Reply to Reviewer #2 Comments

We thank the Reviewer for taking time to review the manuscript. The helpful and constructive comments put us in an excellent position to further improve the paper. The text below contains our response in a point-by-point format. To clearly distinguish reviewer comments from our responses, the reviewer comments are indented.

> In this research, the authors investigate the occurrence of two mega gullies and one deep-seated landslide in an urbanised watershed. They show that these three mass movements, although outliers in term of size in the watershed; are processes associated with non-exceptional rainfall events. The failure of water resources infrastructure (WRIF) is shown as playing a key role in their occurrence, exacerbating the influence of rainfall. The contribution of these three mass movements is important in the overall sediment budget of the watershed. In terms of methods and techniques, the research is based on the acquisition of very-high spatial resolution topographic data from UAV and SfM processing and the use of a process-based erosion model.
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> This research that clearly stresses the role of human activities on the occurrence of hazardous geomorphic processes of climatic origin is an interesting topic that falls well within the scope of NHESS. However, at this stage, although this research brings interesting information, it still suffers from weaknesses; which leads me to the conclusion that the material presented here is not ready for publication.

We thank the reviewer for stating the fit of our work within the scope of the journal, and we believe that a revision of the work which addresses several key issues including several excellent suggestions by the reviewer will make it ready for publication and significant future impact. In fact, we have developed a four-part plan to improve the paper: (1) we will clarify our focus on "abrupt" earth surface hazards which occur over a time scale of hours within the periphery of expanding urban areas and as a result of the combined effects of rainfall and water resources infrastructure failure (WRIFs), (2) we will emphasize that rapid-response photogrammetry and structure from motion (SfM) processing is a promising approach to document these abrupt hazard events, and we will add more information about errors and uncertainties as recommended by Reviewer 1, (3) we will add more contextual information (e.g., history of development, climate, presence of unpaved roads) around our observations to enable a richer interpretation of these important data as recommended by the Reviewer, and (4) we will report the ways in which this work informs our understanding about the triggers and processes that are responsible for these "abrupt" hazards.

We especially regret that our original submission did not clearly explain our interest in "abrupt" events, i.e., mega-gullies and landslides that evolve over a matter of hours. This is a very important detail for justifying the importance and timeliness of rapid-response photogrammetry and SfM to document abrupt mega-gullies and landslides. This detail also bears on the originality of our contribution: to our knowledge, this is the first study to provide documentation of abrupt mega-gullies from a combination of rainfall and WRIFs, and only the second study to document abrupt landslides from a combination of rainfall and WRIFs. Since abrupt earth surface hazards in urban areas pose major safety and damage risks, with little opportunity for early warning and emergency response, we believe primary data documenting these events and reporting their triggers is a very important responsibility of the scientific community.

> First of all, the study is rather descriptive and does analyse the role of WRIF in isolation without really questioning the importance of other factors such as overloading, the pervasive leak iof the water system, the latency between the time the environment is built and the slope/erosion process occur, etc. We would welcome deeper analysis with regard to these processes, especially in a timeline perspective, and expect reference to the relevant international literature to support and discuss the observations. For example:
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> *Demoulin, Alain, and Hans-Balder Havenith. "Causes and Triggers of Mass-Movements: Overloading." Treatise on Geomorphology (2021): in-press.*
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> *Lacroix, P., Dehecq, A., Taipe, E., 2020. Irrigation-triggered landslides in a Peruvian desert caused by modern intensive farming. Nature Geoscience 13, 56–60. doi:10.1038/s41561-019-0500-x*
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> *Makanzu Imwangana, F., Vandecasteele, I., Trefois, P., Ozer, P., Moeyersons, J., 2015. The origin and control of mega-gullies in Kinshasa (D.R . Congo). Catena 125, 38–49. doi:10.1016/j.catena.2014.09.019*
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> *Van Den Eeckhaut, M., Poesen, J., Dewitte, O., Demoulin, a., De Bo, H., Vanmaercke-Gottigny, M.C., 2007. Reactivation of old landslides: Lessons learned from a case-study in the Flemish Ardennes (Belgium). Soil Use and Management 23, 200–211. doi:10.1111/j.1475-2743.2006.00079.x*

We thank the reviewer for offering this excellent suggestion, and we are ready to prepare a revision that includes more contextual information about each event. This corresponds to Part 3 of our four-part plan to improve the paper. For example, we can report that the gullies formed where there are water distribution networks but not paved roads, which is a specific phase of the urban transformation in Tijuana that can last 20+ years. Furthermore, the landslide occurred on an older neighborhood with paved roads and therefore can be considered a chronic hazard independent of time since urbanization.

The citation from Demoulin and Havenith was not available to us at the time of our study, but we are pleased to consider it as an example of WRIFs combining with rainfall to trigger landslides. Furthermore, we note that the other studies cited by the Reviewer help to demonstrate contrast between what has been published in the literature and the work we present herein. That is, in our work we document landslides and mega-gullies that are abrupt, occurring over a matter of hours which is must faster than previous studies (Van Den Eeckhaut et al., 2007; Lacroix et al., 2020). Furthermore, the mega-gullies reported by Makanzu Imwangana et al., 2015 and other authors (i.e., Moeyersons et al., 2015; Zolezzi et al., 2018) occurred in very wet environments compared to Tijuana, Mexico. For example, the annual mean precipitation in the Makanzu Imwangana et al 2015 paper is 1432 mm, which is 7 times higher than the observed in Tijuana.

> A second point for improvement would be on the analysis of the DSM information that can help to better characterise the processes and discuss their mechanisms. Here the multitemporal information is only used to derived volume estimates and dimension parameters, while in can reveal much more than that on how a landslide or a gully has formed.

We agree with the reviewer that, in general, DSM information about earth surface features captured over time can help to characterize processes and understand mechanisms. However, this is not a realistic possibility (at this time) for abrupt events which are the focus of this paper. These events occur unexpectedly during storms, and it generally requires several hours to gain notification through emergency services personnel and to travel to the site with the photogrammetric equipment. We have found that by the time of our arrival, the water mains have been turned off and the mega-gully has stopped growing. Similarly, the landslide that we observed was abrupt and motion had ceased by the time of our arrival, limiting documentation to a comparison between a pre-DSM and a post-DSM. However, we are planning to use the DoD map of the landslide to better illustrate the rotational morphology of this feature, and we can expand the discussion of the mega-gullies characterization to document the role of the terrain slope on the depth of the incision.

> The analysis and discussion around the importance of these three mass movements on the sediment budget suffer from data bias. From three observations on a small watershed, general statements are difficult to be made. The authors need to be more nuanced and one would welcome extra information from the regional surroundings, for example on other landslides and erosion processes that occur there. For example, the landscape seems to offer ideal conditions for gully erosion and we can wonder whether the two mega-gullies are exceptional in size as compared to what occurred elsewhere in the city and in less urbanized areas.

First, the authors agree that it is difficult to generalize our results due to the small sample size, and we are committed to a manuscript that fairly presents our observations, and what we can learn from them, without over generalization. Furthermore, we also believe it is very important to report these data irrespective of the number of events based on our systematic monitoring approach, the absence of previous studies that have ever documented events like these, and the potential implications for public safety and risk management. In particular, we note that five years of monitoring was required to document these three events, and as a result of this work we are in a position to report to readers of the journal about the size of these events and the role of WRIFs alongside other factors.

Second, and in response to the comment about what has occurred elsewhere, the authors note that Fig. 7 in the original manuscript showed a quantitative comparison of the WRIF mega-gullies with rainfall-runoff gullies observed both in the study area and elsewhere (Castillo et al., 2016).

Third, we will add a high-resolution aerial photograph (Figure 6 in the revised paper) to provide context of the exceptional size of these mega-gullies compared to rainfall-runoff gullies in the study area, and to help readers visualize how a water supply pipe under an unpaved road can be exposed and damaged by a rainfall-runoff gully:

> An emphasis is brought on the used of SUV and SfM. However, there is not really an novelty here as these techniques are well known and, in this research, it is “just” applied to produce three DSMs over the three study sites. This methodological part should not be given a high importance and not be presented as a research objective in itself. Note also that it is not always clear on how the photogrammetric data were obtained and processed.

We apologize for the oversight on our part with respect to introducing “abrupt” hazards, which motivate photogrammetry and SfM as a rapid-response technology for documentation purposes. In our four part plan to prepare a revised manuscript, emphasis on the rapid-response monitoring approach is Part 2 and improvements to the presentation of photogrammetric data and processing is Part 3. In particular, we will expand upon our description of data post-processing and error assessment in section 4.1as requested by Reviewer 1.

Technical details on how the climatic data and soil modelling data are processed are needed. It is rather difficult to understand clearly how the results were obtained.

We appreciate this comment and would be pleased to add additional technical detail, which was also suggested by Reviewer 1.

The authors have already published several research papers on erosion processes over that study area and it is not always very clear, especially with regard to what concerns the modelling approaches and SfM methodologies, where the novelties are.

Our previous research was directed at characterizing watershed hydrology and soil erosion processes in the context of sediment management and ecosystem protection. That previous work put us in a position to compare WRIF-generated earth surface sediment fluxes to fluxes from rainfall-runoff alone. Secondly, our focus on earth surface hazards presented here marks a major departure from our focus on sediment management and ecosystems. Third, herein we present and include original data and model results that were not previously published.

I have also made some comments and suggestions directly on the manuscript.

We greatly appreciate the numerous detailed comments and suggestions placed on the manuscript. We embrace all of these points and suggestions and would be pleased to address them with a revision. We do wish to respond directly to two specific comments made by the reviewer on the attached manuscript:

Line 249: such comparisons must be considered with care as it focusses on measurement made on very small study areas. One cannot make robust conclusion based on so few observations (hence the potential of being highly biased in the reasoning).

In Line 247-252 we write the following: "This analysis shows that mass movement associated with WRIFs was significant on an event basis. Mega-gully B generated 4,340 tons (Table 2), which is approximately 80 times the area-normalized annual erosion rate for gullies (tons/ha) and 10 times the total sediment generated by other rainfall-generated gullies (Gudino Elizondo et al., 2018a, Gudino Elizondo et al., 2018b). The WRIF-triggered landslide mobilized more sediment than all of the rainfall-based processes combined, while the mega-gullies triggered by pipe failures and hydraulic mining were responsible for 16 and 20% of the total sediment generation across the watershed (Fig. 5)."

In this case, we are reporting direct measurements in the past tense as well as results from a calibrated model, and we are not making a generalization that this is true everywhere or in the future. Nevertheless, we are pleased to very carefully revise the conclusions section of the paper to ensure that we avoid unfair generalizations, and we thank the reviewer for drawing our attention to this concern.

Line 269: such general statement cannot be made on the basis on three observations over a period of a few years and, in addition, for such a small study area.

In line 266-271, we write the following “The small sample size implies a high degree of uncertainty in all of these estimates; nevertheless, these rates of occurrence are far higher than typical design standards for water resources infrastructure in urban areas. For example, large flood control channels are typically designed with a 0.2-2% annual exceedance probability, and smaller drainage systems in urban areas are often designed for 5-10% annual exceedance probability. Hence, WRIF-based hazards observed during this study are many times more frequent (21-60%) than typical design standards for flood control systems in urban areas (0.2-10%) and thus deserving of greater attention for public safety, infrastructure resilience and environmental protection.”

In this case, our paragraph begins by acknowledging the small sample size and potential for uncertainty. Furthermore, there is no generalization here. We report that the observations made during this study are many times more frequent that typical design standards for flood control systems, which is factual. The final issue is subjective – that the high rate of WRIF-based hazards made them deserving of greater attention for public safety, resilience and so forth. However, we would argue that most public safety officers confronting a hazard that was as much as 10 or even 100 more frequent than other more established hazards would agree that it was “deserving of greater attention”, which could mean additional research and/or data collection. Nevertheless, we will very carefully revise the conclusions section of the paper to ensure that we avoid unfair generalizations, and we thank the reviewer for drawing our attention to this concern.