality, which is one of the critiques of resilience approaches not discussed in the paper.

(f) There is some, but not extensive, theoretical and conceptual novelty in this paper compared to complex adaptive systems theory (although this paper uses quite different vocabulary for similar concepts in complex adaptive systems theory). This is by no means a defence or endorsement of complex adaptive systems approaches. It is querying why the authors would go to such an extent to produce a framework, much (not all) of which is already in the literature, even with different labels—and which many like and many dislike.

In the end, I could probably be convinced that the ultimate result of this paper does not change much. This is especially the case given that the framework produced is meant to be applied and is applied in the paper. But plenty needs to be done in order produce a convincing theoretical and critiquing pathway to this end result.

Regarding the journal's questions:

1. Does the paper address relevant scientific and/or technical questions within the scope of NHESS? Yes.
2. Does the paper present new data and/or novel concepts, ideas, tools, methods or results? Reasonably.
3. Are these up to international standards? Reasonably.
4. Are the scientific methods and assumptions valid and outlined clearly? No, but many details are provided in citations given or else changes are requested in the main comments.

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5. Are the results sufficient to support the interpretations and the conclusions? Reasonably.
6. Does the author reach substantial conclusions? Reasonably.
7. Is the description of the data used, the methods used, the experiments and calculations made, and the results obtained sufficiently complete and accurate to allow their reproduction by fellow scientists (traceability of results)? No, but this is not a concern. These details are provided in citations given.
8. Does the title clearly and unambiguously reflect the contents of the paper? Yes.
9. Does the abstract provide a concise, complete and unambiguous summary of the work done and the results obtained? Yes.
10. Are the title and the abstract pertinent, and easy to understand to a wide and diversified audience? Yes.
11. Are mathematical formulae, symbols, abbreviations and units correctly defined and used? If the formulae, symbols or abbreviations are numerous, are there tables or appendixes listing them? Not applicable.
12. Is the size, quality and readability of each figure adequate to the type and quantity of data presented? Yes.
13. Does the author give proper credit to previous and/or related work, and does he/she indicate clearly his/her own contribution? Reasonably.
14. Are the number and quality of the references appropriate? The number is fine, but not the quality. See the main comments.
15. Are the references accessible by fellow scientists? Yes.
16. Is the overall presentation well structured, clear and easy to understand by a wide and general audience? Yes.

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17. Is the length of the paper adequate, too long or too short? Neither too long nor too short.

18. Is there any part of the paper (title, abstract, main text, formulae, symbols, figures and their captions, tables, list of references, appendixes) that needs to be clarified, reduced, added, combined, or eliminated? See the main comments.

19. Is the technical language precise and understandable by fellow scientists? Yes.

20. Is the English language of good quality, fluent, simple and easy to read and understand by a wide and diversified audience? Yes.

21. Is the amount and quality of supplementary material (if any) appropriate? Not applicable.

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