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Comment on nhess-2021-278

Anonymous Referee #2

Referee comment on "Validating a tailored drought risk assessment methodology: drought risk assessment in local Papua New Guinea regions" by Isabella Aitkenhead et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-278-RC2, 2022

Review on "Validating a Tailored Disaster Risk Assessment Methodology: Drought Risk Assessment in Local PNG Regions"

In this paper, the authors collected, complied, and presented the data of drought hazard, exposure, and vulnerability from various sources to analyze the evolution of drought risk in Papua New Guinea. One of the main points of this research is the presentation and application of a drought risk assessment in a region where droughts have a strong impact and have so far been little explored in this regard. Although this paper has some positive aspects, it needs to be thoroughly reviewed before publication.

This paper is more narrative and descriptive in nature based on potentially biased data related to the different drought risk dimensions. New tools and techniques of data analysis and validation are not evident. Nor is there any innovative approach to data analysis. The strength of the methodology is therefore limited.

There are some key issues with drought definitions, risk framework, the spatial and temporal resolution of the various indicators, and the choice of specific indicators. Much emphasis is placed on early warning systems, but it is not clear how this can be applied, validated, and implemented to these systems. I also had some questions about how the authors reached some of their conclusions about the importance of individual indicators. In that sense, the authors must elaborate more in the improvements that the proposed approach brings compared with the previous methodologies and what is the added value to their inclusion in EWS or todecision makers. Below I mention some general aspects that need to be thoroughly reviewed.

DROUGHT AND DROUGHT RISK DEFINITIONS

The analysis presents two indicators that define droughts from a hazard point of view, but no definition of droughts is mentioned, please note that a drought indicator by itself does

not define a drought event. For example, below what threshold of the indicators is considered the beginning of a dry period, when does adrought end, what is the minimum period that discriminates dry events from droughts, etc. What would happen if the anomalies of one indicator is positive and the other negative or if there is a time lag between precipitation anomalies and vegetation evidence (which is usually the case).

There is a difference between a "static" and a "dynamic" risk assessment. A static analysis identifies hotspots where adaptation or drought management measures should be implemented, and is usually based on a combination of a climatology of events over a reasonable period of time (e.g. exceedance probability or frequency of events), exposure and vulnerability, whereas a dynamic analysis aims to identify/highlight the possible impacts associated with observed events. This manuscript presents a "dynamic" risk analysis, which is correct in this context, but the theoretical framework and implications between a dynamic risk analysis and a static or baseline risk analysis have not been assessed, nor what the advantage of this is over a hazard-only monitoring system. In particular given that the risk indicator does not seem to fall below medium risk levels even in “non-dry periods”.

References are made in many instances to implementation in I-EWS, but no explanation is given as to how this information can be used or implemented by such systems. Risk analysis is undoubtedly one of the fundamental pillars of any EWS, but certain important aspects need to be addressed. For example, how forecasts will be incorporated for early warning, which kind of models (dynamic or statistical modelling), on what timescale (days, weeks, months) and what is the skill of such forecasts.

SPATIAL RESOLUTION AND TEMPORAL COVERAGE

I think this issue places a higher bar on the authors to be clearer about methodological choices, why some indicators were selected (sometimes only refers to the fact that they have been used in other similar regions, e.g. Appendix A), where there is missing data, and the spatial resolution and temporal coverage of the indicators that were chosen (e.g. the authors somewhere mention that data related to vulnerability were not available, this is reasonable, but it needs to be better elaborated). Taking into consideration the dynamics of societal vulnerability and economic indicators used might prevent decision makers to correctly interpret the results. All this information should be explicitly mentioned.

It seems that the decision on the selection of the proxy indicators relies mostly in the previous expert surveys,but further information on this is needed (e.g. references to the surveys, which other indicators were proposed, background of experts). A local level analysis should take advantage and propose a set of indicators really oriented to local vulnerability, main sectors affected by droughts and get to identify the root causes.

In addition, no reference is made to the spatial resolution of the indicators (pixel, admin

units, etc.) or how they were aggregated to regional scale (e.g. using average, median, mode, max, etc.). This is important information that could influence the results.

EXTERNAL VALIDATION AND SENSITIVITY ANALYSIS.

One of the main criticisms of such kind of indices is the challenge of externally validating them. Since you can't observe vulnerability directly or the processes that contribute to it, you have to use proxy indicators and then hope that the indicators you have chosen and the process for aggregating them ultimately represent something real in the world. One way is to validate that they capture something real is by comparing their results to the patterns of real impacts.

There isn't really a thorough-going attempt to externally validate the map patterns in this paper. There is a comparison with literature, but this is not structured and it is hard to extract even a soft validation from this. A sensitivity analysis should test the decisions made in the construction of the combined indicator, like normalization and weighting schemes, sensitivity of adding or removing single proxy indicators, etc.

What about the years when the indicator gives medium/high risk (almost all) but no droughts have been observed? How is this to be understood? In page 12 the authors state that "The 2017 and 2018 drought risk assessments indicated most provinces as having mild or moderate drought risk levels, thus a drought event is not suspected, and these were likely non-drought years" How is a water manager or policy maker supposed to interpret this information?

ABSTRACT AND CONCLUSIONS

Both sections make broad and often unsubstantiated assertions in the manuscript. A revision of these two sections in line with the results obtained in this manuscript would be beneficial.

INTRODUCTION

Introduction is too lengthy to discern the essence of the study. The most relevant part of this manuscript is section 1.5. Please consider focusing the introduction to this section.

METHODS

Methodology is mainly descriptive. Tools and techniques of data analysis are not explained. For example the procedure adopted for generating maps of hazard, exposure, vulnerability and comnining them to get drought risk needs to be better explained in the manuscript. As mentioned above, more information on how droughts are defined, selected indicators, temporal and spatial resoulcuing, missing data, desicions taken to build the combined indicator, etc. should be detailed in this section.

APENDIX A, B and C, could be condensed in only one appendix (or even one table) as all of them shows different aspects of the same variables.