

***Interactive comment on “Erosion after an extreme storm event in an arid fluvial system of the southern Atacama Desert: an assessment of magnitude, return time, and conditioning factors of erosion caused by debris flows” by G. Aguilar et al.***

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This is an informative paper that is appropriate to the focus of the journal “Natural Hazards and Earth System Sciences”.

The manuscript contains numerous unclear passages, yet the major points are well developed and thoughtfully analyzed. I recommend that it be published after major revisions.

I have made numerous editorial suggestions within the pdf of the manuscript (attached). A few broader points are noted here.

1) The introduction lacks any statement of background information that would justify the authors' hypothesis that rock strength is a control on the rate of weathering or the generation of debris flows. Lacking that background, this reader was surprised to find that data are collected with a Schmidt Hammer, and that mean and IQR values of Schmidt Hammer data are considered to be potentially meaningful. Previous studies which make these assertions should be briefly described, including clarification of the rock types, climate zones, and topographic characteristics of the catchments from which the previous studies extracted their interpretations.

Related to the lack of background information, the reader does not know whether to treat the result that there is no correlation of SH mean and IQR to the phenomenon of debris flow generation as a surprising result (because it contradicts a body of published knowledge), or instead as a demonstration that the hypothesis was negated here and may likely also be incorrect in other locations.

2) The authors refer in the Discussion, section 4.1, to the generation of debris flows in tributary catchments as "random." I do not think that they have provided data which justify the statement that the phenomenon occurs randomly. In fact, it seems that their discovery that topographic features of the catchments are predictors of the spatial distribution of debris flows suggests that the phenomenon is not random.

3) In section 4.2, the concept of an "uncoupled" landscape is referred to. Nothing earlier in the paper provided an explanation of what the authors mean.

4) The caption to Figure 3 lacks vital information and guidance. At the least, it should be stated that left sides are "before" and right sides are "after". We also need to know whether the general color tone change is a physical evidence of erosion due to the March event, or if it merely indicates different sun illumination.

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Please also note the supplement to this comment:

<https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-239/nhess-2019-239-RC2-supplement.pdf>

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