

Dear Editor,

On behalf of my co-authors, I would like to submit the corrected manuscript entitled “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 and debris flows generation”. We feel that our corrected version after Interactive comments in NHESSD is a valuable contribution to the understanding of the extreme erosion processes in the Atacama Desert. I thank you for the consideration of this work for publication in the journal Natural Hazards and Earth System Sciences (NHESS).

Sincerely,

German Aguilar

#### Specific responses to the reviewer comments

Page 3 line 28: it is quite strange that the first cited figure in the manuscript is figure 9. Please, consider revising the order of figures.

Reply 1: This is a mistake after a change in the manuscript organization of an ancient version. Now is corrected with the change in figure order. Fig 9a => Fig. 2 and Fig 9b => Fig 10

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Page 3 line 30: it is not clear what “this strategy” means; could be referred to the Terrestrial Cosmogenic Nuclides concentrations measurement but it is not clear, I suggest revising the organisation of this part of the chapter.

Reply 2: We reviewed the content of the sentence: “This strategy and the measurement of Schmidt hammer values has been applied in the Morocco Atlas by Stokes and Mather (2015) to characterize the lithological influence in the debris flows generation”. We eliminated this sentence because it is out of context (remain after manuscript re-organization of an ancient version). The scope of this sentence is well located at the end of the methods chapter.

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Page 4 line 7: I am confident that the submitted paper Cabré et al. (submitted) will be published, but I suggest improving the description of the debris flow deposit to make this manuscript more detailed.

Reply 3: Indeed, Cabre et al. (2020) have already published (<https://doi.org/10.1177/0309133319898994>). However, we include a table (Table 1) summarizing the main characteristics of sedimentary facies of debris flow deposits.

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Chapter 2 general comment: the description of the study area is too limited. Readers are not able to know the extension of the area, the name of basins, and other necessary geomorphological information. This chapter should be thoroughly revised and partially rewritten.

Reply 4: We improved the first paragraph of this chapter to provide more physiographic and geomorphological information. We include: area of the study zone, altitudes and other general

characteristics. Figure 1 was modified to show these informations. The detailed topographic, morphometric and geological information of the 124 tributary catchments studied are in appendix. Also, this chapter was fully revised and partially rewritten.

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Figure 1: this picture is presented a lot of information not cited in the text. I do not think that this is correct. Please, consider the possibility to add a chapter with the description of available data.

Reply 5: The information of the Figure 1 now are partially cited in the text of the chapter 2 and mainly in the new chapter 3 titled: Rainfall data and erosion processes operating during the March 2015 storm event.

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Chapter 3: this is the most important chapter of the manuscript, and it presents a lot of problems and limitations. First of all, readers are not able to understand exactly what the authors did. There a sequence of general information about methods and approach, but is not easy to understand what exactly was performed in this area. Did authors use the formula (1) or the ANOVA approach, or both? I suggest considering a workflow that describes better what authors exactly did.

Reply 6: We separate the chapter into sub-chapters for better reading and understanding of the methodology workflow involved.

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Chapter 4.1: this is a general description that has already done by authors in the previous part of the manuscript.

Reply 7: We considered this observation and now the information provided by sub-chapter 4.1 has been moved to the new chapter 3, taking care not to repeat the information. So chapter 4.1 was deleted.

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Chapter 4.2: This is an important point of the manuscript. Volume estimations of debris flows are provided but in the chapter "method" there is not a real description of the accuracy of the presented method. Even in this chapter, volume estimations are presented without an explanation of the accuracy of the method and a definition of the tolerance of the evaluations. The definition of volumes expressed in meters without a range that describes the tolerance of the estimations is a big limitation of this paper.

Page 6 line 16: The sentence: "The propagation of errors entails an uncertainty of 10% for the volumes calculated with field measurements and 40% for volumes estimated by satellite image measurements" cannot be considered sufficient for the definition of uncertainty. A better description of the procedure adopted for the definition of these values should be provided by authors.

Reply 8: Now results chapter is the 5. We include a table as supplementary data with all the volume calculations made and the accuracy of the method. Also in the new version the uncertainty of the data is better showed and supplementary data are cited in the main text..

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Page 7 line 5: the lack of description of the studied area has a strong impact in this chapter. Readers are not able to understand which (and how many) catchments produced debris flow and which are characterised by clean water flow.

Page 7 line 9: "Debris flows occurred only in 9% of the catchments" how many catchments are considered in this papers? I do not find a detailed description of this basic information.

Reply 9: Now results chapter is the 5. The first sentence of subchapter 5.1 says in how many catchments debris-flow deposits were generated (49) and in which they did not develop (75). This information is also clearly expressed in figures and tables. However, at the request of the reviewer, this information was given again in the paragraph mentioned in sub-chapter 5.2 and 5.3. Also in the chapter of methodology (4) we include the total of studied tributary catchments (124). I would like to clarify that the inventory of catchments where debris flows were deposited is part of the results of this work and does not represent the framework of the study area. Note also the improve of the description of the studied area (Chapter 2 and 3).

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Chapter 4.4: this chapter is very short, and the description of obtained results it is not very clear.

Reply 10: We're not quite sure what the reviewer is referring to at this point. The text is short, nevertheless the related figures use a lot of data and its clearly presented. We think that no modifications are necessary at this subject.

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Page 8 line 19. "We propose based on field observations that this is because debris supply from rocky outcrops of hillslopes is not a significant source of sediments during the storms. Instead, we propose that the main source of debris flows is sediment stored in the drainage network." This is a good point, but I am not sure that this comment is clearly supported by presented data.

Reply 1: Now we rewrite the paragraph.

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Page 9: this page should be totally rewritten. It is not clear the real objective of this part of the manuscript. There is a critical mix between data of the studied basin and data of other basins studied by other authors and not related to the 2015 event. This data, in my opinion, should be presented earlier and the authors can make a discussion pointing out differences between the studied phenomenon and other case studies.

Reply 12. Paragraphs in this page, and in the whole chapter of discussion, were rewritten, considering the comments of the reviewer. Now, we present first the interpretation of our results, and later include the discussion of its significance in relation with other works.

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Page 10 line 1: the mean value of 1.3 mm of erosion has been calculated without an effective description of the accuracy of the method. The lack of a serious scientific method is very critical.

Reply 13: see Reply 8.

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Page 10 line 3: “and the return time of these kind of events from paleoclimatic record of 1 event each 100 years.” I did not find in the manuscript this paleoclimatic study before this sentence.

Reply 14: The reviewer refers to line 3 of page 11, but in the same chapter we refers to paleoclimatic record of others works. We think that no modifications are necessary at this subject.

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Page 11 line 10 “The limitations to study erosion associated with an individual storm event include the lack of pre-event high resolution topography” in this paper, I do think that authors mentioned the availability of a detailed topography after the event.

Reply 15: Indeed, there is a detailed topography (LIDAR) taken after the event, but not before the event and not for the whole study area. We think that no modifications are necessary at this subject.

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Page 11 line 15: “Nevertheless, these records also underestimate the peak discharge associated with the storm because the gauging stations are commonly damaged by flash floods” Is this e general comment or referred to the presented case study?

Reply 16: This is a general statement, at least for the Huasco River and other nears rivers of Chile. For example, in this year, the governmental of Chile inaugurate a more robust station in the Huasco River (<https://atacamanoticias.cl/2019/11/14/nueva-estacion-fluviometrica-el-maiten-en-el-rio-huasco-lleva-un-85-de-avance/>). We think that no modifications are necessary at this subject.

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