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Comment on nhess-2022-184

Anonymous Referee #1

Referee comment on "Antecedent rainfall as a critical factor for the triggering of debris flows in arid regions" by Shalev Siman-Tov and Francesco Marra, Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2022-184-RC1, 2022

The goal of this research is on the characterization of rainfall that lead to the occurrence of debris flows in arid regions. More specifically on a type of debris flows, here called "short-lived debris flows).

Although the paper is well written in general, I have some difficulties at really understanding some parts of the manuscript. The topic is certainly of interest for NHESS and the research gap on studying debris flows in arid environment is well identified. Nevertheless, this research shows scientific and technical weaknesses that I identified a bit everywhere in the manuscript. They are summarized here in bullet points, followed by more specific comments pointing to issues directly in the text (and also the commented PDF):

- The definition and identification of debris flows is questionable and I would suspect that in several cases the slope failure processes that are studied are not debris flows. In addition, the justification of defining "short-lived debris flow", i.e. quite a singular terminology, is not backed up by a sound support of the literature.
- The authors use an impressive amount of muti-temporal Lidar derived data to map the debris flows from differences in topography. Although this represents surely one robust way to map mass-movements, there is not really a justification about the use of such a sophisticated approach, especially with respect to the fact that there is no proper analysis/discussion associated with the morphometry of the debris flow processes.
- The rainfall analysis of radar-derived data is quite complex to finally say that a few storms have been identified and validated with the use of media/social network information. In other words, the dates of the events could have been found in a easier and more direct way. In addition, overall, the strategy for looking for a potential rainfall candidate should be better defined, especially with respect to the fact the rain gauges are sometimes located on areas quite close to the places of DF occurrence. If a rain gauge station is a few km away from a event, why not using its data instead of information from a radar situated ~70 km that provides data at a 500 m spatial resolution?
- Furthermore, although the authors acknowledge it in several places in the manuscript,

little is said about the problems/challenges of using radar data in the identification of rainfall conditions, temporally and spatially. The authors claim that the products have been validated against rain gauge information, but they remain rather vague on the topic. When one knows that the whole analysis is based on four DF events, this has deep implication on the overall robustness of the work and the discussion that comes out from it. In several places, results to contradict each other (see for example comment in the PDF, line 311).

- Concerning the discussion, there is a lack of analysis on the DF processes with respect to the literature. Since arid environments are clearly understudied, one could expect that rainfall conditions are here analysed with respect to what is known from the literature. That would not only allow to better highlight the originality of the work, but also to better identify some problems in the method (errors and uncertainties of the radar product). Without this connection to the international literature, the "arid regions" perspective highlighted in the title is not present, and, consequently, we remain at a very case-study level.
- Concerning the literature, one would expect that it is also used to back up the definition of antecedent rainfall conditions. There is also some unclear statements about antecedent and triggering conditions. One could further question the fact that the triggering rainfall, that are also measured here, are not discussed.
- Some parts of the text are quite (too much?) descriptive on the "geological-geomorphological" context in which the DF occur. In addition, such information is not used in the analysis and discussion. For example, when it concerns shallow landslides, one key aspect that explains their occurrence is the availability of colluvium. Without it, rainfall will not have an impact (e.g. Dykes et al., 2002; Parker et al., 2016). Such an aspect on colluvium availability could really explain why in some places DF are not observed despite the presence of potentially "good" rainfall conditions (keeping of course in mind the reliability of the latter). This is something I would really like to be discussed.

With all these issues in mind, I doubt that the present work is robust enough to be published in NHESS. I do not say that the study does not show the potential for it, but it needs at least substantial reworking which could be addressed with difficulty in an "simple" revision.

Specific comments

From the title, we expect several things such as "short-lived" debris flows and antecedent rainfall be a focus of the introduction. However, antecedent is mentioned only once, while "short-lived" is not.

From the title, we expect the focus on the arid regions, however, most of the issues related to the study of debris flows in arid regions that are mentioned in the introduction are illustrated with the description of the study area. I would have expected an introduction that better highlights the challenges/novelties/needs to study debris flows in these arid regions.

Lines 53-56 explain the objective of the research. Two key methodological aspects are highlighted: high-resolution topographic models and high-resolution radar rainfall estimates. It is somehow surprising that none of these "technical aspects" are mentioned in the introduction.

Section 3.1 on mapping methods: I have some difficulties at understanding why there is a focus on the use of multi-temporal Lidar-derived DSM to map the debris flows. Lidar data acquired here 4 times (every other year over the period 2013-2019) represent a great source of data for the characterization of the processes. However, such data are not useful to get the timing information of the debris flows initiation that would allow the rainfall characteristics be analysed. The only way to get the exact timing of a debris flow, assuming that at least an one-day accuracy is needed for such a rainfall analysis, is certainly through direct field observation/media/social network (as explained in lines 110-111). Therefore, also the use of orthophotos is not really appropriate here.

Note however, that the processes that are being studied always need a minimum of "geomorphologic" characterisation (size, shape, mobility) and as such, combining very-high resolution orthophotos and Lidar-derived topographic data, is a great plus to achieve this.

Section 3.2:

- Here reference is made to landslide mapping; which sound different from that of debris flows. Rock fall identification is also mentioned. This is confusing to refer to slope processes that are not the focus of the study.
- This is only here that a definition of short-lived debris flows is provided. This is a terminology that is barely used in the literature and one would welcome more insight on the reason why the authors pay a focus on this process differentiation from "normal" debris flows (see also my comment on the introduction).

Figure 2 is the only visual information that allows to see what a short-lived debris is. And here I must admit that I question the processes that are analysed. To me some of the features look more like debris avalanches. This is the reason why more illustrations (as stated earlier) could be needed. My doubts about the characterization of the processes are further confirmed with the description provided in lines 133-142.

Line 122. Larger debris flows (than the short-lived ones) are not included in the analysis while their occurrence is said to be possible. I am ok with that. However, the authors say that the scars left by these DF could have been blurred by subsequent road construction and floods. Hence my question. If large features can disappear from the landscape, what about the short-lived DF? How reliable is the inventory?

Section 4.1. Rain gauge data are used. However this is not mentioned in the objectives where only the focus on radar-derived rainfall is made.

Figure 5. shows that the highest intensities are not necessarily over the group 3 of SLDFs. However, SLDF do not seem to have occurred in the other areas. Figure 6 confirms this.

Line 255-256: for the first time in the manuscript, a definition of antecedent rainfall is provided. One would expect something definition according to what is usually adopted in the literature on DF so that better justification/comparison/discussion is carried out. Line 294, antecedent rainfall is defined as a continuous rain period that ends up at the moment when the potential highest rainfall intensity is seen. Here also, one would need reference to the literature (see for example: . Bogaard, T.A., Greco, R., 2016.).

References

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Please also note the supplement to this comment:
https://nhess.copernicus.org/preprints/nhess-2022-184/nhess-2022-184-RC1-supplement.pdf