General comments:

The idea presented in this paper is interesting and potentially very useful. The paper is well structured and well written. The authors present a novel procedure aimed at computing a series of flood extent maps on a dense stream network and directly evaluating the possible associated impacts. The proposed approach consists of an integrated forecasting chain that combines a one-dimensional simplified hydraulic model and a distributed rainfall-runoff model for the simulation of discharges over the stream networks, and it has been tested on insurance claim data. Potential limitations and critical issues in the implementation of the proposed methodology are also well documented and discussed throughout the paper. In my opinion, this paper can be considered for publication in the present form, after some minor points will be taken into account. Two main issues and some minor comments are listed below.

At first we would like to thank the referee n°2 for this positive appraisal of our manuscript, and for the detailed comments and suggestions provided. Our answers are detailed below.

1. In section 2, the automatic implementation of 1-D hydraulic models is described; however, the description of the third step is too short and should be completed with additional information. Moreover, from figure 1 it seems that four steps (a-b) are required for obtaining the final map of flooded areas, that is not clear from the text. Finally, the choice of a fixed roughness coefficient is mentioned: a suitable reference should be inserted here. How is this value computed? And what are the possible consequences of keeping it fixed?

This remark is close to another one formulated by referee n°1. Actually figure 1 illustrates both the three computation steps mentioned in section 2.1 (p.3 l.20-23), and the additional post-treatments described in section 2.2 (p.4 l.21-24 and l.32-33). We propose to group the description of all these treatments in section 2.1 and to reformulate the text for an explicit reference to the appropriate figures. In the same time the description of the third step (p3 l.23) will be detailed as follows:" the estimated water levels are interpolated between successive cross sections and compared to the DTM elevations to compute the flood extent and water depth maps (figure 1.c) ".

The roughness coefficient was fixed according Lumbroso et al. (2012) who showed the necessity to limit the roughness values to keep reasonable values of flow velocities. This reference will be added.

The consequence may be a systematic overestimation of the water levels. But according to the results obtained in section 4, it seems it is not the case here.

2. If I understood well, the application of the method exclusively focuses on the flood peak as the variable of interest. However, it should be stressed that other variables (like, e.g., the flood volume and the flood duration) may play a significant role to the study of extreme flood events, and that the dependence among such variables can seriously influences the estimates of flood magnitudes (see, e.g., Salvadori, G., De Michele, C., and Durante, F.: On the return period and design in a multivariate framework, Hydrol. Earth Syst. Sci., 15, 3293-3305, 2011).

Indeed the flood duration and volume may largely influence the flood extents, particularly in the case of large rivers with wide floodplains. These variables could be accounted for by using synthetic hydrographs and unsteady hydraulic computations. However this procedure would largely increase the computation times, and would require comparing the reference synthetic hydrographs to the actually simulated ones for the application of the whole simulation chain. In the particular context of flash floods (very fast evolution and limited lead times), we considered here it was preferable to simplify the procedure, through the assumption that the flood extent is mainly influenced by the peak discharge. This enables to conduct the hydraulic computations in steady-state regime. This assumption is clearly presented as ones of the limits of the procedure (see section 2.2, p.5 l.10). It nevertheless remains reasonable in our opinion since we are here working on small upstream watersheds, in relatively hilly areas. A comment will be added in section 2.2 about the reasons for this choice.

Minor comments

- Page 5 - lines 16-17: You mentioned 10 flooded areas, but I could not find them in figure 2.a. Please also check the return periods reported on the x-axis in figure 2.b which differ from the one mentioned at the beginning of section 2.2.

Yes indeed the text includes here an error and has to be corrected. The 10 return periods used are actually the following: 2,5,10,20,30,50,100,300,500,1000. Figure 2.a presents only one part of the computation results. An updated version is proposed (see joint file) including all computed flood extents.

- Page 6 - lines 23-24: Here, it may be appropriate to clarify that "the continuous discharge-impacts relations" are the continuous curves obtained by linear interpolation that express the relations between discharge return periods and number of impacted insurance policies.

Yes the relations are obtained by linear interpolation. We propose here to add a reference to section 2.3 and figure 2 which provide details on this important methodological aspect.

- Page 8 - line 8: "September" and "June".

Ok this will be corrected (page 11 line 8).

- Page 8 - line 15: I suggest to specify here that the rating curves are graphs of discharge versus stage for a given point on a stream, and maybe add a comment on how such curves are extrapolated in your work.

The extrapolation may be based either on a local hydraulic modelling (Mialet and Alès) or on an expert-analysis of the hydraulic control of the streamgauge section (Banne). This precision will be added in section 4.1.

- Page 10 - lines 32-33: The sentence "It was worse testing if it could provide a number of private houses affected by the floods for each river reach to be compared to the outputs of the proposed forecasting chain" is not clear. Please, reformulate.

We propose a simplification of this sentence: “We tested herein if it could provide a number of private houses affected by the floods to be compared to …”

- Page 12 - lines 7: replace "figure 4d" by "figure 4.d". Please check the crossreferences throughout the paper.

OK, to be corrected.

- Page 12 - line 33: A synthetic incoherent surface ratio is here introduced. I suggest to add a comment on such quantity and/or an appropriate reference.

We propose to add the following comment: “A synthetic incoherent surface ratio (ISR) is computed (eq. 1). It represents the extent of excess and default surfaces, expressed as a proportion of the reference surface”.

- Page 13 - lines 22-23-24: Remove the space before the semicolon.

OK.

- Please check the punctuation of the figures’ captions.

OK.