Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2019-239-AC5, 2019


Natural Hazards and Earth System Sciences
Discussions



# *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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Received and published: 18 November 2019

Dears reviewer and editor, following I include specific responses to your comments and corrections. Attached pdf file is the corrected manuscript include all modifications. Thank you very much for your great contribution in ours manuscript. Best Wishes German Aguilar

Specific Answers to Reviewer 2



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Discussion paper

1. Introduction

P2 Line 24-25: « On the other hand, meteorological stations and fluviometric stations suitable to measure hourly-rain intensity and sediment load yielding respectively, are scarce or lacking in the Atacama Desert.»

Comment R2: «The problem is compounded the lack or scarcity of meteorological ...»

Change: « The problem is compounded the lack or scarcity of meteorological and fluviometric stations suitable to measure hourly-rain intensity and sediment load yielding, respectively.»

P2 Line 28-29: « We have calculated the erosion within an area of 1,500 km2 based on volumes of debris flow deposits measured in alluvial fans after the storm.»

Comment R2: « if this 1500 km2 area is the white box in Figure 1, you should state that specifically.»

Change: « We have calculated the erosion within tributary catchments (whole area of 1,500 km2) based on volumes of debris flow deposits measured in tributary junction alluvial fans after the storm.»

Main comment: ÂńThe 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 knowl-

edge), or instead as a demonstration that the hypothesis was negated here and may likely also be incorrect in other locations. Âż

[Reply] Measurement stations of Schmidt hammer values were selected considering a statistic analysis of lithology, structural context, and geomorphology of the studied area. Details of this work are in the undergraduate thesis of Fredes (2016) (http://repositorio.uchile.cl/handle/2250/140357) (now in reference). In this analysis we take into account a background-review the geology 1:100.000 of Sernageomin (Salazar et al.2013) and geomorphological studies of two Ph.D. thesis of the Universite de Toulouse and Universidad Catolica del Norte (Aguilar, 2010; Cabré, 2019). We include in the corrected manuscript a section “2. Study area” with a background-review of the geology and geomorphology of the Huasco river valley. We will cite in the method section works that benefit from Schmidt hammer measurements to quantify resistance to rock weathering in catchments. Nevertheless, the validation of the Schmidt Hammer is far from being the focus of this paper.

2. Methods

P3 line 18-19: « In these cases, we estimated 1 meter of debris flow thickness on average for each fan based on mean field observations.»

Comment R2: « this statement is clear. But it is not clear what it implies: Is the remaining volume of each fan treated as alluvial sediment that is NOT debris flow material? Or is it treated as an older stage of debris flow material? »

Change: « Based on the fieldwork measurements, a thickness of one meter was considered for the fans whose length and width were measured on RapidEye images.»

3. Results

P4 line 22-23: «In which amongst all, erosion of the upper mantled-hillslopes layer occurred when water concentrated and formed rills or gullies (Fig. 2 and Fig. 3).»

Comment R2: « This phrase is unclear.»

Change: «The most widespread indicators of hillslopes erosion are rills and gullies generated when water was concentrated and confined in streams (Fig. 2 and Fig. 3).»

P4 line 32-33: «Hillslopes or gravitational landslides and rockslides are the main sediment sources that characteristically fill these channels within storm periods and after storm events.»

Comment R2: «his sentence is not clear, and may be out of best order with respect to the paragraph. "Hillslope or gravitational" seems like a strange combination to connect with an "or", one word related to a landform and the other to a force.»

Change: « Gravitational landslides and rockslides of hillslopes the main sediment sources that characteristically fill alluvial channels within storm periods and after storm events.»

P5 line 7-8: «Debris flows that reached the tributary junctions during the March 2015 event were reported in forty-nine outlets out of one hundred twenty-four catchments (Fig. 4).»

Comment R2: «this phrase is ambiguous, and it is important. Each catchment must have an outlet, one would think. So one interpretation of this phrase is that there are 124 catchments, and 49 of those catchments exported a debris flow. However, the reference to "outlets" at one part of sentence and "catchments" in the other part of sentence raises the question of whether one is a subset of the other, or not. The label "ND" appears to signify No Data for about 68 catchments. Yet that adds to the ambiguity, because "no data" is not the same as "we have no debris flow here", because the absence of a debris flow IS data.»

Change: « Debris flows that reached the tributary junctions with the trunk valley and produced deposit greater to 500 m3 of sediment during the March 2015 event were reported in forty-nine of one hundred twenty-four catchments (Fig. 4).The remaining seventy-five catchments did not yield debris flows deposits greater to 500 m3 of sedi-

ments in the tributary junctions.»

P6 line 1: «The inverse correlation is also observed in the percentage of catchments that generated debris flows because the percentage increases with Size Factor while it decreases with the increase of Relief Factor (Fig. 6ab).»

Comment R2: «Note big arithmetic error on table 1)»

Reply: There's no error. Since the percentage is calculated with the basins that share in range of values and not on the total of studied basins, i.e.: Size factor: 18% of catchments with size factor 0.05-0.25 (4 of 22), 39% of catchments with size factor between 0.25-0.75 (30 of 74) and 57% of catchments with size factor 0.75-1.50 (13 of 23). Relief factor: 9% of catchments with relief factor 1.75-2.00 (1 of 11), 33% of catchments with relief factor between 1.75-1.25 (15 of 46) and 50% of catchments with relief factor 1.25-0.25 (31 of 62). We include the number of catchments in the table and change the phrase to: «The inverse correlation is also observed in the percentage of catchments with different range of factors, because the percentage of catchments that generated debris flows increases with Size Factor while it decreases with Relief Factor (Fig. 6ab).»

P6 line 12-13: «Finally, the weighting factor calculated by the PCA resulted in a normalized catchments-clustering is added (Fig. 7).»

Comment R2: «phrase is unclear. Needs a verb somewhere.)»

Change: «Finally, the weighting factor calculated by the PCA resulted in a normalized catchments-clustering. This catchments-clustering is added in a geographic information systems and resultant in a map of susceptibility (Fig. 7).»

4. Discussion

P7 line 23-24: «Recent studies of debris flow generation assessment show that soil moisture and shallow debris-mantled hillslopes failures, during intense and low frequency storm events, are not required to trigger debris flows in arid catchments (Ver-

gara et al., 2018). This is favored by the catchment transport-limited conditions characteristic of arid catchments, where debris entrainment by run-off from alluviated channels can occur from any storm that affects the area (Coe et al., 2008; Kean et al., 2013).»

Comment R2: The next several sentences would be greatly clarified if this phrase specifies whether you are referring to debris flows within channels or debris flows on hillslopes, or both, or neither. Comment R2: this sentence is constructed in a confusing manner. It appears to say, 1st, soil moisture is not required to trigger debris flows (even during a rain event the soil remains dry?). 2nd, it appears to say that shallow hillslope failures are not required (I have no idea how "debris-mantled" fits into the failure statement). Is this a correct understanding of the sentence?

Change: «Recent studies of debris flow generation assessment show that shallow debris-mantled hillslope failures is not required to trigger debris flows in arid catchments during intense and low frequency storm events (Vergara et al., 2018). Hillslope stability is favored by the transport-limited conditions characteristic of arid catchments. Furthermore, transport limited condition favor the storage of sediment in the alluvial channels, where debris entrainment to tributary junction alluvial fans by run-off from alluvial channels can occur at any storm that affects the area (Coe et al., 2008; Kean et al., 2013).»

P8 line 21-23: «So, the high altitude of zero-isotherm during the March 2015 storm explains the great debris flow generation in the studied zone because the area with effective water capture, as well as the distribution and the magnitude of water discharge down system, is great.»

Comment R2: «the content is appropriate, but the sentence is somewhat unclear.»

Change: «So, the high altitude of zero-isotherm during the March 2015 storm explains the great debris flow generation in the studied zone. In fact, greater area with effective water capture resultant in an widespread distribution of run-off in the head-watershed

and higher volumes of water discharge downstream.»

P8 line 26-28: «In this context, the selective activation of tributary catchments and debris supply from channels by run-off during the March 2015 storm depends on the heterogeneous distribution of storm cells and on the hydrological conditioning factors to store sediments during periods without storms.»

Comment R2: «This sentence needs to clarify that the available data leads the authors to hypothesize the two controls stated, even though this study lacks the data most suitable data with which to test the hypotheses.»

Reply: Phrase deleted because the subject is included in the next paragraph.

P9 line 14-18: «This has been evidenced in the Holocene alluvial fan stratigraphy by a number of cohesive debris flow layers interpreted as a result of episodic high-water discharge events registered in the fans of El Huasco river valley (Cabré et al., 2019). Alluvial fans present at the tributary junctions; the highest sediment yield, in volume, during the relatively arid periods in the Mid-Holocene (Cabré et al., 2017, 2019). Therefore, stormy conditions and high sediment discharge at least occurred after 8 ka BP.»

Comment R2: «this is not a sentence in structure, and I cannot understand its message. How are the "number of cohesive debris flow layers" related to the "highest sediment yield"?»

Comment R2: «unclear meaning. Did Cabre et al 2019 provide chronological information which shows that the alluvial fan deposits of interest span the time from 8 to 0 ka? I ask because the previous mentions of age in this paragraph refer only to 8-4 ka, and to Mid-Holocene, not to late Holocene.»

Change: «This has been evidenced in the Holocene stratigraphy of the alluvial fans of El Huasco river valley by a number of cohesive debris flow layers and radiocarbon age, interpreted to result from episodic high-water discharge events during the last 8

ka BP (Cabré et al., 2017). Therefore, stormy conditions and high sediment discharge at least occurred after 8 ka BP.»

P9 line 25-26: «Wherever, a return of 118 years for the storms like March 2015 can be proposed for the southern Atacama Desert during the last 5,500 years (Ortega et al., 2019).»

Comment R2: «Unclear where this 118 year return value comes from, since the previous part of this paragraph tells us that Ortega et al. 2019 reported a return time of 1 event/40 years to 1 event per 210 years. These numbers seem to have nothing to do with a 118 year return time.»

Reply: 118 years take account the average during the last 5,500 years, e.i. 1 event/40 years during the last 1,000 years result in 25 events and 1 event/200 years between 1,000 to 5,500 years BP result in 22,5 events. 47,5 event in 5.500 years result in 116 years as average. We change to: «A return of 116 years for the storms like March 2015 can be proposed as average for the southern Atacama Desert during the last 5,500 years.»

P9 line 34- P10 line 1-2: «The similarity with the long-term erosion rates suggests that erosion rates have not decreased during the last 8 Ma and that very slow erosion results in an uncoupled landscape stablished at least since the Miocene Andes uplift (Aguilar et al., 2011).»

Comment R2: «Similarity of what? If you are referring to the rate inferred for the March 2015 storm, state the rate and the reference to March 2015. The rate given earlier is 1.3 mm/event. Until you integrate this over time (which occurs in a following paragraph), the reader cannot understand the comparison.»

Comment R2: «unclear what is meant by "uncoupled". If this is important, then material in the introductory section would be needed to prepare the reader for this discussion.»

Reply: We refer to the similarity between the erosion rates on a scale of thousands of

years (Aguilar et al., 2014) and the erosion rates calculated by Aguilar et al., 2011 for the last 8 million years (Aguilar et al., 2011). We change to: ««The similarity between erosion rates during the last thousands of years (Aguilar et al., 2014) with those calculated during the last 8 Ma (Aguilar et al., 2011, suggests that long-term erosion rates remain unchanged.»

P10 line 8-9: «Therefore, these two independent proxies of long-term denudation show a great significance of erosion linked to extreme storms like the March 2015 storm.»

Comment R2: «meaning is unclear. The sentence needs to be rewritten.»

Change: «These two independent proxies of long-term denudation show a great significance of erosion linked to extreme storms at scale of 106-104 years.»

Figure 1: « (a) Synoptic maps of daily precipitation during the March 23–26th, 2015 storm in the northern region of Chile (data from TRMM 3B42v7 mission). (b) Topography extracted from a Digital Elevation Model....»

Comment R2: «I don't see much value to the TRMM data, in the context of this paper. And it has been shown that the TRMM approach worked poorly for these desert region rain events.»

Reply: The figure 1a was eliminated.

Fig. 3: «Before and after from optical imagery retrieved from Planet Team (2017) showing gullies evidences after March 2015 storm. Arrows indicate different evidences of erosion processes.»

Comment R2: «caption should be more informative. 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.»

Change: «Optical imagery retrieved from Planet Team (2017) before and after March

2015 storm from . Left images are before the March 2015 storm and right images are after. Arrows indicates different physical evidence of erosion processes due to the event and gully presence after March 2015 storm. The general color tone change merely indicates different sun illumination.»

Please also note the supplement to this comment:
https://www.nat-hazards-earth-syst-sci-discuss.net/nhess-2019-239/nhess-2019-239-AC5-supplement.pdf

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