

The research by Deves and co-authors on an assessment of multi-hazard risks is welcome for a country like Burundi. Burundi is a small country of tropical Africa whose population is particularly exposed and vulnerable to natural hazards; these concerns being on an increase due to the rapid growth of the population. One of the reasons of this exposure is a lack of information on the presence of these hazards. Providing maps that locate these hazards comes therefore as a handy for the stakeholders.

Burundi, like many other regions of the Global South is clearly data-scarce. This scarcity of information is not only reflected on the lack of observations on the hazards (inventory, databases, etc), monitoring (climate, seismicity), but is also due to a lack of ancillary information (e.g. lithology, soils, etc.). In such a case like Burundi, insisting on the data-scarcity context for the hazard risk assessment is therefore a very valid point made by the co-authors of this research.

In this research, the natural hazards that are studied are diverse: flooding, torrential rains, landslides, earthquakes, and strong winds. In addition, a preliminary assessment of the potential effects of climate change is carried out. A specific focus is also put on shallow landslides for which a national-scale data-driven model has been derived.

Overall, this research falls very well within the scope of NHESS. However, when going through it, I have pointed some issues that should be addressed in, I think, a better way. They are listed below. Note that as an expert working on geo-hydrological hazards (landslides, flash floods, gully erosion) in the region, my comments are more specifically oriented towards these processes. For the other hazards, my critical look is definitely less developed.

- **The data-scarcity context and the lack of use of available data and knowledge in the assessments**

Although we are in a data scarce context, there are some available datasets and knowledge that, if not used, the authors should refer to and discuss.

For example, Depicker et al (2020) produced a comprehensive landslide susceptibility map for a region that covers a large part of Burundi. This assessment is based on thousands of landslide observations and has proven to deliver excellent predictive performances. The model is available online free access here: <https://zenodo.org/records/5027004>

Besides that, landslide inventories available online can be found here:

- Shallow landslide inventory compiled by Depicker et al. (2021a) , with more than 800 entries for Burundi
- Extra landslide inventory on a specific landslide event (Deijns et al., 2022): <https://zenodo.org/records/7198322>

For the region of Bujumbura in particular, comprehensive efforts on mapping landslide processes have been done with the location of more than 1000 features (Kubwimana et al., 2021).

For the same region, floods have been documented and analysed in details by Nsabimana et al. (2023).

The relationship between landslides and flash floods has been investigated, either as cascading or compounding processes (Nibigira et al., 2018; Kubwimana et al., 2021; Deijns et al., 2021; 2024).

For earthquake hazard, reference the work of Delvaux et al (2017) on seismic hazard assessment for a region that includes the whole Burundi is also missed.

Vulnerability and risk to hazards have also been studied, either from a local perspective (e.g. Nsabimana et al, 2023) or from a regional one (Depicker et al., 2021b).

The examples I provide here are not an exhaustive list for all the hazards. However, what I want to point is that in its current version, this research misses the point of being framed around the state of the art knowledge in the region. Besides the implication it has for the science it itself, it is also problematic for the confusing message it could bring to the stakeholders. Why should new assessments be made if they are not discussed/compared with respect to current ones? Why should the local knowledge and expertise from researchers based in local institutions, even when available online, not be used?

- **Robustness of some analyses and lack of methodological information**

Once again, I am not able to put a critical eye on all the types of hazard assessment. However, when it comes to landside assessment for which I have more knowledge I have some concern. The analysis is based on a data-driven approach that is calibrated from an inventory of 770 landslides. The authors say that this information is from Nibigira et al (2013 – a non peer-reviewed information that cannot be accessed) and from Broeckx et al (2018). The PhD thesis of Nibigira (2019 - <https://orbi.uliege.be/profile?uid=p125344>) shows that he has mapped a total of $94 + 338 = 432$ landslides over two well constrained regions of Burundi (see page 66 of the thesis). The data by Broeckx et al (2018) provide 204 entries for Burundi. We are therefore not having a total of 770 landslides. In addition, Broeckx et al (2018) and Nibigira (2019) contain also deep-seated landslides. Furthermore may entries in this dataset include mass movements associated with large gully features and with river bank erosion. These processes, in addition to not being landslides, are also strongly associated with human activities in the region (Dewitte et al., 2021; Kubwimana et al., 2021). Lastly, we shall also keep mind that the dataset of Broeckx et al (2018) is spatially biased towards the city of Bujumbura where image availability and density are higher than in other parts of the country, especially at the time when the inventory was compiled (see Depicker et al., 2021a; Figure 5). The dataset of Nibigira is focussed on only two regions of Burundi, which also leads to a spatial bias in the analysis.

Research in the region has shown that landscape rejuvenation due to the presence of migrating knickpoints associated with the rifting faults plays a major role in the distribution of the landslides. This is demonstrated at regional (Depicker et al., 2021a; 2024) and local levels (Kubwimana et al., 2021) for different types of landslides processes, whether from purely natural origin or from conditions associated with human activities (e.g. deforestation). Such influences of the rift is not even mentioned in the manuscript.

For the temporal analysis associated with landslides, reference to existing assessments with respect to landslide mobilisation rates (Depicker et al., 2021b, 2024) and rainfall thresholds (Monsieurs et al. 2019a, 2019b) would be welcome.

The climate change analysis was carried out with a rather straightforward analysis. I am surprised that such analysis is not carried out with reference to the state of the art (for example Souverijns et al., 2016) and the fact that conclusions on the issues of climate change are

difficult to draw in the region due to the absence of relevant data; which often leads to conflicting perspectives (IPCC, 2021).

- **Target audiences**

As stated in lines 40-42: “The results of the assessment were aimed at decision makers, civil protection authorities and other stakeholders at national and sub-national levels to support planning, decision-making and prioritisation of Disaster Risk Management (DRM) investments and activities.”, the motivation of this research is to provide assessments for a specific audience. In that context, I find it a bit strange that the involvement of researchers from local institution is not considered. Usually, as expert, we are usually pleased to be invited to take part to a research where we can bring our own expertise. That would also have been a better strategy to try to overcome this gap between science and policy where the stakeholders are usually barely listened (Gill et al., 2021). Local scientists are certainly better at making stakeholders aware of the problems of natural hazards.

- **Introduction**

The state of the art is missed, especially on multi-hazard risk assessment. It is thereover difficult to position the research beyond a simple case study in the literature. Furthermore, the introduction brings quite a substantial amount of methodological information. Overall, the introduction reads more like a technical report than a research paper.

Note that the introduction points to cascading and compounding hazards, issues that are barely addressed in the subsequent analysis.

- **Study area**

The description of the study area remains very basic. One could have expected for example that reference to the rifting context is mentioned. Rifting is associated with the presence of faults and differences in relief, which has implication for earthquake hazard, local climate, and, as said earlier, landslide hazard.

The DRR context remains very general, relying on EM-DAT disaster data, a database that is known to come with some caveats. No reference is made to the local knowledge about the hazard (see earlier comments and the non-exhaustive list of references provided at the end of my comments).

Figure 1. What data sources are being used for the map? Where does the classification map of the landform come from? What is the used of such an information about the forms?

- **Methodology**

In addition to what I mention above about the landslide assessment, a lot of methodological steps and choices are not clearly justified and the descriptions of the methods are overall too superficial, preventing any reproducibility. Reference to the literature is very limited, hence

leaving the readers with questions about the relevance and reliability about the methodological aspects.

For example, for the climate drivers, no justification is made with respect to the use of the climate models to extract climate information. Why these models? Why not other products? There is quite a lot of literature on climate product comparison (even for the region – e.g. Camberlin et al., 2019; Nkuzimana et al., 2020); something of importance especially with regard to the specific climate variables that are to be used for the hazard assessments.

For the pluvial flooding, the collection of the information is not explained. The authors says that they have an inventory of 64 events. How? For exemple, Monsieurs et al (2018) and Nsabimana et al. (2023) did similar work in the region bringing enough information for making sure that the method can be reproduced.

For the flooding, another example is with this statement “geomorphological analyses based on digital terrain model were carried out in those flood-prone areas that were insufficiently covered by the historical flood information. “ that is made without brining extra information. This is much too vague.

For the vulnerability for the landslides, the authors invoke a lack of literature. Such work in an neighbour environment similar to that of Burundi could be useful (Sekajugo et al., 2024)

- **Discussion**

This part should be a key aspect of the research. However a proper discussion is missed and there is nothing that is said with respect to the existing assessments. What is the added value of this research? What is its use? Where are the caveats?

The authors put an emphasis on climate change, which is of course a valid point. However, the main concerns about the natural hazards in this region are the exposure of the population and the weakness of the management (see for example Raju et al., 2022 that discuss such aspects in general). In addition, the impacts of human activities in the incidence of natural hazards such as landslides and floods (for example Depicker et al., 2021a, 2024) and the implication it has for the risk (Depicker et al., 2021b) are clear. These are points that for such as work would need to also be discussed, especially for a research that it aimed at targeting stakeholders. Overall, in that sense, the contextualized aspect of this research is weak I believe.

To summarize, the authors propose a research that aims to tackle a lot of issues on different hazards, their vulnerability, and climate change related aspects. This is a very ambitious work. However, lack of (i) methodological justification, (ii) use of local knowledge, (iii) discussion with respect to previous work, and (iv) absence of state of the art literature are factors that weakens the quality of this work. In addition, the research shows a lack a connection with its assume target audience. Per say, that is an point that one could understand, especially with respect to the constraint of going in the field (COVID restriction) and connecting with the local experts and institutions. Nevertheless, one would then have assumed a more elaborate discussion on those aspects.

I hope that my comments will be helpful.

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