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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Dynamics of large wood during a flash flood in two mountain catchments" *by* A. Lucía et al.

A. Lucía et al.

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We wish to thank the reviewer for her/his valuable comments and suggestions. Below are the answers to his/her comments, which are reported before our response.

1) The authors discussed about the accurate estimation of stream width and the use of variables related to it such as unit stream power (Section 3, from line 25, page 1651). I agree that this could be disputable, but the same discussion could be applied for the estimation of the slope and the use of stream power. The authors used a pre-event slope, but the slope could also change significantly in Gravegnola. Do the authors think that stream power could be overestimated when the slope extracted from the pre-flood DEM is used? They measured the slope and width during the post event survey (Lines

6-7 in page 1649), did you compare pre and post values? Could this influence the final results (i.e. relationships between SPI and LW? About the final correlations (Table 2), the authors showed correlation between LW variables and slope, area, stream power and SPI, but actually the slope and the area are used to calculate the other variables, so they are not independent variables.

Although the flood could have caused some local variations of channel slope due to incision (increase of slope) or aggradation (decrease of slope), these changes are typically small at the channel reach scale, which is the scale adopted in this study. As stated in the Lines 6-7 in page 1649 of the original submitted manuscript, we have measured the longitudinal slope in the field using a laser rangefinder, but this was only possible in some of the reaches. In the remaining reaches, slope measurement in the field was not been possible due to the length, sinuosity and presence of vegetation. The only information of the slope before the event can be derived from topographic maps or from the DEM (10 m of resolution). There are differences between the slope measured in the field and the slope calculated from the DEM, but these differences could be due not only to the variations during the flow, but also to the different origin and accuracy of the measurements, which would hamper the comparison between pre-flood slope from the DEM and post-flood slope from field surveys. Therefore, based on the likely little variations of channel slope and on these methodological issues, it was decided to use the slope extracted from the DEM for all the analysis. We will modify the text to specify this choice in the methods section. As to the correlation among the controlling factors due to the presence of drainage area and slope in the stream power and stream power index, this is not an issue in our opinion because the dependent variables (LW related) are not calculated nor normalized based on these two variables. Indeed, we think it is quite interesting to seek which controlling factor leads to higher correlation with LW dynamics.

2) How do you explain the relationship between LW recruited in the slopes by landslides with the stream slope or stream power?

We believe it may stem from the fact that the steeper reaches (typically featuring also higher SP) tended to incise their bed during the event, thus causing erosion of hillslope toes, favoring mass wasting. Also, steeper reaches are typically more confined, hence more likely to interact with the adjacent hillslopes. This sentence will be included in the results section.

3) How do you estimate the LW input from upstream used to calculate the LW exported? (Table A1)

The LW input to a reach from the channel upstream corresponds to the LW exported from the upstream reach. A row will be included in the Table A1 for LW input.

4) Regarding the flood and its magnitude reconstruction, the authors assigned a very high return period (particularly for the Cassana Creek, about 200-500 years). I wonder how they estimated these values; is there any stream gauge in the area? If so, where is it? What discharge values were recorded?

The return period of the flood of October 25, 2011, was estimated by comparing peak discharges assessed by means of post-flood surveys with the application of regional equations relating peak discharge (corresponding to various return periods) to drainage area. This explanation will be included in the text. Unfortunately no stream gauges are present in the two rivers, but only in the much larger Magra River where recurrence interval was estimated up to 200 yr (see Nardi and Rinaldi, 2015).

5) The mapping of the eroded areas and those affected by landslides was carried out using post flood imagery and field survey, and they also used pre-flood pictures for measuring widening (as the difference between channel width before and after the event). How is the different resolution of the images influencing the mapping? And why they only mapped the LW deposits in Gravegnola Creek? It is not very clear how the authors measured LW in Pogliaschina.

The resolution in the post-event photos was 0.1 m and in the pre-event 0.5 m. The

problem was not so much related to the difference in resolution, but it was more difficult to deal with the presence of vegetation that practically covered the channels in the pre event photos. Therefore, when it was not visible, the pre-event width was assigned as a range and the uncertainties derived from this range were taken into account. As explained in the page 1649 (lines 16-21), LW in Pogliaschina was measured in the field only.

6) It could be very interesting to compare the size of logs in both, Gravegnola and Pogliaschina In both basins the vegetation distribution is slightly different; could this difference influence the recruitment and transport of wood?

Unfortunately, it was not possible to measure the LW size in the Pogliaschina due to the lower quality of the post event aerial photos (as explained in the 1649, lines 16-21) of the submitted manuscript. However, based on the field inspections, the two basins seemed not to differ much in terms of log size, as both floodplain and hillslope vegetation was quite similar in most of the recruitment areas.

7) The authors said that they measured diameters and heights of living trees (Lines 21-22 page 1649), what these data were used for? Did you compare the size of deposited logs with the dimensions of trees in the forest?

Because these data were not used later in the paper, the sentence will be deleted.

8) Bridges were playing an important role in wood deposition. Could the authors place them maybe in Figure 7? This will help to understand the spatial pattern of LW deposits.

Yes, we will include the bridges in the Figure. Thanks for the suggestion.

9) In the conclusions, Section 6, the authors discussed about (see my first comment about this in Technical corrections) the connectivity between slopes and streams and they affirmed that the first attempt to this was proposed by Lucia et al. (2015b) in Line 27, this is true if they refer to the use of the connectivity method proposed by Cavalli et al 2013, because Mazzorana et al., 2009, Rigon et al., 2012, Ruiz-Villanueva et al

2014 already included connectivity in their methods for LW recruitment estimation.

The mentioned studies certainly included connectivity in their methods but they only focused on lateral (hillslope-channel) coupling relationship. The model proposed by Lucía et al., 2015 includes also within-channel (longitudinal) LW connectivity, i.e. the one affecting the propagation of the LW along the channel. This will be specified in the text

10) In the same section, line 6, the authors mentioned maintenance of riparian vegetation, what do you exactly mean? What type of management would you suggest to reduce recruited wood volumes? This is of importance and an ongoing debate among scientists and river managers. Finally, I totally agree with the affirmation that the infrastructures design has to be re-defined, and about this I would also cite the work by Lassettre and Kondolf 2012 (River Research and Applications, Volume 28, Issue 9, pages 1477–1487).

We will specify in the text that the vegetation maintenance corresponds to the periodic cuts (more or less selective). As will be stated in the text, these periodic cuts could reduce the LW volume recruited by channel dynamics during a flood, but they cannot reduce the LW recruited from the slopes due to mass wasting, which can be high enough in many mountain basins. Therefore we think that the most reliable solution are: i) a renewed approach to bridge design, and ii) the installation of wood retention structures. We will include the suggested reference, thank you.

11) I found some paragraphs describing methods that are actually in the results section. As an example all statistical tests are described in different result sections (sections 4.3, 4.4 and 4.5). I would recommend describing the data analysis, including all statistical tests, in Section 3, and then just results in Section 4.

This has been noticed also by reviewer 2. We will modify the text as suggested, and we will also subdivide the methods into subsections to make it clearer for the reader

12) If the paragraph about methods in section 4.3 is removed, the remained text about the two basins could be located in section 4.1, where they described the flood event.

We would prefer to maintain the former section 4.3 as separate, moving it just after the section describing the flood event

13) I also found some results in the discussion section, such as those related to LW deposition (section 5.2, line 21-24)

The reviewer is correct; we will move this part to section 4.3

14) The part of the last paragraph in page 1651 and first paragraph in page 1652 could be better located in the discussion section, around line 17 in section 5.1, where they actually wrote about this issue as follows: . . .as discussed in the methods section. . .

The paragraph will be restructured as suggested the reviewer

15) Moreover, some parts of the conclusions section are still discussions (those related to the references). I think this section could be shortened or the main findings and conclusions better highlighted.

We will shorten the conclusion, highlighting the main findings

16) I would suggest summarizing the basins characteristics (area, altitude, slope, forested area, main vegetation, studied river reaches length etc.) in a Table.

Good suggestion, we will include a new Table.

17) Table 1: I would recommend to add one row with the header of Channel variables and LW variables instead of explaining this in the caption: . . .(2nd-5th column) and LW-related (6th-12th column).

Ok, we will modify the table accordingly

18) Figure 1 and 3 could be maybe combined in one single figure?

Yes, we will group Figure 1a and 1b and Figures 3b and 3c to form a new figure 1

19) The image 10A and 4A are the same, do the authors have any other example to show in figure 10 instead?

Yes, we will include a different image in Figure 10.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 1643, 2015.