Authors´ responses to B. MacVicar (Referee #1)

We thank the reviewer, Bruce MacVicar, for his helpful commentary. We have responded to his comments in the attached supplement, and we have revised the manuscript accordingly.

Overall comments: This study is a detailed analysis of wood and morphologic response to a large flood in a Swiss River. Complemented by a rapid mobilization to obtain the appropriate data, the study does a good job of addressing the fundamental need for data to understand the dynamics and risks associated with wood in rivers. As noted by the authors, there are very few studies with which to compare as the data is typically not available after a big flood to generate a reasonable budget. For these reasons I think this is a valuable addition to the literature on wood in rivers and worthy of publication. I have a number of minor suggestions for grammar and points for clarification that should be easily addressed by the authors. I do have a few points that may warrant some consideration for framing the results and the discussion.

Authors' response: We are grateful for the constructive and detailed commentary provided by the reviewer. We have revised the manuscript to address the reviewer comments and responded to those comments below.

The first is that I don't think it is worth discussing insignificant trends. There is a lot of data here and I appreciate that it is difficult to tease out highly significant relations between the variables you use, but the fact is even the best model only explains less than half of the variability. There should be more discussion about what might be missing and why only a few things are significant rather than reaching for things that have been shown to not be important. Alternatively you could argue that the explanatory variables themselves are uncertain.

Authors' response: We agree with this comment. We tried to better discuss the uncertainties and to remove discussion about non-significant variables. Although in general terms we observed a large scatter in our data and that not all variables were significant, we did observe significant correlations, and some of the models explained up to 70% of the variability. We tried to better highlight the most meaningful results in the revised manuscript.

The confinement index, for example, relies on dem quality and the ability of the algorithm to accurately delineate the floodplain. If you think that there is a lot of error in that variable then maybe the error is partially obscuring the 'real' trend and it could be justified to look at trends that are close to significant.

Authors' response: The reviewer is right with this comment. There are of course uncertainties in the results given by the algorithm used to estimate the valley width. However, we do not think there is a lot of error in that variable. The DEM we used for the analysis is 2 m spatial resolution, which we believe is good enough to delineate the valley width. The quality of the results was checked by a visual evaluation, comparing the aerial images with the obtained valley delineation. We discuss this in the text.

The second point is that I think the study provides an opportunity to test some conceptual models such as that of Seo and Nakamura (2009) where they separate channels into different classes to explain differences in wood dynamics. It would be useful to understand whether this model holds for an individual flood and whether the morphologic variables used could be used to divide the channels into two or three classes rather than the rather arbitrary division used in the study of the Emme and everything else. Alternatively, the decision to group the channels as you did could be supported by using the conceptual model to justify the choice.

Authors' response: We thank the reviewer for this comment, and we appreciate the suggestion of bringing up new avenues of thought for considering our results. We considered Seo and Nakamura´s conceptual model, but we unfortunately could not apply this model to our data. They observed that LW dynamics (in terms of LW recruitment, transport, storage, and fragmentation and decay) varied along the gradient of watershed size. They classified their data in three size groups, small (<20km^2), medium (20-100km^2) and large (>100km^2) watersheds. A similar classification cannot be done with our dataset, as the largest watershed has an area of less than 100 km^2 (96km^2). Still, we grouped our dataset in the two smaller classes, small and medium watersheds, but we could not find significant differences in the variables analysed by Seo and Nakamura. The following figure shows our dataset grouped by the watershed size:

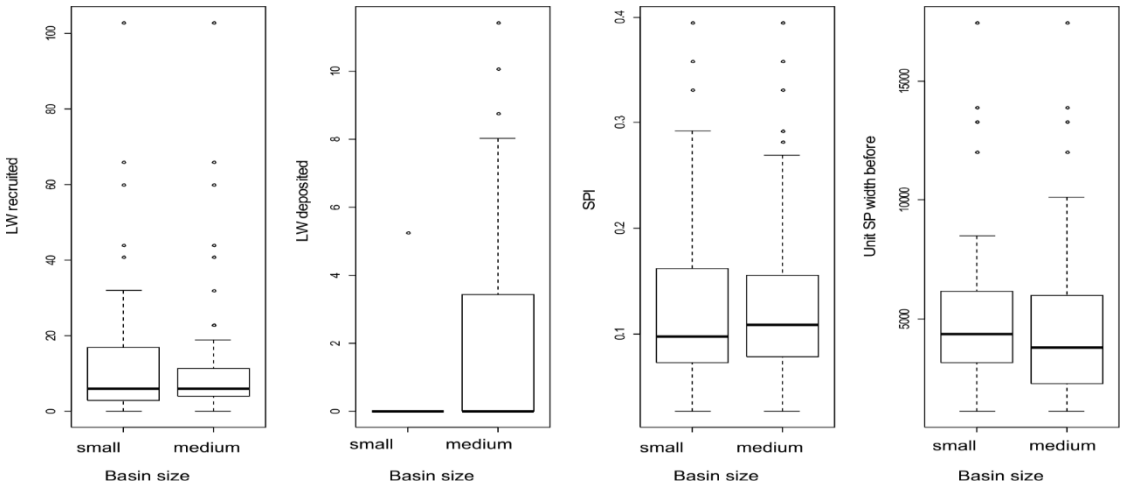

Figure: Boxplots of LW recruited (m^3/ha) and deposited volume (m^3/ha), stream power index and unit stream power calculated with the estimated discharge and the initial channel width for small and medium watersheds following the classification by Seo and Nakamura (2009).

We observed slightly similar trends to the trends in Seo and Nakamura in our data, regarding LW recruitment, which was much higher in the small watersheds (Emme tributaries) than in the larger watersheds (mostly the Emme subreaches and some larger tributaries), as it is shown in the figure above and in the Figure 7 in the manuscript. However, differences are not significant. LW storage was only analysed along the main stem of the Emme River and in two tributaries, so our dataset is not suitable to test the conceptual model proposed by these researchers. The same observation made for recruitment is also true for stream power. As the figure above shows, SPI (which is just based on drainage area and channel width) and unit stream power do not show significant differences between small and medium watersheds.

We added some discussion about that in the revised manuscript.

Please also note the supplement to this comment: https://www.earth-surf-dynam-discuss.net/esurf-2018-44/esurf-2018-44-RC1-supplement.pdf

Authors' response: Thank you, we accepted all suggestions made by the reviewer concerning the grammar and English usage and added them to the revised version of the manuscript.

Authors´ responses to Anonymous Referee #2

We thank the Anonymous Reviewer for the detailed revision of the manuscript and the useful comments. We have responded to all comments in the supplement document, and we have revised the manuscript following her/his recommendations.

This paper studied the channel widening and LW dynamics through a post-event survey carried out after a large flood in a mountain river basin in Switzerland during the summer 2014. After detailed statistical analysis the authors find out control factors for the studied phenomena, among which they highlight the precipitation. The paper is well written and structured and presents a solid methodology and the results presented in this paper are highly valued to better understand this kind of phenomena. The kind of data presented in this paper are highly valuable because, as commented in the paper, the data are still scarce and relevant for river management. Therefore, I suggest the paper to be published. However, I have few comments that I hope help the authors to improve the present manuscript and make it clearer for the reader. These comments are divided in sections.

Authors' response: We appreciate the positive evaluation.

GENERAL COMMENTS I think that the authors talk about flood response but they mainly analysed channel widening (not other phenomena such as landslides) so I would suggest specifying this in the title, as well as in the text and use channel widening rather than flood or channel response.

Authors' response: The reviewer is partially right with this comment. We mostly analysed channel widening because this was clearly the main geomorphic process triggered by the event. Few shallow landslides and debris flows occurred during the event and they were also included in our analysis, but they were of minor importance in terms of LW dynamics. We therefore changed the title and the text following the reviewer's recommendation.

INTRODUCTION The introduction is very complete and well structured.

Line 85. The specific aim described here is not tackled in the rest of the manuscript (there is no quantitative information of debris flows, landslides etc, so I suggest rewriting this to focus the goal on the channel widening from the beginning.

Authors' response: We edited the text accordingly. We removed this sentence and clarified our goals (focused on channel widening and LW dynamics).

Line 92. The hypothesis is not very clear, in which terms do you describe similarity of the river? What it seems quite obvious is that if you analyse similar rivers (from the morphology of the channel and the catchment) one that was affected by severe precipitation and other that did not, the response would be different. Maybe you can specify better this, if they had similar morphology and similar precipitation, but their response to the flood is different, and in this case, what would explain the difference.

Authors' response: Agreed. We rewrote this paragraph, also following the suggestion by referee #1.

Line 93. Maybe you could already introduce which parameters you used for the analysis.

Authors' response: We added the parameters.

MATERIAL AND METHODS This section leads to some issues that are not clear, I have tried to point them out so that the authors can clarify them.

Line 121. Maybe you can move this paragraph together with the first paragraph of the section. Also, you can refer to the different parts of the figure in the text, like (Fig 1.b) in the paragraph of the line 105 and the Fig 1.c in the paragraph of the line 101.

Authors' response: We thank for the comment, and refer to the figure accordingly. The paragraph was moved to the beginning of the section as suggested.

Fig.1. This is a good figure. The photos d and e are easy to locate with the caption, but not the f., Räbeli bridge is located near Räbloch?

Authors' response: The reviewer is right, we edited the figure and added the location of the sites shown in figures 1 d, e, and f. By doing this, we also changed the order of the figures to place them in a downstream direction order.

Table 1. This table has a lot of information and I am not sure if I is relevant, like the sub reaches and the Total number of transects, maybe you can consider removing them.

Authors' response: It is true that this table has a lot of information; we consider most of it as relevant. Hence, we removed the number of transects as suggested by the reviewer, but not the subreaches as they are used in some of the manuscript´s figures.

Table 2. In the note of the table (*) you talked about two sections, but it is only marked in Räbloch. Maye or comparison you could add a column with the specific peak discharge (m3s-1km-2), but it is true that it can be calculated with the data present on the table. Maybe you could include these sections in the map of the Figure 1 similarly as you did with the raingauges.

Authors' response: We added the specific peak discharge to Table 2 and we added also the location of the cross sections in Figure 1 (by doing this we removed some tributaries names), indicating them with numbers (these numbers were also added to Table 2).

Line 228. I suggest deleting "and mapped in GIS" and talk about it more in detail in the next section. Maybe you can give more details about how you measured the wood deposit and where. Also it would be interesting to know more in detail (maybe in the GIS section) about how you extrapolated the information from two tributaries and the Emme river to the rest of the basin.

Authors' response: We edited the text to clarify our methods. We did not extrapolate any information from the two tributaries to other parts of the catchment. Some variables were only measured in the field, some only measured using remote sensed data, but nothing was extrapolated. We also clarify this aspect in the revised version of the manuscript.

Line 233, change <10 cm to >10 cm so it is according to the definition made in the line 231. The classification into size classes was made in the field for all the LW deposited pieces in the studied reaches?

Authors' response: We corrected this accordingly. Yes, the classification was made in the field.

Line 152, maybe you can describe the mean max and minimum length of the sub reaches.

Authors' response: We added these values in the text and provided the length of the sub-reaches in Table 1.

Line 260. Could you specify which where the post flood units? Did you map them as polygons?

Authors' response: the units were in meters, as we used the cross sectional transects (lines) and cross sections in the field, as explained in the text.

Line 262. It is not very clear for me why you used transects for obtaining the width if you had already mapped the channel before and after the event and had the length of the channel or centreline you could calculate the average width of the subreaches. In the case of the transects, the different parameters are measured along exactly the same transects? What was the active channel used to obtain the centreline, the preor the post-event? If they are the same, it may be that the width is not measured perpendicularly to the flow in one or the other case. Cold you also explain why the transects interval ranged from 20 to 50 and why?

Authors' response: Channel width was measured using the exact same points for the transects, checking that all were perpendicular to the corresponding centreline (before or after flood), and data was validated with field observations when available. We clarify this in the text.

Line 277, could you specify where this equation is coming from (the regression coefficients and so)? I think that in the case of this catchment, where the precipitation had big variations, using an approach based only on the Area could lead non realistic data. Maybe you could apply a simple rainfall-runoff model calibrated with the data of the Table 2 to better estimate the peak discharge or specific peak discharge of each tributary.

Authors' response: The equation is obtained using the data presented in Table 2. We agree that this is not the most robust approach, and this is why we did not use the stream power calculated using the estimated discharges. Instead, we used the SPI and we discuss the limitations associated with the use of this proxy. A rainfall-runoff model would be more accurate; however, this is out of the scope of this study. However, it could be considered for further analyses. We added some discussion about this issue in the revised manuscript.

Line 282. The use of SPI may be appropriate when you have similar specific peak discharges in the catchment, but in this case, when there are large variations in precipitation (and possibly discharge) to relate the stream power just to morphological characteristics of the catchments could lead to errors. Maybe you can discuss on this.

Authors' response: Yes, the reviewer is completely right. We better highlighted this point in the discussion.

Line 282. This comment is not for this line, but I think you should add also in this part how you calculated the sinuosity.

Authors' response: We added the explanation about the sinuosity parameter in the methods section.

Line 284. I would add also discharge or stream power here, even if in the end you did not see the correlations.

Authors' response: We added discharge, but not stream power because it is calculated based on the already mentioned variables. We moved this paragraph to the end of the introduction as suggested in the following comment.

Line 287-289. I think that this hypothesis should be better in the introduction rather than in the methods section. However I think this hypothesis is a little weak, maybe you could add the precipitation could be considered in case that the morphology, landuse and the geology of the catchments compared were homogeneous, otherwise the hydrological response of the channel would be different. I agree with the authors that having the chance to understand complex responses through the rainfall is a nice starting point, because the data on precipitation is much easier to get than peak discharges after an event, but I think that an accurate data on discharges (complemented with other morphological variables) could lead to better estimations.

Authors' response: Yes, thank you for this suggestion. We moved the paragraph to the introduction. We also discussed our hypothesis more deeply in the discussion.

Line 304. Specify which channel area did you used to normalize the wood volume (pre or post-event)

Authors' response: We specified this in the text. We used initial channel area (pre-flood).

Line 307. Add the date when the video was filmed

Authors' response: We added this information to the text and to the supplementary material. The videos were recorded on the day of the flood. The links to some of these videos are also provided in the supplementary material. In addition, pictures and images from other sources were also used.

Line 309. Could you calculate the volume deposited at each site or the total volume for each subreach with that information?

Authors' response: We estimated the volume of deposited LW at each site identified in the media dataset and in the field. Then we summed up all volumes and aggregated the data to the subreach scale.

RESULTS The analysis presented in this section are very interested, however the amount of information provided in the manuscript and in the supplements can be overwhelming for the reader and it may benefit from a reduction of the amount of data provided and analysed that are not useful for the paper. For example, in the Figures with Spearman Rank correlations (that could be named as tables), the authors may consider to avoid showing all the correlation of all the variables, since some of them are not meaningful, this can be done by showing the correlation between analysed parameters and analysed control factors (some of them, like the widening ration, can be in both sides, but it does not make sense to show the correlation of the mean and max precipitation, for instance. Another example is the regression graphics of parameter with control factors that have been shown to not to have correlation (ie. figure 7. b and c). Also the logistic regressions table provide large information of data that are shown in the end to not to be that explanatory, I would suggest to try to summarize to the main points that are relevant and specify more clearly the maybe in the results but also in the discussion the limitations of these variables to be used in the future (due to the large scatters and uncertainties)

Authors' response: We appreciate the comment, and we are aware about the large amount of information provided in the results. However, we consider that the information shown in this section is relevant to understand our results, our discussion and conclusions. Concerning Figure 5, we show here all the variables analysed, and the matrix shows all correlations between them. The reviewer is right, that showing the correlation between mean and maximum precipitation might not be very useful, but this is only one single cell in the matrix, while, precipitation correlation with other variables is very relevant. We gave this issue considerable thought and decided not to simplify this figure.,

Similar reasoning applies to Figure 7. This figure shows all the data we collected in this study. In contrast to other figures where we selected only relevant variables (e.g., Figures 6 and 8), we believe it is important to show in one figure all our data together.

Regarding the logistic regression results, we agree that they are not so relevant. That is why we provide these results only in the supplementary material, and only summarized it in the main text.

Line 348. In the S5 figure caption maybe you can add which statistical test you used for the analysis of the differences among the Emme and Tributaries.

Authors' response: This information was added to the caption.

Line 363. Maybe you can explain why you highlight these channels in the figure 4.

Authors' response: In Figure 4, we labelled subreaches showing the largest values. We added this in the caption of Figure 4.

Figure 4. Is there any reason why some subreaches are highlighted with numbers and not the Sadelgrabe and the Gärtelbach?

Authors' response: We named the two main tributaries (Sädelgrabe, Gärtelbach) in which field work was done and a wood budget computed. In contrast we highlighted some subreaches (with large values of widening or LW volumes) using just numbers because we do not refer to them anywhere else in the text.

Line 382. In the supplement material, change the caption Figure S78 to Figure S8

Authors' response: Thank you, this was corrected.

Line 390. The threshold of 1.2 was chosen according to a specific criteria based on literature or was it decided for this specific study site?

Authors' response: This threshold was selected assuming that 20% is a reasonable threshold to distinguish significant widening. We clarify this in the revised text.

Line 471. In this paragraph you should refer to the Table S1 and S2. (In the Table S2 S4, S5 caption you could specify what is the last column? Is it p-value?)

Authors' response: Thank you for the comment. Yes, we refer to p-value, we corrected this in all table captions in the supplementary information. Line 471 is the Figure 7´s caption, not sure we should refer to Tables S1 and S2 here (we think there might be a misunderstanding). We did not add the suggested reference to these tables here (line 471), but the tables are cited in other parts of the text.

Line 424. Mean, max or both?) for the logistic analysis you only showed the maximum, right? I have doubts about this value, as I have understood it, you took the maximum accumulated precipitation in a pixel 1x1 km of the catchment, right? Could you discuss (maybe not in this specific location) about the relation of this value to the catchment size and the representativity of it with respect to the average

accumulated precipitation in the catchment? And in case of nested catchments (like the Emme, that includes the catchment of the tributaries, you choose the maximum value of the entire catchment?).

Authors' response: Yes, we refer to maximum precipitation. We clarified it in the text. We appreciate this comment, because it points out that the estimation of this variable was not very well explained. We describe it now with more detail in the methods section and we raise the topic in the discussion. We did not use nested catchments for the analysis of precipitation. This means that the Emme subreaches were considered without including the tributaries upstream as explained in the methods. The subreaches belonging to the Emme main stem are 3, 4, 7, 11, 28, 30, 33, 36, 37, 40, 41, 44, 46, 48, 49, 52, 54, 55, 58, 59, 61, 62, 63 (see Table 1of the manuscript). For each of them we delineate the drainage area and for this drainage area we computed the precipitation.

Line 438. The Gärtelbach does not seem to be among the tributaries with bigger LW volumes recruited according to Fig. 3, it seems that the Chaltbach and the Leimbach have recruited more wood. Could you check or explain?

Authors' response: Thank you very much for this comment, because it pointed out a labelling error in Figure 3. As written in the text, the streams where largest quantities of LW were recruited were the Gärtelbach, Sädelgrabe and Schöniseibach, together with the Emme itself. We corrected the figure (and added the Schöniseibach in the text).

Line 441. Specify if the stream hectare was before or after the flood.

Authors' response: We added this.

Lines 459-460. May this be because you are correlating two variables that include the channel width within it (LW volume /channel width x length) and (Channel width post/channel width pre)?

Authors' response: The reviewer is right, this may be a reason.

Line 476 change wi thsub-reaches ! with sub-reaches

Authors' response: Corrected.

Line 536. I think that pointing out the uncertainties are up to 50% is not needed in the table Title. In this table however the budget should be 0 for an extreme event according to the available equations. How did you calculated the exported?

Authors' response: We removed "of up to 50%" from the caption of Table 4 and Table 5. There are some uncertainties and inaccuracies in the calculation, so it is very challenging to compute a perfect budget with 0 as a result. We corrected some issues in Table 5 and clarified all in the text and Table caption and footnote.

Line 549. Which segment? Could you point it out in figure 1 or name the subreaches that were included in this segment?

Authors' response: We specified this in the caption of Table 5.

Table 5. Same as comment for the table 4. And also the wood budget should be 0, so you could could assume that 501 m3 (+- uncertainties) must have come from upstream reach of the analysed segment. Does the deposit along the river does not have uncertainty? (line 563-565)

Authors' response: We are aware about the mismatch between recruited, deposited and exported LW in Table 5 and we explained this issue in the text.

DISCUSSION This section summarizes the results and compared with the available literature when the authors wrote the manuscript. My main comment on this section would be to discuss a little more on hydraulic parameters to explain the widening and LW recruitment, on the difficulty to obtain that values, or why the data about discharge you could retrieve and calculate is not considered reliable.

Authors' response: We extended the discussion now, adding a reference to a recently published study in Germany that was not available at the time the initial manuscript was submitted.

Line 588. You did not analyse the entire catchment for the LW dynamics and budget.

Authors' response: We analysed the entire catchment for LW recruitment and channel widening, but not for LW deposition, or computing the full LW budget. We clarify this in the revised text.

Line 590-591. These thresholds are not so clear according to your data and analysis

Authors' response: We observed some significant correlations, thresholds and variables, and they are discussed and summarized in the text.

Lines 600-603. You could point out that in these studies hydraulic and morphological controls were analysed.

Authors' response: We are not sure what the reviewer means here, and hence we did not modify this sentence.

Line 608-610. This sentence can be a little contradictory with your previous statement of the need to analyse the entire basin.

Authors' response: We believe this is not contradictory, it highlights the differences in significant variables when studies are only including subreaches that experienced widening. Similar reaches (in terms of morphology) could respond differently (some experiencing significant widening and some not), and therefore, identified control variables may vary when analysing all reaches together.

Line 674. What about the work of Steeb et al, 2017?

Authors' response: We expanded the discussion here, including this study and a very recent published study in Germany by Lucía et al.

Line 699-701. I think this sentence should be explained in the methods and removed from here.

Authors' response: Thank you for the suggestion, we moved the sentence to the methods section.

Lines 706-710. Maybe this is also because, if I understood correctly, you only quantified some deposits and not all the LW deposited along the affected channels, as the cited papers, where the LW may have been transported less distance.

Authors' response: The deposits of LW were analysed in a selected reach along the Emme and in the Sädelgrabe and Gärtelbach. These are the areas where most of the wood was deposited, together with the large accumulation within the Räbloch gorge. Differences in travel distance may exist between the different studies mentioned in the text, but as no data is available about that we did not add anything in the discussion.

Lines 713-724. I think this paragraph could be moved right before the previous, to connect it with the LW budget.

Authors' response: We moved the previous paragraph up instead; so all the text about LW budgets is now together.

CONCLUSIONS The conclusions section is well written and supported by the presented data in this study.

Authors' response: Thank you.