RE: NHESS 2017 20

Imaizumi et al. Interactions between the accumulation of sediment storage and debris flow characteristics in a debris-flow initiation zone, Ohya landslide body, Japan

Overview

The authors present a study that link the debris flow types to the morphological characteristics of the initiation zone and the entrainment and deposition processes to the debris flow types. The writer identified the following deficiencies:

1) The text, in some parts, is not fluid but hard to read. It looks like an ensemble of pieces more and less linked each other.

2) Authors through a static equilibrium-based relationship (equation 5), obtain angles corresponding to different debris flow types or the sediment concentration of debris flow corresponding to bed slope angles, if the other quantities are known. If used for dynamic computation (i.e. the sediment volume concentration of the flowing material) this equation is misleading. In the case of motion, the equation (5) is a bit different (Lanzoni et al., 2017) and is obtained through the ratio between the basal bed shear stress and the basal normal stress. Moreover in the case of flowing material, the angle ϕ is not the static friction angle but the quasi-static of dynamic friction angle (Lanzoni et al., 2017). Therefore, also the sentence "Thus, not only…..Eq(5)", is uncorrected. Therefore, I suggest the authors to write that an equation of the same structure of eq. (5) can be obtained through the ratio between the basal bed shear stress and the basal normal stress that have a similar structure to the ration between shear stress and strength (Lanzoni et al., 2017).

3) About triggering of debris flow, both the experiments of Gregoretti (2000) and the theoretical computations of Gregoretti (2008) clearly show that the entrainment of debris material into a water stream is provided by the surficial erosion of the debris layer rather than by the slide of the debris layer. Moreover, field works of Berti and Simoni (2005) , Cannon et al. (2008), Coe et al. (2008), Gregoretti and Dalla Fontana (2008), Theule et al. (2012), Hurliman et al. (2014), Degetto et al. (2015), Hu et al. (2016) point to a triggering mechanism where runoff erode the sediments and spread them along flow depth rather than the slide of a saturated mass. Recent works of Gregoretti et al. (2016) and Rengers et al. (2016) show that runoff descending from cliffs is an impulsive phenomenon characterized

by a peaked hydrograph and that the impact of peak against debris deposits entrain enough material to have a solid-liquid current. Finally, authors cited the work of Kean et al. (2013) and Navratil et al. (2013). In the first work, it is expressly written that debris flow initiation by surface runoff is different from the debris flow initiation by shallow landslide (the title of the work deals with runoff-generated debris flow), while in the second it is stated that debris flow is initiated by surface runoff rather than landslide. Therefore, if the authors intend to use their slide model, they should state that channelized debris flow initiate by runoff as a surficial erosion (cite the references above) and that they approximate it by a “slide” model using the calculation that Prancevic et al. (2014) show.

4) About 6.1, the writer agrees with authors that sediment availability determines the type of debris flows but has some concern about partially saturated debris flow even if stated by other authors (e.g. Iverson and Vallance, 2001). They could be very dense debris flows, where fluid phase is just under the surficial sediments. Measurements carried out at Illgraben (Mc Ardell et al., 2007) show a flow density of the front that approximates that of a saturated terrain. An alternative way is that of very dense debris flow rich of debris material and more fluid debris flow. About the outcome of the correspondence between short-lasting rainfall and partially saturated debris flows and that between long-lasting rainfalls and saturated debris flows, the field experiences in the initiation area Cancia debris flow given by Bernard et al. (2016) and Gregoretti et al. (2016) seem to contradict it. At Rovina di Cancia (Northeastern Italian Alps) an hyperconcentrated flow occurred about 11 days later a partially saturated debris flows (according to the author definition; see the video at the following link https://www.youtube.com/watch?v=oKQSZVwOuRo). The rainfall depths were just a bit smaller in the second event even if the terrain was not dry as for the first event (Gregoretti et al., 2016). The main reason is that after the first event, channel did not recharge and the quantity of entrained sediments in the second event in the initiation reach was at least an order smaller than that entrained during the first event.

The following are the detailed comments and specifications.

1. Page 1 – line 20: the sentence “The small-scale channel gradient….” is unclear.
2. Page 1 - line 26: has been
3. Page 1 - line 28: the word “activities” after “monitoring” is missing.

4. Page 2 - line 1: some parts of the sentence “Understanding ………..system” are unclear.
5. Page 2 – line 4: perhaps the reference Takahashi 2007 is better than Takahashi 1991.
6. Page 2 - line 18: add the reference Gregoretti and Dalla Fontana (2008).
7. Page 4 – line 5: what does it mean “is the exception ….. between α and ϕ?”.
8. Page 4 – line 30: the sentence “The explanations…..2014)” is unclear as it regards the terrain formed by the sediment mass: what does it means? Moreover, the relationship given by Takahashi (2014) should be written.
9. Page 6 – lines 9-11: the sentence “ Most of the channel…..low” is bad written and misleading.
10. Page 7 – line 8: P2 is not in fig. 2a but in fig. 2b, 142-143: the finding of Chen (1991) could be due to the increase of melting water, while in the present case debris flow has been triggered by rain storm.
11. Page 8 – lines 2 and 6: perhaps mean flow depth velocity or flow depth averaged velocity should be better than “mean velocity of all layers of the flow”.
12. Page 9 – line 14: the sentence “The location of …” is bad written.
13. Page 11 – line 5 and following: which technique did used the authors to obtain the map of storage through the photographs taken at P1 and P4?
14. Page 12 – line 3: substitute “section between sites” with “reach between the sites”.
15. Page 12 – line 9: “high value of total rainfall depth” instead of “high total rainfall”
16. Page 12 – line 10: “characterized by high intensity” instead of “of high intensity”.
17. Page 12 – line 14: “varied between the events” instead of “differed between events”.
18. Page 12 – lines 14-16: the sentence “For example……(Fig. 5)” should be rewritten as: For example in the event of 5 August 2008, 88% of debris flow surges (percentage respect to the total event duration) was composed of partly saturated flow, while in the event of 30 August 2004 (Fig. 5), 90% in time of the phenomenon was composed of fully saturated flow.
19. Page 12 – lines (16-19): sentence bad written: what does it mean the relation between camera location and debris flow characteristics?
20. Page 12 – lines (19-24): sentences very bad written; please rewrite them stating that the typology of rainfall determined the flow typology.
21. Page 14- lines (8-9): the writer does not understand the sentence “The roughness of the ground surface attributable to boulders…….” How the roughness that is a length can be estimated by a slope gradient map? Moreover, the authors should introduce the definition of slope gradient or explain it.

22. Page 14 – line 13: authors should specify that obtained the DEM after using TLS data and the method they used for determining the slope gradient.
23. Page 14 – line 14: please place "the" before highest
24. Page 14 – line 17: talus slope is the slope gradient?
25. Page 15 – caption of Figure 7: calculated after using TLS data…..
26. Page 16 – lines (1-8): the writer does not understand what the authors mean. In other words, what does it the meaning of the channel topography forming after debris flow occurrence?. It means that all the channel was flooded by debris flow or that the debris flow changed the bed.
27. Page 16 – lines (9-14): even in these sentences the writer does not understand what the authors mean. Visual inspection of the two bed profiles of Figure 9a (are they from post-event surveys?) show that there is an high lowering of the bed profiles at the beginning and ending reaches. Authors should provide a much better description/comment of this figure.
28. Page 17 – caption of Figure 9: authors should specify that the profiles correspond to post-event surveys.
29. Pages 19-20. All the comments about α_1, α_2 the storage volume and slope gradient should consider that the middle part of the channel was not interested by erosion phenomena. Therefore, the authors should explain a reason and then exclude it from the further comments. Moreover, the sentence between pages 19 and 20 is not clear.
30. Page 21 - lines 14-15. The explanation on the effect of grid size on hystogram shape should be coupled with some estimation of roughness (i.e. median grain size or something else) . Moreover, the authors should initiate the subsection explaining the reasons they produced the histograms.
31. Pages 21-22: the writer does not understand well the scope of the written sentences: authors should clearly rewrite them. For istance, fully saturated debris flow removed only fine sediments while partially saturated debris flows washed out entire bottom reaches. The writer thinks that the amount of entrained material depend also on the main characteristics of the of the solid-liquid current: flow depth, sediment concentration and velocity that in the case of coarse grained debris flow depends also on the runoff discharge (Lanzoni et al., 2017).

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