RESPONSE TO REVIEW COMMENTS

We thank the reviewer for the insightful comments and suggestions. We believe that the edits in response to the comments have significantly improved the manuscript in terms of clarity, language, and structure. Our responses are provided below. **Please note that the original comments provided by the reviewer are in black letters and our responses are in blue letters**. In addition to these responses, we will provide a revised manuscript, as well as a copy with the tracked changes where all revisions were implemented.

Before beginning, we offer here a summary of our responses to the key concerns of the reviewer. More detailed responses are provided under the specific comments.

1. Do you have any replication of each experiment? Did you perform only one set of measurements per experiment?

> Three replicates of each rainfall intensity were performed. Repeatability was confirmed by evaluating changes in RR at specific cross-sections in the rainsplash dominated zone. It was found that on an average, the relative error of the RR ratios between replicates did not exceed 7%.

2. Do you overestimated the relevance of your results and reach conclusions that are not sufficiently proven by their data?

> Our discussion of the results has been adjusted to be more in line with the level of the analysis provided. Please see the specific comments below for each case.

A. When and where does microroughness matter?
Through these studies we were able to determine that microroughness really matters in these two cases: (1) when there is no cover, which is between harvest and planting; and (2) at the beginning of a storm event. We therefore offer only a small slice of the whole erosion process in this study under the controlled condition experiments.

B. Why perform controlled condition experiments?
Our experiments were designed to help us decipher the role of rainsplash on roughness by isolating it from the role of other processes such as runoff, variable water content, bare soil surface, texture, etc. Microroughness can lead to the formation of depression storage which can ultimately affect ponding and the formation of flow pathways.

General Comments:

The manuscript entitled "Quantifying the changes of soil surface microroughness due to rainfall-induced erosion on a smooth surface" (Reference number NPG-2016-76) authored by B.K.B. Abban, A.N. Papanicolaou, C.P. Giannopoulos, D.C. Dermisis, K.M. Wacha, C.G. Wilson, and M. Elhakeem presents results from a simulated rainfall experiment consisting of applying three different intensities to a smoothed bare soil surface. Authors calculated two widely-used indicators of surface roughness and discussed the implications of their results for modelling approaches.

The reported work is interesting and fits within the scope of Nonlinear Processes in Geophysics. However, the manuscript has an unusual organization and authors mixed methods with results and discussion. Moreover, relevant information is missing from the Materials and Methods section.

Another major concern that I have after reading this manuscript is the feeling that authors overestimated the relevance of their results and reached conclusions that are not sufficiently proven by their data, especially because of the number of events that they experimented (only three, one per rainfall intensity with no replications).

Finally, a few English mistakes must be corrected.

In the following lines, I provide the authors with some suggestions in order to improve their manuscript. They must correct them in order that this manuscript achieves the standard quality for being published in Nonlinear Processes in Geophysics.

Therefore, I recommend the rejection of this manuscript. However, if the editor feels that the research presented is of interest, I made a great number of comments and suggestions in the following pages.

Response:

Regarding restructuring, we have reallocated those areas identified by the reviewer as being out-of-place to the recommended spots. The specifics are provided in the comments addressed for the Materials and Methods and the Results.

We have also included more specifics regarding the methods used.

Those comments where the reviewer feels we have stretched too far in the relevance of our results have been adjusted to be more in line with the level of the analysis. Finally, the grammatical mistakes have been addressed with a thorough read following the edits.

Specific Comments:

1. Abstract: **The abstract must be greatly improved.**

Comment 1: Page 1, lines 13-14: "in agricultural landscapes", this is too general and does not describe what you have done. You did not study all agricultural landscapes, even not a few of them, just one and adapted to the smooth surface conditions that you were interested in.

Response:

We acknowledge that the expression "in agricultural landscapes" may be too general. The sentence (Page 1, lines 13-14) has been reworded as follows to be more precise:

"This study examines the rainfall induced change in soil microroughness of a bare smooth soil surface in an agricultural field."

Comment 2: Page 1, line 17: "representative intensities", representative of what? For instance, 75 mm/h is a very high intensity; does it frequently happen in your region?

Response:

The term "*representative intensities*" was based on the intensity recurrence intervals for Iowa using Huff and Angel (1992). The 30 mm/h event has a recurrence interval of ~0.25-yr, while the 60 mm/h event has a 20-yr recurrence interval. Finally, the 75 mm/h event has a 60-yr return period. Rainfall intensities of 75 mm/h usually appear in late May and June from convective thunderstorms in the study area, as seen from tipping bucket data near the test plot. Here, the Abstract (Page 1, lines 19-21) has been modified as follows and the citation placed in the reference list of the manuscript:

"Three rainfall intensities of 30 mm/h, 60 mm/h and 75 mm/h were applied to a smoothened bed surface in a field plot via a rainfall simulator. These intensities represent the range from typical to extreme rainfall intensities that appear in the region of study (e.g., Huff and Angel, 1992)."

<u>Reference</u>

Huff, F. A. and Angel, J. R.: Rainfall Frequency Atlas of the Midwest. Midwestern Climate Center Research Report 92-03. Champaign, IL, 1992.

Comment 3: Page 1, line 21: I would remove "for initial microroughness length scales of 2 mm" since this is already stated in line 15 and you did not study any other length scales.

Response:

The phrase "*for initial microroughness length scales of 2 mm*" was removed.

Comment 4: Page 1, line 22: How can your results contradict literature when you have said that there is no literature for the surface conditions you have assayed?

Response:

We understand the reviewer's confusion here. What we meant to convey was that the results contradict the commonly adopted belief in the literature that microroughness decays with rainfall regardless of the initial roughness conditions (e.g., bare flat soil vs. bare disturbed soil). We modified the sentence (Page 1, lines 25-26) as follows:

"*This contradicts the commonly adopted notion in existing literature that a monotonic decay of soil surface roughness with rainfall is expected regardless of initial surface roughness conditions.*"

Comment 5: Page 1, lines 23-24: "Analysis shows", what analysis? This last sentence must be re-phrased and a conclusion should be added.

Response:

This last part of the abstract (Page 1, lines 23-28) has been modified to include a summary statement as follows:

"Findings show a consistent increase in roughness under the action of rainfall, with higher rainfall intensities resulting in higher relative roughness increase. This contradicts the commonly adopted notion in existing literature that a monotonic decay of soil surface roughness with rainfall is expected regardless of initial surface roughness conditions. This study sheds light on the interaction between rainfall and smooth bare surfaces which can result in increasing soil surface roughness, and highlights the need for further examination of the phenomenon and its potential implications on hydrologic response."

Introduction: This section is well-written and provides enough information about the background of the presented work.

Response:

Thank you.

Comment 1: Page 1, line 29: You could remove "reported in the literature"

Response:

The phrase "*reported in the literature*" was removed.

Comment 2: Page 2, line 4: "From the outlined above" instead of "From the classes outlined above".

Response:

This change was made.

Comment 3: Page 2, line 7: What is "scape"? Is this a mistake? Should it be "scale"?

Response:

The term "*soil scape*" is used often in soil science, referring to a soil column. We have kept this term in the text.

See for example: Yaalon, D.H.: "The changing model of soil" revisited. Soil Science Society of America Journal, 76, 766-778, 2012.

Comment 4: Page 2, line 8: I am not sure if "enhancing" is the right word here.

Response:

The sentence was modified as follows to remove the term "*enhancing*":

"*As a result, RR affects key hydrologic processes at the soil scape and ultimately at the hillslope scale (e.g., overland flow, infiltration), by affecting the depression storage and the associated runoff and erosion processes (Gomez and Nearing, 2005; Chi et al., 2012).*"

Comment 5: Page 2, line 10: I do not understand why you cite Paz-Ferreiro et al., 2008 here. Besides, according to the reference list, Allmaras (1966) should be Allmaras et al. (1966).

Response:

We cite Paz-Ferreiro et al., (2008) to support the claim that RR is the most widely used descriptor of soil surface roughness. We have also corrected the citation to "Allmaras et al. (1966)". The text now reads as (Page 2, lines 15-16):

"*According to Paz-Ferreiro et al. (2008), the RR index, which was first proposed by Allmaras et al. (1966), is the most widely used statistical microrelief index for the evaluation of soil surface roughness.*"

Comment 5: Page 2, line 21: "Few to none"? Not sure about this.

Response:

We have performed an exhaustive literature search of studies that involve the quantification of soil surface roughness. Few of the existing microroughness scale studies explicitly examine the interaction of rainfall with bare soil surfaces when the surface is initially smooth and undisturbed (e.g., in most studies the surfaces are either partially covered by vegetation or disturbed by tillage). We modified the sentence (Page 2, lines 28-29) to convey the above as follows:

"*Few existing studies, to the best of our knowledge, have explicitly examined the interaction of raindrop impact with bare soil surfaces for initial microroughness scales on the order of 2-5 mm.*"

Comments 6-7: Page 2, lines 22-23: "This condition"? Do you mean less than 2 mm? Here you say that initial microroughness scales less than 2 mm is the prevalent condition in agricultural hillslopes. However, I am not so sure that this is the prevalent state.

Response:

You are right, the sentence needed clarification. Our study explicitly deals with surfaces of initial roughness less than 2 mm. However, it is considered as an extreme condition and findings can be applicable to surfaces of the order of ~2-5 mm, which have been found to be common in agricultural landscapes according to pertinent studies (e.g., (Burwell et al., 1963; Allmaras et al., 1966; Burwell et al., 1969; Cogo, 1981; Currence and Lovely, 1970; Steichen, 1984; Unger, 1984; Zobeck and Onstad, 1987). The sentence was reworded as follows for more clarity (Pages 2-3, lines 29-2):

"*Surfaces with microroughness on the order of ~2-5 mm are common in agricultural landscapes (Burwell et al., 1963; Allmaras et al., 1966; Burwell et al., 1969; Cogo, 1981; Currence and Lovely, 1970; Steichen, 1984; Unger, 1984; Zobeck and Onstad, 1987) where the soil is "smoothened" due to long, undisturbed exposure to rainfall impact and runoff (Abaci and Papanicolaou, 2009) and freeze-thaw cycles (Zobeck and Onstad, 1987). Within these landscapes, soil surface conditions are usually bare in the period of the crop rotation between post-harvest and before plant growth is established, which approximately corresponds to 30-75% of the cyclic crop rotation period.*"

Comment 8: Page 2, lines 25-28: This portion of text about the models is not very well linked with the rest of the introduction. Moreover, you cite three studies. Huang and Bradford (1992), Rosa et al. (2012) and Zheng et al. (2014) that stated that RR increased with rainfall, but later you do not discuss your results in the light of these three studies, why?

Response:

The studies of Huang and Bradford (1992), Rosa et al. (2012), Vázquez et al. (2008), and Zheng et al. (2014) provide quantitative indications that roughness can increase with rainfall, but they neither explicitly acknowledge the increasing trend nor link it to bare smooth surface conditions. Our goal herein is to highlight that phenomenon.

The part of the Introduction referring to these studies (Page 3, lines 3-13) was modified to outline their relevant findings:

"*There are some quantitative indications that under bare smooth surface conditions, soil surface roughness may actually increase under the action of rainfall. Specifically, the study by Huang and Bradford (1992) calculated the semivariance with respect to length scale before and after* *rainfall, and a slight increase in roughness with rainfall was denoted using the Markov-Gaussian model for a surface with low initial roughness. Rosa et al. (2012) introduced an index (called Roughness Index) estimated from the semivariogram to describe roughness, and the index increase with rainfall under specific conditions was observed and attributed to the fragmentation of aggregates and clods to smaller aggregates. Zheng et al. (2014) reported an increase in values of the RR after the application of rainfall on smooth soil surfaces. Finally, Vázquez et al. (2008) examined the evolution of the surface of three different soils during successive events. They reported that for two out of three soils, roughness increased for the first event, however decreased for the following events; the third soil showed scarce trend to either increasing or decreasing roughness due to successive rainfall events. Nevertheless, none of the above studies explicitly stated or acknowledged the increasing trend of roughness and its potential linkage to smooth bare soil surface conditions.*"

We have also added the Results further comparison between these studies and our own findings (Page 8, lines 5-19):

"*First, our study along with Vázquez et al. (2008) and Zheng et al. (2014), which were performed for the smooth surface initial condition, report an increase in RR with rainfall in general. Exception seems to hold for one soil surface of the study of Vázquez et al. (2008), as well as the smooth surfaces of Vermang et al. (2013) which show decaying roughness due to rainfall due to different soil type and conditions. Second, the present study indicates that the RR ratio becomes higher with higher cumulative rainfall amount when the surface is classified as smooth, whereas the opposite tends to hold for soil surfaces classified as disturbed (Fig. 5, Table 1). Vázquez et al. (2008) and Zheng et al. (2014) recorded an increase in RR with rainfall but had significantly lower values of RR ratio than we did. However, this may be attributed to the fact that they applied lower cumulative rainfall amount, and the initial microroughness conditions in their study were higher. Other studies that are not included in Table 1 have also shown increasing trends of roughness with rainfall, as quantified by the use of different indices. For instance, Huang and Bradford (1992) calculated the semivariograms for different surfaces and used fractal and Markov-Gaussian parameters to quantify the roughness. Markov-Gaussian analysis showed a relative increase in the roughness parameter for a surface of low initial roughness. Finally, Rosa et al. (2012) introduced the Roughness Index which is estimated from the semivariogram sill in order to quantify roughness, and observed its increase with rainfall under low initial roughness conditions. That increase was attributed to the fragmentation of aggregates and clods to smaller aggregates but was not linked to smooth bare soil surface conditions.*"

Comment 9: Page 2, lines 29-30: This is already stated in the former paragraph.

Response:

The sentence was removed.

Comment 10: Page 3, lines 3-9: This is a little bit messy from my viewpoint. Moreover, the two last sentences can be removed.

Response:

To clean the text, we shortened and reorganized the last part of the introduction. We highlight more the specific objectives (Page 3, lines 20-24), which read as follows:

"The key specific objectives of this study are (i) to quantify the soil surface microroughness of smooth bare soil surfaces before and after the effect of rainfall, and (ii) calculate the relative change in roughness for different intensities. To meet the two specific objectives we employ four commonly used indices, the RR index, the crossover length, the variance scale from the Markov-Gaussian model, and the limiting difference. The last three indices are alternate methods and used here to supplement the RR index analysis for relative change in roughness."

2. Materials and Methods: **This section lacks from essential information. Do you have any replication of each experiment? Did you perform only one set of measurements per experiment? It is not clear what geostatistical analysis has been performed.**

Response:

All tests were performed three times to ensure repeatability in terms of homogeneity in the raindrop distribution over the rainfed test area, steady state conditions in terms of runoff volumes, and the same soil water content at the inception of the tests. Figure 1b provides a glimpse of the complex set-up needed for the experiments and hence the level of work in these experiments. Raindrop distribution was tested with image analysis (Image J software) of rain splashes within painted areas in the plot to discern them from the rest of the soil background. A weir at the outlet of the plot ensured the occurrence of steady state conditions. The continuous monitoring of volumetric soil water content showed that we had the same water content at the start of the test. Agreement between the test replicates in terms of the key geomorphic and roughness features was found by evaluating changes in random roughness at specific cross sections.

Comment 1: Page 3, line 12: Maybe, you should give the geographical coordinates and elevation of your study area.

Response:

The geographical coordinates and elevation of the experimental plot are now provided (Page 3, lines 27-29):

"*This study was conducted on an experimental plot (Fig. 1b) of the U.S. National Science Foundation Intensively Managed Landscapes Critical Zone Observatory in the headwaters of Clear Creek, IA (41.74º N, -91.94º W and an elevation of 250 m above mean sea level).*"

Comment 2: Page 3, line 14: What do you mean by "mixed, superactive"?

Response:

The terms "*mixed*" and "*superactive*" are soil taxonomy nomenclature used by the USDA (https://www.nrcs.usda.gov/Internet/FSE_DOCUMENTS/nrcs142p2_051232.pdf). The term "*mixed*" is related to the mixed nature of clay mineralogy, since these soils contain both smectite and illite. The term "*superactive*" is associated with the high degree of activity of cation exchange in the soil. The cation ion exchange capacity for these soils is between 15 and 30 Meq/100 g. Even though the goals of this paper do not include the effects of texture (since we only use one soil type) this information is helpful for the reader to judge the applicability of this study to their sites. We have added this info along with the textural characteristics of the soil (Pages 3-4, lines 29-1):

"*The soil series at the plot where the experiments were conducted is Tama (fine-silty, mixed, superactive, mesic Cumulic Endoaquoll) (http://criticalzone.org/iml/infrastructure/field-areas-iml/). It consists of 5% sand, 26% clay, 68% silt, and an organic matter content of 4.4%. The aggregate size distribution of the soil consists of 19% of the soil size fraction less than 250 μm, 48% between 250 μm and 2 mm, and 33% greater than 2 mm. These soils contain both smectite and illite, with high cation exchange capacity between 15 and 30 Meq/100 g.*"

Comment 3: Page 3, line 18: "to the plots", how many plots?

Response:

There was one experimental plot. This was a typo, which we have corrected.

Comment 4: Page 3, line 25: "widely accepted", by whom?

Response:

The Marhsall-Palmer distribution is an accepted relationship by the American Meteorological Society (http://glossary.ametsoc.org/wiki/Marshall-palmer_relation). The following reference was added in the text:

Marshall, J.S., Palmer, W.M.K.: The distribution of raindrops with size, Journal of Meteorology, 5, 165–166, doi:10.1175/1520-0469(1948)005<0165:TDORWS>2.0.CO;2, 1948.

Comment 5: Page 3, lines 27-29: This statement is too strong, even though rainfall characteristics were similar, other regions may have different soil types than the one studied here. What are the potential biases you are referring to in this sentence?

Response:

We agree with the reviewer that the direct comparison of our results may only be possible under similar soils examined in this study. This includes regions exhibiting semi-humid climate conditions and have mollisol soils. We have added the following caveat to the Discussion and Conclusions (Page 10, lines 26-28) and the citation to the reference list:

"*It is recognized that dryer, silty type soils may not exhibit the increase in RR shown here. Also, the role of sealing may be important on roughness development under bare soil conditions and needs further examination. Soil water retention characteristics of the soils under sealing and its implication to RR must be considered (Saxton and Rawls, 2006).*"

In terms of other potential biases between natural rainfall and the rainfall supplied by our simulators, a proper calibration must be performed to match the drop size. Drop size affects the terminal velocity and the kinetic energy of the falling rain. A poor calibration of the raindrop size distribution would affect the overall size and shape of the roughness formed. The sentence (Page 4, lines 13-16) has been modified as follows:

"*This level of attention was taken to minimize any potential biases compared to natural rainfall with respect to raindrop size distribution, and, thus, render the rainfall simulation experiments scalable to other regions experiencing the same type of soil, bare surface, roughness conditions, and natural rainfall characteristics.*"

Comment 6: Page 3, line 32: I would substitute "a priori the runs ensured that" for "before the runs confirmed that".

Response:

This change was made.

Comment 7: Page 4, line 1: This seems more like a result than materials and methods.

Response:

We agree with the reviewer. This sentence was removed from the Materials and Methods.

Comment 8: Page 4, line 3: What is CCD?

Response:

It is a charge-couple device camera that serves to transfer the electrical charge to the attached computer and to convert it into digital signal. "*Charge-couple device*" was used in the text instead of "*CCD*".

Comment 9: Page 4, line 5: I would use "by software" instead of "from the desktop, using a computer program".

Response:

The change was made.

Comment 10: Page 4, line 7: Please, provide the names and references for the specific software used.

Response:

The reference for the software is now provided in the text (Page 4, lines 25-28) as follows:

"*The information from each scan is converted into a set of (x,y,z) coordinates using a calibration file and the software developed from the USDA-ARS National Soil Erosion Research Laboratory for data transformation as explained by Darboux and Huang (2003).*"

Comment 11: Page 4, line 12: Remove "experimental" before "tests".

Response:

The word "*experimental*" has been removed.

Comment 12: Page 4, lines 12-13: Change the sentence to "Rainfall intensities were respectively 30, 60 and 75 mm/h for experiments 1, 2 and 3".

Response:

The text has been changed per the reviewer's suggestion.

Comment 13: Page 4, line 14: Is the duration of your rainfall events the same of the storms you are referring here? In fact, 75 mm/h during 5 hours means 375 mm in 5 hours, which seems too much. What is the return period of these events? I mean, are these storms really so usual?

Response:

No, the duration is not the same. However, because rainfall effects for the controlled experiments performed are isotropic it is the intensity that affects splash and RR (see all responses to comment 1 of reviewer 3). The 30 mm/h event has a recurrence interval of ~0.25-yr, while the 60 mm/h event has a 20-yr recurrence interval. Finally, the 75 mm/h event has a 60-yr return period.

Duration would only matter if the goal of the tests was to examine the role of runoff and shear on RR. Here the goal is to examine the role of KE on RR.

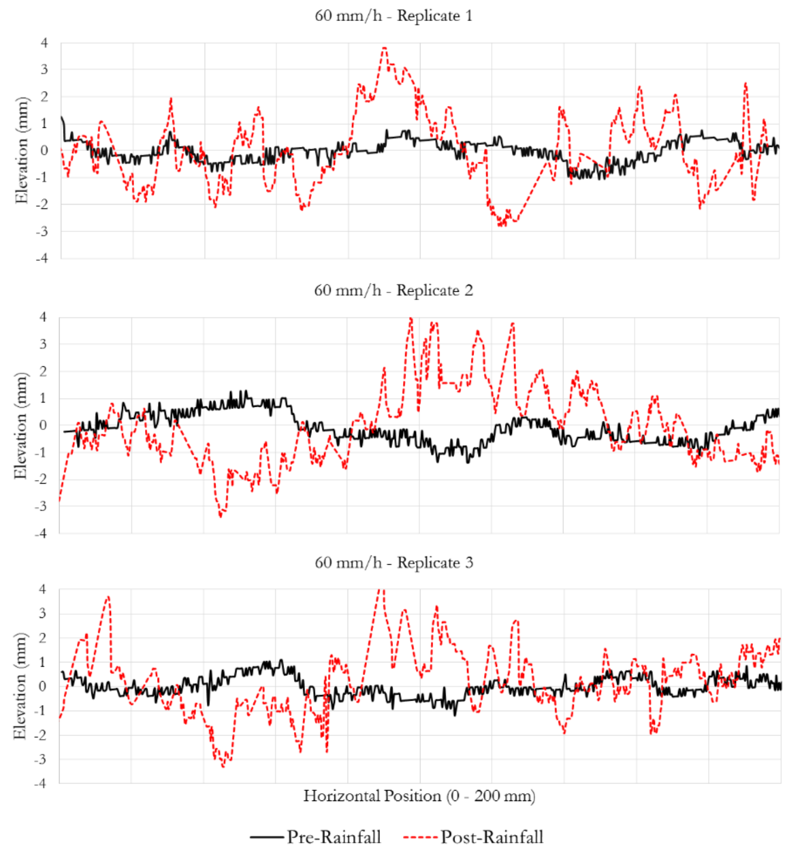
$$\mathrm{KE_i} = \frac{1}{2}\mathrm{m_i v_{t\,i}^2}$$

where mi is the mass of raindrop i (kg), v_{ti} (m/s) raindrop terminal velocity, ρ_i is the density of raindrop i (kg/m3), and V_i is the volume of the raindrop i (m^3) which assumes a spherical shape.

During each of the experiments, the rainfall intensity remained constant and the storm duration was chosen in order to reach steady-state conditions in terms of infiltration and runoff and thus obtain repeatable results in terms of roughness patterns. Three replicates of each rainfall intensity were performed, and repeatability was confirmed by evaluation of changes in RR at specific cross-sections in the rainsplash dominated zone, as shown below for the 60 mm/h case. We found that on an average, the relative error of the RR ratios between replicates did not exceed 7%. Per the reviewer's comment, we have added (Pages 4-5, lines 34-3):

"*Three replicates of each rainfall intensity case were performed, and repeatability was confirmed by evaluation of changes in RR at specific cross-sections in the rainsplash dominated zone. It was found that on an average, the relative error of the RR ratios between replicates did not exceed 7%.*"

	Pre-Rainfall RR (mm)	Post-Rainfall RR (mm)
60 mm/h - Replicate 1	0.42	1.44
60 mm/h - Replicate 2	0.55	1.59
60 mm/h - Replicate 3	0.37	1.40



Comment 14: Page 4, line 15: "Decagon soil moisture sensors", specify depth, how many and model.

Response:

Six 5TE soil moisture sensors were placed to a depth of 10 mm along the whole plot (Page 5, lines 3-4):

"*The volumetric soil water content was recorded with via six 5TE soil moisture sensors manufactured by Decagon Devices, Inc. and placed along the whole plot to a depth of 10 mm.*"

Comment 15: Page 4, line 16: What do you mean by 35%? What is the field capacity of this soil? Please, specify.

Response:

This value of 35% refers to the average initial volumetric water content at the plot. The field capacity for the soil is 38%. The sentence (Page 5, lines 4-6) reads as:

"*The initial volumetric water content was found to be the same for each experiment and approximately equal to 35% at the whole plot, where the field capacity of the specific soil is 38%.*"

Comment 16: Page 4, line 29: Remove "By definition".

Response:

The phrase was removed.

Comment 17: Page 4, line 31: "extracted" instead of "extract".

Response:

The word was replaced.

Comment 18: Page 4, line 32: "were" instead of "are".

Response:

The word was replaced.

Comment 19: Page 5, line 2: What is "its commonality found in the literature"?

Response:

The sentence has been reworded to better convey our message (Page 5, lines 29-30):

"*The RR index calculated from Eq. (1) was used in this study as the principal method to quantify soil surface roughness due to its frequent and widespread use in various studies and landscape models as a descriptor of microroughness.*"

Please note that additional indices were considered per the request of Reviewer 2.

Comment 20: Page 5, line 3: "was used" instead of "is used".

Response:

The phrase has been replaced.

Comment 21: Page 5, lines 8-12: This is not clear to me. Did you use all these methods? At the end of the sentence you say "among others", what do you mean? Do you imply that you used more methods than those indicated here?

Response:

We see how confusion may have arisen from our original statement. No, we did not use all the methods outlined. We have modified the sentence to read (Pages 5-6, lines 31-2):

"*If correlation exists within a certain spatial scale, RR will likely change with the changing window size of observed data (Paz-Ferreiro et al., 2008) and may be dependent on the resolution of the measurement device (Huang and Bradford, 1992). Thus, alternative scale-independent methods that consider spatial correlation have been developed by other researchers in order to address this issue.*"

Comment 22: Page 5, line 14: I would split this sentence in two. Instead of "with the advantage of its quantification being scale independent", I would use a point and then "It has the advantage of being scale independent".

Response:

The sentence was modified as suggested.

Comment 23: Page 5, line 16: "the semivariogram is a useful…" I do not think this sentence is really needed.

Response:

The sentence was removed.

Comment 24: Page 5, lines 22-25: it seems rather peculiar that you explain what a semivariogram is but not what the Hurst exponent indicates.

Response:

The Hurst exponent is briefly explained after Eq. (3). However, it is not extensively described because it is not of particular interest for this study. It has been shown to be less sensitive than crossover length when describing soil surface evolution influenced by rainfall (Vázquez et al., 2005). The text now reads as (Page 6, lines 23-24):

"*The generalized Hurst exponent is a less sensitive descriptor of soil surface evolution as influenced by rainfall (Vázquez et al., 2005), hence attention is mostly centered on the crossover length.*"

Comment 25: Page 5, line 28: "and 0 < H < 1", this should come before, when you refer to H and not after the crossover length.

Response:

This changed has been made.

Comment 26: Page 5, lines 32-33: I did not understand this sentence. Please, re-phrase it.

Response:

We rephrased these two sentences (Page 6, lines 24-27), which now read as follows:

"*Given the semivariogram plot calculated using Eq. (2), H and l can be extracted by fitting a power law relationship in the form of* $y = Ax^B$ *to the semivariance-lag distance data, where* $y = \gamma(h)$ *and* $x = h$*. According to Eq. (3), the B regression variable gives the generalized Hurst exponent value and the A regression variable yields the crossover length.*"

Comment 27: Page 6, lines 1-4: Could you, please, re-phrase this paragraph? I do not understand it properly, it is a bit confusing.

Response:

We have completely removed this paragraph to avoid confusion of the reader. Besides, it is explained in the Results section, please see Comment 8.

3. Results

Comment 1: Page 6, lines 6-14: This looks more like materials and methods than results.

Response:

We moved and modified this paragraph to the end of the section of Materials and Methods (Page 7, lines 12-18):

"In order to negate the effects of the differences found in initial microrelief amongst the three runs and compare rainfall induced changes in relative terms, the results from the rainfall experiments are presented in the form of ratios of the roughness indices. More precisely, the RR ratio, defined as the ratio of the RR index post-rainfall over the RR index prior to the rainfall (RR_{post}/RR_{pre}*), is calculated for each experiment. Semivariograms are plotted under pre- and post-rainfall conditions at the ROI to assess the spatial correlation of surface elevations. Along the same lines, ratios between pre- and post-rainfall conditions are calculated for the crossover*

length, the variance length scale of the Markov-Gaussian model, and the limiting difference to assess changes in microroughness along with the RR ratio."

Comment 2: Page 6, lines 12-14: Have these comparisons been performed against literature data? Did this literature provide ratios?

Response:

Yes, sections 3.1 and 3.2 are now updated and include more detailed comparisons with the cited studies. Figure 5 is updated to show the RR ratio versus the initial value of RR for our study along with the relevant studies considered. From Fig. 5 it is now clear that our study captures the behavior of RR for an initial range that was not covered before. In some of the studies (e.g., Zhang et al., 2014), ratios of the roughness indices were already provided, whereas in others they were calculated, since only the initial and final values of the indices were provided. Moreover, the study of Vázquez et al. (2008) which was previously missing from our study is now added to Fig. 5 and Table 1 and discussed in the text for completeness, since it involves quantification of RR for nearly smooth surfaces. Finally, cumulative rainfall amounts for each experiment are provided in Table 1 for a better inter-study comparison.

The updated Results section for the inter-study comparisons now reads as (Pages 7-8, lines 23-2):

"*Figure 5 shows the RR ratio, i.e., RR_{post}/RR_{pre}, with respect to the initial value of RR for the present study along with other studies that quantify rainfall induced microroughness changes. The dashed line at the RR ratio value of unity reflects no change in roughness, thus all points above that line show an increasing trend with rainfall, while all points below show a decreasing trend with rainfall. All the studies capture a wide range of initial RR values – up to 21 mm – and it is clear that our study captures the behavior of RR for an initial range that was not covered before. Figure 5 suggests that roughness may increase with raindrop impact for a range of low initial RR values (< 5 mm), while it consistently decays for high initial RR values (> 5 mm). It is acknowledged that the values of the roughness indices among different studies may involve some experimental error and may reflect different conditions (such as rainfall forcing and soil type). Specifically, Vázquez et al. (2007) used clay textured soil, Vázquez et al. (2008) used silt loam textured soil, while our study along with all the other studies cited conducted rainfall experiments for silty clay loam textured soil. Rainfall intensities and cumulative rainfall amounts varied significantly among studies.*"

The revised Figure 5 is below:

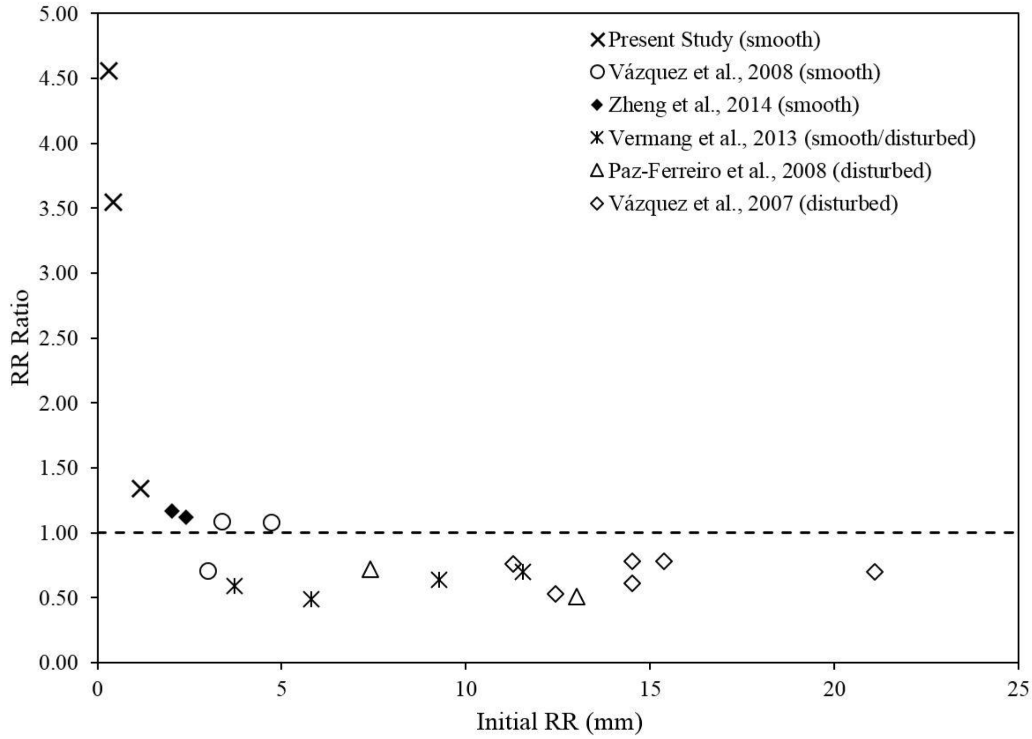


Figure 5: Random Roughness (RR) Ratio versus initial RR for this study and other selected studies.

Comment 3: Page 6, line 20: Have these experiments been performed only once?

Response:

This question was answered earlier under comment 13 in the Materials and Methods. Three replicates of each rainfall intensity case were performed.

Comment 4: Page 6, lines 21-23: Please, re-phrase this. It seems redundant.

Response:

The whole paragraph (Page 8, lines 3-19), which contained this phrase was modified to put our study into a context with the available literature. In doing so we eliminated the part that was considered redundant.

"*Table 1 summarizes the results of this study along with results from the selected studies in quantitative terms, documenting the RR index values before and after the rainfall events, the cumulative rainfall, as well as the associated RR ratio. Two inferences can be made from Table 1. First, our study along with Vázquez et al. (2008) and Zheng et al. (2014), which were* *performed for the smooth surface initial condition, report an increase in RR with rainfall for the most part. Exception seems to hold for one soil surface of the study of Vázquez et al. (2008), as well as the smooth surfaces of Vermang et al. (2013) which show decaying roughness due to rainfall. Second, the present study indicates that the RR ratio becomes higher with higher cumulative rainfall amount when the surface is classified as smooth, whereas the opposite tends to hold for soil surfaces classified as disturbed (Fig. 5, Table 1). Vázquez et al. (2008) and Zheng et al. (2014) recorded an increase in RR with rainfall but had significantly lower values of RR ratio than we did. This may be attributed to the fact that they applied lower cumulative rainfall amount, and the initial microroughness conditions in their study were higher. Other studies that are not included in Table 1 have also shown increasing trends of roughness with rainfall, as quantified by the use of different indices. For instance, Huang and Bradford (1992) calculated the semivariograms for different surfaces and used fractal and Markov-Gaussian parameters to quantify the roughness. Markov-Gaussian analysis showed a relative increase in the roughness parameter for a surface of low initial roughness. Finally, Rosa et al. (2012) introduced the Roughness Index which is estimated from the semivariogram sill in order to quantify roughness, and observed its increase with rainfall under low initial roughness conditions. That increase was attributed to the fragmentation of aggregates and clods to smaller aggregates but was not linked to smooth bare soil surface conditions.*"

Comment 5: Page 7, lines 4-5: How did you check this significance? Only by stating that difference is less than 10%?

Response:

A nonparametric test for spatial isotropy was performed per Guan et al., 2004 using the public domain R statistical package with 'spTest' library. A p-value less than 0.05 for all cases confirmed the spatial isotropy hypothesis. Thus, there would be no bias by taking one representative direction to calculate the semivariograms and the associated crossover lengths. The corresponding part of the paragraph was adjusted as follows (Page 8, lines 26-30):

"*Since the action of rainfall is isotropic and adds no systematic trend along any direction, no significant differences were expected between semivariograms. A nonparametric test for spatial isotropy was performed per Guan et al., 2004 using the public domain R statistical package with the 'spTest' library. The spatial isotropy hypothesis was confirmed ($p < 0.05$). Thus, there would be no bias in taking any direction to calculate the semivariograms and the associated crossover lengths.*"

Comment 6: Page 7, line 6: "representative semivariogram", that corresponds to which angle?

Response:

The semivariograms were chosen at the downstream direction that corresponds to an angle of 0 degrees. We have removed the term "representative".

Comment 7: Page 7, line 7: “semivariograms” instead of “semivariogram”.

Response:

This change was made.

Comment 8: Page 7, lines 10-11: “which is considered sufficient to assume no spatial autocorrelation at the scale examined in this study”, I do not get this; you checked all roughness for the 200 mm, so you should have accounted for lag distances less than 10 mm.

Response:

The semivariograms describe the mean variance of the elevation measurements for a range of lag distances and indicate the distance at which spatial autocorrelation ceases to exist. RR is independent of the window size, only if the window size significantly exceeds the spatial autocorrelation range, which is the case here. Our intent here was to point out that the window size of the ROI is sufficiently large enough to eliminate the effects of spatial autocorrelation of data on RR quantification. The last part of the paragraph (Pages 8-9, lines 33-5) has been rephrased as follows:

“*These lag distances are approximately 10 mm, so the selected 200 mm window size of the ROI is almost 20 times greater than the spatial autocorrelation range. This implies that the window size of the ROI falls at the scale of the ‘‘semivariogram sill’’ (which is defined as the near-constant value of semivariance at large lag distances where the semivariogram levels out – see horizontal dashed lines in Fig. 6). RR is directly related to the semivariogram sill (e.g., Vázquez et al., 2005; Vermang et al., 2013), therefore it is independent of the selected window size, given that the latter significantly exceeds the spatial autocorrelation range.*”

Comment 9: Page 7, lines 16-17: “pre-rainfall values” instead of “pre-rainfall value for all three intensities”.

Response:

The change was made.

Comment 10: Page 7, line 18: I would use “events” instead of “precipitation intensities”.

Response:

The change was made.

Comment 11: Page 7, line 19-20: Please, check English and re-phrase this sentence.

Response:

The (Page 9, lines 9-11) sentence has been reworded as follows:

"*Complete agreement between the trends of the RR index and the semivariogram sill justify the use of the RR index as a representative and unbiased descriptor of microroughness.*"

Comment 12: Page 7, line 22: Remove "in existing literature".

Response:

The phrase has been removed.

Comment 13: Page 7, line 24: "reported" instead of "report".

Response:

The change was made.

Comment 14: Page 7, line 25: "found" instead of "in the crossover length reported".

Response:

This has been done.

4. Conclusions and Discussion: **This section is very weak and should be greatly improved. Besides, it should be entitled "Discussion and Conclusions".**

Response:

The title was changed to "Discussion and Conclusions". A great effort was made to improve this section in response to the reviewer's valuable comments. Please see the specific responses below.

Comment 1: Page 8, line 2: Are you sure that these experiments are "unique and novel". I am also concerned about the fact that you state that your experiments "mimic natural rainfall conditions" but you never described those natural rainfall conditions.

Response:

Very few studies have been designed to assess microscale variations under controlled conditions and thus record increases in RR with rainfall intensity. There are several rainfall simulator experiments out there; however, our experiments are unique in the sense that they were designed to help us decipher the role of rainsplash on RR by isolating it from the role of other processes such as runoff, variable water content, bare soil surface, texture, etc. They also mimic natural rainfall conditions, as described in the Material and Methods (Comment 4). We have modified the text to better describe the uniqueness of our experiments (Page 10, lines 2-5):

"*Very few studies have been designed to assess microscale variation under controlled conditions to purposely examine increase in RR with rainfall intensity. Unique experiments are presented herein that were designed to help us decipher the role of rainsplash on increasing RR by isolating the role of other processes such as runoff, variable water content, bare soil surface, soil texture, etc*."

Comment 2: Page 8, lines 5-6: "which are confirmed as reliable descriptors of microroughness"; this is already known.

Response:

This phrase was removed.

Comment 3: Page 8, lines 7-9: I have doubts about this, you only performed your experiment once and considered a small surface where raindrop detachment prevails over runoff; were the same conditions in the other studies? Did they consider only raindrop detachment?

Response:

Experiments were performed 3 times. But we agree with the gist of your comment.

These lines probably come across too strong and may not reflect the main message of this study. As the spatial scale increases the effects of rainfall may not be dominant with time. When considering larger areas, runoff and concentrated flow (i.e., rills) will become more prevalent.

However, we all have to agree that during a storm, especially in the early part, there is a period when rainfall action may be more important than runoff, independent of location. It is hard to pinpoint the duration of that period because in some instances the soil may be saturated and runoff dominates right away. During that initial period, though, modeling the evolution of RR is important. This period is the focus of our controlled experiments and this study. The upslope area of the experimental plot provides the controlled conditions. We have therefore taken extra steps following a few other available studies (e.g., Zheng et al. 2014) to design tests where we can isolate the effects of rainfall on RR evolution. We discovered that RR does not always decay with intensity (and the kinetic energy of the raindrop) at all times but depending on the initial surface micorroughness condition RR can increase with time. This process can lead to the formation of depression storage which can ultimately affect ponding and lead to the formation of flow pathways.

All studies that reported on microroughness were performed at similar size experimental plots because this way they can isolate the effects of raindrop from runoff. Small plots suggest less area for runoff and formation of concentrated flow pathways. This is the reason in fact we have focused on a smaller section within the upper part of the plot where RR effects were dominant.

We have rephrased this statement to avoid confusion as follows (Pages 10, lines 5-10):

"The results obtained are consistent with findings of other studies that have examined length scales up to 5 mm. These length scales (i.e., ~2-5 mm) have been found to be common in agricultural landscapes due to prolonged exposure to rainfall impact, runoff and freeze-thaw cycles. Within these landscapes, the reported increase is expected to occur during the early part of the storm where rainsplash action may be more important than runoff."

Comment 4: Page 8, lines 11-12: What are the implications of this?

Response:

Please see the response to Comment 5 below, where the implications of localized microroughness residuals are better highlighted.

Comment 5: Page 8, line 13: "Roughness residuals infer depression storage residuals", what do you mean?

Response:

We suggest that localized microroughness residuals as shown from this study will leave additional depression storage at the surface prior to runoff generation. This may alter ponding and flow pathway patterns for storm events. The sentence (Page 10, lines 14-16) was altered to the following and the citations were added to the reference list:

"*Increase in microroughness further infers increase in depression storage at the soil surface prior to runoff generation (Kamphorst et al., 2000), which can significantly alter the ponding and flow pathway patterns especially at the onset of a storm event (Onstad, 1984).*"

Comment 6: Page 8, lines 15-20: I am not sure about this. Your results come from a limited number of experiments and you are implying that they have a strong importance in various disciplines and applications… it seems overestimating your findings.

Response:

We understand the reviewer's hesitation here and the potential overestimation of the findings' importance. This study's results, although being limited due to the level of work needed to complete them, consistently report a similar finding, which has been essentially undocumented to date. We feel that it will benefit and possibly further motivate environmental modeling and research, although maybe not directly other disciplines and applications. This part (Page 10, lines 16-18) has been modified as follows:

"*Our findings provide a better understanding of the highly dynamic phenomenon of soil surface microroughness evolution under the impact of rainfall. Our study motivates further research on the extent of influence of the examined phenomenon and its mathematical formulation for modeling applications.*"

Comment 7: Page 8, lines 21-23: This must be further explained, I do not see your point.

Response:

We point out that soil surface roughness both depends on and affects hydrologic response. Localized increases in microroughness create additional depression storage at the surface (Kamphorst et al., 2000) with potential implications to flow and pathway patterns (Gómez and Nearing, 2005). However, the extent to which soil surface roughness increases would affect depression storage, runoff, and flow pathways is unknown and further research to quantify this effect is needed. To be more concise, the last part of the paragraph was modified accordingly (Pages 10-11, lines 31-4) and the citations added to the reference list:

"*Finally, this study and other studies demonstrate that the evolution of soil surface roughness in response to rainfall is dependent on initial roughness conditions and rainfall intensity and can contribute to hydrology, i.e., another factor shaping the soil surface (e.g., through runoff). Different behavior of surface roughness evolution, i.e., increase or decrease, depending on initial roughness conditions indicates a dynamic and nonlinear feedback between hydrologic response and surface roughness which may affect depression storage, ponding and flow pathways (Kamphorst et al., 2000; Gómez and Nearing, 2005). However, the extent to which soil surface roughness increase would affect depression storage, runoff, and flow pathways is unknown and further research to quantify this effect is needed.*"

Comment 8: Page 8, line 24: Remove "study's".

Response:

It has been removed.

Comment 9: Page 8, lines 23-25: I really think that you are overestimating your results.

Response:

It has been shown through our own research and others (e.g., Gómez and Nearing, 2005) that due to the demonstrated interplay between roughness and hydrology at the microscale, we will see changes in runoff in terms of magnitude and timing. Most physically based models (e.g., WEPP) which assume a decrease in roughness following a rain event in all cases will have an error in the estimations of runoff and hence erosion. We do not infer that lack of consideration of our findings will necessarily or drastically affect hydrologic response. However, the addition of our results may address some of the discrepancies in the results. The last sentence (Page 11, lines 4-5) has been added:

"*Finally, this study and other studies demonstrate that the evolution of soil surface roughness in response to rainfall is dependent on initial roughness conditions and rainfall intensity and can contribute to hydrology, i.e., another factor shaping the soil surface (e.g., through runoff). Different behavior of surface roughness evolution, i.e., increase or decrease, depending on initial roughness conditions indicates a dynamic and nonlinear feedback between hydrologic response and surface roughness which may affect ponding and flow pathways (Gómez and Nearing, 2005). However, the extent to which soil surface roughness increase would affect depression storage, runoff, and flow pathways is unknown, and further research to quantify this effect is needed. Nonetheless, the current findings may help explain some modeling discrepancies in terms of depression storage and runoff predictions.*"

Comment 10: Page 8, lines 27-29: Yes, alright but is the initial roughness less than 2 mm? Besides, you indicate that Intensive Managed Landscapes have bare soil 75% of the time; it looks not very intensive…

Response:

As noted above, increase in surface roughness has been recorded for surfaces with initial microroughness of the order of 2-5 mm. Several studies have shown that landscapes with surface roughness of this order of magnitude are common in agricultural landscapes (Burwell et al., 1963; Allmaras et al., 1966; Burwell et al., 1969; Cogo, 1981; Currence and Lovely, 1970; Steichen, 1984; Unger, 1984; Zobeck and Onstad, 1987) due to long exposure to raindrop impact, runoff and freeze-thaw cycles (Zobeck and Onstad, 1987; Abaci and Papanicolaou, 2009).

Depending on the management (i.e., tillage intensity), the period between harvest and planting where the surface cover is bare will vary from 40 days to 6 months. It is understandable, when seeing the long period of inactivity between harvest and planting, to have misgivings with the term "intensively managed landscapes". However, even though intensively managed landscapes have essentially bare soil conditions from harvest to planting, the level of work that goes into planting, harvest, fertilization and other amendments is quite extensive in a short window. This is also quite demanding for the soil and microbes living in the soil, thus the term "intensive".

Comment 11: Page 8, line 33: "landscape response to precipitation"; however, your study refers only to 200 mm^2 surface… is this not overestimating your results?

Response:

We understand the reviewer's concern. We were lax with our wording and speaking generally. The term "landscape response" may be a stretch, especially since our paper deals heavily with scales. As mentioned before, we believe that our findings are applicable to the early parts of storm events when rainsplash effects are most dominant for soil surfaces with roughness on the order of 2-5 mm. The sentence (Page 11, lines 12-14) has been reworded as follows:

"*To the extent that microscale processes are considered significant, we argue that such models should adequately capture the increasing and decreasing trends in soil microroughness during all stages of a storm event in order to accurately predict local response to precipitation.*"

Comment 12: Page 9, line 1: Again overestimating the importance of your results. How this slight increase in RR may affect erosion processes?

Response:

Indeed, the extent to which the increase in RR recorded herein can affect erosion processes is not yet known. However, it has been noted that different values of RR can affect flow pathways and runoff, which consequently can affect erosion processes (Gómez and Nearing, 2005). The above has been documented in the text (Page 11, Lines 14-16):

Furthermore, the following sentence has been removed in order not to overstate the importance of our results of the study:

"~~From this standpoint, our study provides a much-needed insight into processes other than tillage that can result in increasing soil surface roughness after the surface is smoothened through natural weathering or surface erosion processes.~~"

Comment 13: Page 9, line 4: This needs, at least, a reference.

Response:

This section was deleted in order to maintain better focus on the main message of the study.

Comment 14: Page 9, line 5: "new statistical analyses", what statistical analyses did you perform?

Response:

Again, in order not to divert from the main message of the study, we have removed the sentence.

Comment 15: Page 9, line 6: I am not sure about what you mean by "is present a priori".

Response:

The sentence was modified as (Page 11, lines 16-17):

"*In fact, the majority of existing models assume that RR always decays over time with rainfall.*"

Comment 16: Page 9, line 7: "in the current paper" instead of "in the paper".

Response:

The change has been made.

Comment 17: Page 9, line 9: "is improved for current models", do you mean that is already done in current models?

Response:

Many existing models have parameterizations for the evolution of surface roughness (e.g., the Water Erosion Prediction Project, WEPP; Flanagan and Nearing, 1995). However, they do not account for the possibility that under certain microroughness conditions, rainfall may increase in roughness. Therefore, our study is calling attention to the need that current and future models must account for this condition of increasing roughness with rainfall over a smoothened surface. The ratios provided in this study are good first step for improving these models. The sentence (Page 11, lines 18-20) was reworded as:

"By providing the ratios of increase in roughness indices with rainfall intensity, the parameterization of the evolution of surface roughness with rainfall could be improved for current models."

Flanagan, D. C. and M. A. Nearing. USDA-Water Erosion Prediction Project: Hillslope profile and watershed model documentation, 10, NSERL report, 1995.

Comment 18: Page 9, lines 11-12: "extension of the experiments in areas such as downslopes where concentrated flow and rilling are of importance" That you did not want to account in your study although you could have done in view of the surface of your experimental plot.

Response:

In this study, we examined only a small slice of the problem by isolating the effects of rainfall on roughness for smooth bare soil surfaces to get a better understanding on this phenomenon. It was considered as an essential first step before extending the study to include the combined effects of flow concentration, rilling, and rainfall on surface roughness. The last sentence (Page 11, lines 20-23) has now been improved based on the above:

“*Future research will complement the present efforts by quantifying the evolution of microroughness under the collective action of rainfall and runoff. This will require a good understanding and quantification of the extent to which the initial increase in roughness in the early part of the storm could have an impact on flow pathways, runoff, and processes at subsequent parts of the storm.*”

5. References

Comments:

Eight references are not cited in the text. Please, check them and also edit the reference list according to the journal guidelines.

Page 10, lines 17-18: Chu et al. (2012) is not cited in the text.

Page 10, lines 20-23: Why did you use upper-case letters for the title of these publications?

Page 10, line 27: Why did you use capital letters for CATENA?

Page 10, lines 30-31: Why did you use upper-case letters for the title of this publication?

Page 11, lines 1-3: Le Bissonais (2016) is not cited in the text.

Page 11, lines 4-5, 10 and 17-18: Why did you use upper-case letters for the title of these publications?

Page 11, line 23: Why did you use capital letters for SOIL ORGANIC CARBON DYNAMICS?

Page 11, line 28: Potter (1990), this reference does not follow the style of the journal; the year of publication should come at the end.

Page 11, line 29: Why did you use upper-case letters for the title of this publication?

Page 11, line 32: Why did you use capital letters for CATENA? Besides, Römkens et al. (2002) is not cited in the text.

Page 12, lines 4-5: Remove the quotation marks.

Page 12, lines 8-9: Vázquez et al. (2006) is not cited in the text.

Page 12, line 11: Remove “European Geo-sciences Union (EGU)”.

Page 12, lines 15-17: Vázquez et al. (2010) is not cited in the text.

Page 12, lines 18-19 and 25-28: Why did you use upper-case letters for the title of these publications?

Page 12, lines 22-28: Zhao et al. (2014), Zhao et al. (2016) and Zheng et al. (2012) are not cited in the text.

Response:

For the comments regarding the references, we have addressed them all as requested by the reviewer. In summary, we consistently followed the journal guidelines in terms of formatting, removed the uncited references, and added all new references during the major revision process of the manuscript. Please see the updated version of the References (Pages 12-15).

6. Figures

Comments:

Figure 1: Modify the caption to "(a) Types of soil surface microroughness. (b) Experimental plot. The rainfall simulator is placed above the bare soil surface and a base made of wood is put into place to facilitate the movement of the surface-profile laser scanner".

Figure 2: Modify the caption to "Setup of the experimental tests: (a) Rainfall simulators are mounted in series and a pump provides them with water from a tank; (b) rainfall simulators are placed and adjusted at a height of 2.5 m above the experimental plot surface to ensure drop terminal velocity is reached".

Figure 3: Indicate in the caption the interpolation technique that was used.

Figure 4: Why not a and b as in the former figures and you used left and right? You should define ROI in the caption. Besides, why "part"? If the whole experimental plot was 7 x 1.2 m is the whole plot what you are representing in the right-hand side of the figure and not only part of it.

Figure 5: Remove "considered herein". I think that you do not need to include experiments 1, 2 and 3 if you indicate the rainfall intensity. Remove the border of the figure and the second decimal from the Y-axis.

Figure 6: Remove "Spatial" and "considered herein". Use "region of interest" instead of "ROI".

Figure 7: Remove "considered herein". I think that you do not need to include experiments 1, 2 and 3 if you indicate the rainfall intensity. Remove the border of the figure and the second decimal from the Y-axis.

Response:

Regarding the comments to the figures, which are listed above, we have addressed them all as requested by the reviewers. These comments were all essential, but for the most part cosmetic and do not need further elaboration. Figure 5 was updated and is described in the Results Comment 2 above. Finally, Figure 7 was removed since it was considered as unnecessary. Please see the updated version of the figures (Pages 16-20).