Dear reviewer dear Editor,

In the following we report the answers to the comments and the clarifications about some choices done in the work. Various comments request to make modifications on manuscript (figures, tables, text modifications…), in the answers we discuss how we propose to modify the text and we give some anticipations about them. We think the reviewer suggestions are in various cases useful to improve the work, anyway despite the reviewer seems to think that the analysis is interesting in the complex, he issued a **rejection** final evaluation; as a consequence we do not know if we have the chance to submit a new version. We ask to editor and reviewer if they think that we can go on with the review process.

In my opinion, the study on the discharge maxima can be potentially interesting to assess limitations and potentialities of the proposed approach. However, I have serious concerns on the paper quality and novelty and, at this stage, I recommend its rejection. In the following, I motivate the reasons of my choice.

1) While I am not a native English speaker, I need to point out that the paper is poorly structured and the quality of the English is low. This is a very serious concern that has to be solved even before discussing the technical details. Many sentences are quite long and with poor grammar. In each section, the length of the paragraphs varies too much (from a few lines to an entire page). I suggest the authors to have a native English speaker proofread the paper.

We can review the general quality of language and writing, moreover since we are not native English speakers we often ask to native English speaker to improve the language but this is always occurred during an advanced phase of the review.

Since all the authors has a certain experience in publishing papers we were surprised to realize that this work is such poorly readable, anyway we will follow the reviewer suggestions to improve the text.

2) The paper structure needs to be improved. The methodology presents results of the hydrologic model calibration; there is a section on the datasets, but new datasets seem to appear in the Results (or maybe they are the same, but named in a different way). There are too many figures that can be merged (see below).

Agreed, this can be done since this is a request similar to those made by reviewer 1. Our intention is to improve the description and naming of data and give more detail about hydrological model. Some parts of the paper can be restructured by moving some sections in different parts of the manuscript. Some figures can be merged.

3) The authors need to describe better what is the novelty of their analyses. The idea of applying a cascade-based approach to issue hydrologic predictions has been used in other studies to evaluate climate change impacts (in this case, climate model outputs are used instead of reanalysis) and improve hydrometeorological forecasts (in this case, outputs of Numerical Weather Prediction models are used instead of reanalysis). The Introduction of the paper does not discuss previous applications that follow the same strategy and does not highlight what are the main contributions of this study (in my opinion, it is the analysis of the simulated distribution of flood extremes). In addition, the authors should clearly state which new analyses and simulations have been performed in the paper –I guess these are bias correction, statistical downscaling and hydrologic modeling– or conduced in other studies –the dynamical downscaling of ERAINT.

It is true that the similar modeling cascade were already applied, but to the best of our knowledge this is one of the first works of this kind done in the considered study area in which the environment is constituted by small basins with flash flood hydrological regime. Moreover the usage of such high resolution reanalysis is quite new. We can highlight this point in the text as suggested, even if the introduction already mentions this fact: "….On one side the distributed nature of the state and output variables allowed to investigate the possibility of using this kind of modelling chain for extreme streamflow statistical analysis (e.g. distribution of annual discharge maxima) and long term water balance (e.g. long term runoff coefficient) in a fully distributed perspective. On the other side the high spatial grid spacing and time resolution of forcings,

together with the use of a rainfall downscaling model, allowed to explore the use of such high resolution reanalysis in regions characterized by the presence of small hydrological watersheds in areas characterized by very complex topography..."
We can also improve the discussion of previous works that use similar strategies, but this is already present. In lines 3 to 12 of page 3 we cited various works that involve usage of reanalysis for hydrological purposes.

4) The authors need to present more details on the hydrologic model, including: how the model has been parameterized, calibrated, and validated with observed data; which soil and vegetation have been used (maybe show a map as well?); which observed hydrometeorological forcings have been adopted and how they have been interpolated in space; and show examples of simulated and observed hydrographs in the calibration and validation periods.

Agreed, this is also a request of the reviewer 1. Moreover we have a recent published paper to be cited where we used a similar setting of the model on the same study area for different purpose (Davolio et al., 2017). We did not devote too much space regarding the calibration of the model because it was partially faced in other works and it is not the core of the paper but it is a "functional" activity for the other analysis. Some information are already present in the text, for example regarding interpolation : "....ground stations measurements were interpolated on a regular grid of 1 km resolution by using Kriging method and used as input to the model...."
*To summarize the performance of the model we inserted a table with the skill scores value. If the editor and the reviewer consider it necessary some graphs of model versus observations we can do that, but, in our opinion, this appears a little bit out of the scope of the paper and in contrast with the need to reduce figures number.*

5) A discussion on the performances of the statistical downscaling algorithm is missing.
The testing and application of RainFARM in the study area for forecast purposes was done in many works, some already cited in the text (Rebora et al. 2006; Silvestro et al. 2012). In this work it is used, as mentioned in the text, to generate a possible downscaled rainfall scenario from 4km-3hours to 1km-1hour resolution. Effectively we did not test the benefit of using or not the downscaling.
*To assess the impact of downscaling we made the hydrological simulation with BC rainfall but **without** applying the downscaling, we estimated the ratio of Qmean (mean of 30 years ADM) without and with Downscaling for each pixel.*
Results are plot versus drainage area (see figure below). It is quite evident the impact of downscaling, the impact generally increases when drainage area decreases. The downscaling seems to be important even in this application especially to deal with small basins.

Qmean
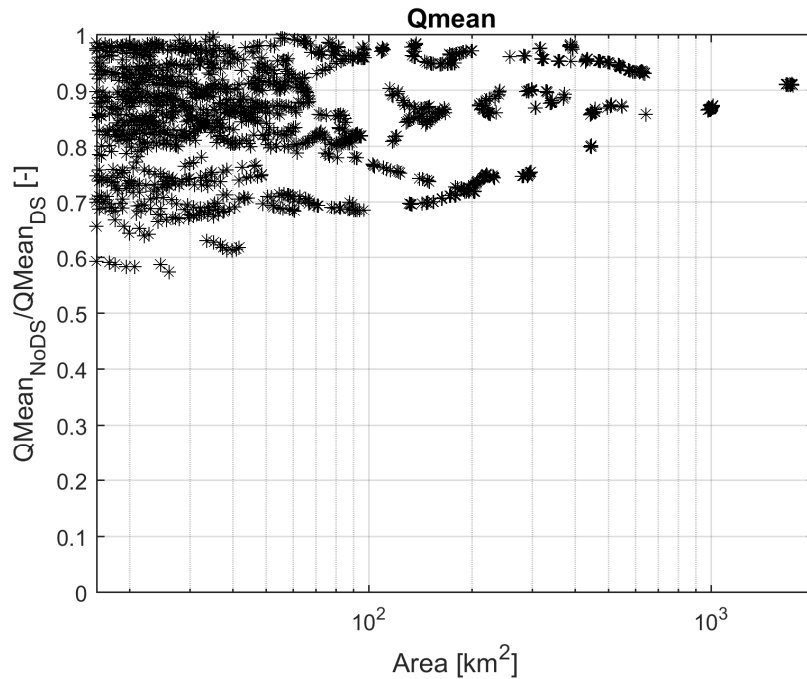
6) The relatively long discussion of the comparison of the WRF simulations with an alternative rain gauge network to the one used by Pieri et al. (2015) should be shortened. By the way, are the two rain gauge networks completely different?
We can better clarify this point in the text. The raingauge data set used in Pieri et al. (2015) is a subset of the one used in our work.

Figures.
We can reduce and improve the figures following the reviewer suggestions.

References

Cenci L., Laiolo P., Gabellani S., Campo L., Silvestro F., Delogu F., Boni G., and Rudari R., "Assimilation of H-SAF Soil Moisture Products for Flash Flood Early Warning Systems. Case Study: Mediterranean Catchments", IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, vol. 9, no. 12, pp. 5634-5646, Dec. 2016. DOI: 10.1109/JSTARS.2016.2598475

Davolio, S., F. Silvestro, and T. Gastaldo,: Impact of rainfall assimilation on high-resolution hydro-meteorological forecasts over Liguria (Italy). J. Hydrometeor., 18, 2659-2680, 2017. https://doi.org/10.1175/JHM-D-17-0073.1

Giannoni, F., Roth., G., and Rudari, R.: A Semi – Distributed Rainfall – Runoff Model Based on a Geomorphologic Approach. Physics and Chemistry of the Earth, 25/7-8, 665-671, 2000.

Giannoni, F., Roth, G. Rudari, R.: A procedure for drainage network identification from geomorphology and its application to the prediction of the hydrologic response, Advances in Water Resources, 28(6), 567-581, 2005, doi:10.1016/j.advwatres.2004.11.013.

Rebora, N., Molini, L., Casella, E., Comellas, A., Fiori, E., Pignone, F., Siccardi, F., Silvestro, F., Tanelli, S., Parodi, A.: Extreme Rainfall in the Mediterranean: What Can We Learn from Observations? J. Hydrometeorol., 14, 906-922, 2013.

Silvestro, F., Rebora, N., Giannoni, F., Cavallo, A., Ferraris, L., (2016) The flash flood of the Bisagno Creek on 9th October 2014: an "unfortunate" combination of spatial and temporal scales. Journal of Hydrology, 541, Part A, Pages 50–62, doi:10.1016/j.jhydrol.2015.08.004