

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

Anonymous Referee #1

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General comments:

The manuscript presented by Lucia et al. shows a very interesting study of recruitment and storage of large wood during an extreme flash flood in two mountain basins in the Apennines. As pointed out by the authors, there are few papers documenting in detailed the dynamics of large wood after extreme floods, therefore the manuscript represents an important contribution. In my opinion the manuscript fits very well with the journal scopes, so I would recommend it for publication after some revisions. I provide here some suggestions and comments.

Specific comments:

The authors discussed about the accurate estimation of stream width and the use of
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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. In addition, how do you explain the relationship between LW recruited in the slopes by landslides with the stream slope or stream power? How do you estimate the LW input from upstream used to calculate the LW exported? (Table A1) 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 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. 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? And in addition, 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? 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. 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. 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 Lassetre and Kondolf 2012 (River Research and Applications, Volume 28, Issue 9, pages 1477–1487).

Technical corrections:

One general concern is regarding the general structure of the manuscript. 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. 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. I also found some results in the discussion section, such as those related to LW deposition (section 5.2, line 21-24). 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. . .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. I would suggest summarizing the basins characteristics (area, altitude, slope, forested

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area, main vegetation, studied river reaches length etc.) in a Table. 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). . . Figure 1 and 3 could be maybe combined in one single figure? The image 10A and 4A are the same, do the authors have any other example to show in figure 10 instead?

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