Manuscript: Analysis of the streamflow extremes and long term water balance in Liguria Region of Italy using a cloud permitting grid spacing reanalysis dataset, by Silvestro et al.

General Comments:

This study has the potential to be a good contribution to the literature; however, it needs to be greatly improved to be accepted. A significant part of the manuscripts is based on the data and analysis provided by Pieri et al., and it is unclear weather some of the analysis showed was performed by Silvestro et al. or not. The structure of the manuscript should be improved; part of the methodology should be moved to introduction and part of the result to methodology (details below). A flowchart describing the methodology would greatly benefit the methodology section. The role of the hydrological model is not sufficiently address, and I think this is a key component that should not be left behind; no matter how good or bad the rainfall input is, if the hydrological model can not properly represent the hydrology, little analysis can be done. For this purpose, more details about model inputs, parameters, processes, uncertainties and performance need to be included. There are too many figure, most can be either removed or combined with others (see below). There are question regarding the usefulness of the regional peakflow analysis, if the purpose of this to be used for prediction in ungauged basins it should be addressed in the manuscript. The English writing style should be revised and improved.

Specific Comments:

Introduction

- Weak, include more about issues with peakflow estimation and uncertainties, previous studies in the region, hydrological modelling, downscaling and bias correction. Much of the text in other sections should be moved to the introduction (detailed below)
- Page3, line 13-16: I would move it to the methodology section.

Materials and Method

Study Site and Case Study

Need a better description of the study basins: how many are they and where? Include geomorphological characterization (area, slope, etc), vegetation cover and soil description; are these rivers regulated by dams or do they have extraction, hydrological regime? How is the climatology of this region (precipitation: snow and rain, and temperature?), you should give more details about this. If there are other studies in these basins they should be included and mentioned.

- Page 4, line 11-12: “The response time …” add Reference.
- Figure1: Add to the figure a North Arrow, scale bar, color bar for topography and its source. I would zoom in to the area of interest and add an inset plot showing the regional location of the study site within Europe.
- How many stations from the OMIRL network do you use in the study? What about their quality control, any issues that should be mentioned in this regard? Any snowfall measurements?

- Page 4, line 22: Mention about “historical validated data”, what do you mean by that, validated against what exactly? Is there a reference showing the validation?
- Need a better description of the driving data used in the study. Number of stations, a better map showing their location with respect to the basins, available period, gaps in the data if any and variables being recorded (more details).
- Similar for the annual maxima time series, need to include an official identifier number (if available) or name of the hydrometric stations. How reliable are these records? This is particularly important when you are assessing peakflow.

EXPRESS-Hydro reanalysis

- Page 5, line 6-20: Rewrite, move to introduction and add references, this is not part of the methodology. Here you should only describe the EXPRESS-Hydro reanalysis.
- Avoid the use of superlative like “very high”, instead state the resolution.
- Remove Figure 2, not relevant.
- Add reference for ERA-Interim and WRF.
- Page 6, line 1-17: This is more of a discussion about Pieri et al, I would remove it or synthesize as part of the introduction section. This section should be about the reanalysis’ technical features, pros and cons and its suitability for the area of study. What variables are you using from EXPRESS-Hydro? Temporal and spatial resolution? Available period? Etc.

Bias correction of rainfall fields (B.C.)

- Replace “Pieri et al (2015) reanalysis” by “EXPRESS-Hydro reanalysis” (everywhere in the text)
- Page 6, line 22-23: no need to include the link for the dataset again.
- Why don’t you use the observed dataset as opposed to the corrected EXPRESS-Hydro reanalysis?
- You should not give additional information about EXPRESS-Hydro or observed dataset here; those should be listed in their respective sections.
- Page 8, line 17-18: that’s part of the results section
- Did you perform any correction to other weather variables used in the hydrological model, such as air temperature?
- Is there any snowfall in the study basins, was this bias-correction also applied to snowfall? Please clarify
- Is there a particular reason you performed the CDF correction at a monthly scale, how different are the precipitation patterns between months? Maybe with an annual or seasonal correction is enough?
- You need to better argue the use of rainfall station interpolation to bias-correct the reanalysis, spatial interpolation can introduce significant errors in you rainfall estimations, particularly if your monitoring network in sparse, the rainfall regime is very heterogonous or has significant local topographical controls. If any of these apply, then I’m not sure how robust the spatial interpolation is, which can lead to significant problems in the hydrological model performance. You said that regression with other variable, such as elevation, were tested did not show

significant correlation, but is your monitoring network dense enough to argue that? In other words, how is the elevation distribution of your monitoring network? If most of them are at lower elevations then no correlation should be expected.

Downscaling the precipitation with RainFARM Model

- Page 9, line 19-22: Could you elaborate more about "runoff formation at small scales". I wonder if there is a study about the runoff mechanisms in this region or if there is something you could include about this. This seems to be rather critical to support your methodology, as this is apparently, the only reason behind applying spatio-temporal downscaling.
- Page 10, line 1-2: Reference

The hydrological model: Continuum

- Page 10, line 11-12: Reference
- What's the soil surface temperature used for in the model?
- How is evapotranspiration being simulated by the model? Does the model require air temperature?
- Does the model simulate snowmelt and accumulation?
- Include the units of the parameters.
- This section needs clarification regarding the parameterization approach. There are 6 parameters requiring calibration, did you use the same values for all your basins and landcover? How was the calibration performed, automatically or manually? What was the period used for calibration? Did you calibrate using hourly streamflow measurements? What are the final parameter values for the calibrated and uncalibrated parameters?
- Can you describe the meaning of the REHF index
- Page 12, line 18-19: basins without data for calibration/validation should be removed from the analysis. The empirical nature of the model does not support any parameter transferability; therefore, assuming average values from calibration at other basins should not be used.
- Page 13, line 1-3: clarify, did you run the model for the entire period and used the final conditions from year 2008 as initial conditions for the year 1979?
- What meteorological variables does the model use?

Results

Precipitation analysis

- Page 13, line 13-20: Apparently, these are not your results should not be in the result section
- Figure 3 to 7: are these your results or from Pieri et al?. If there are not yours you should remove and just reference them. Otherwise you can combine them and only show the difference map for annual and seasonal scales. Note that the mean daily errors are between -3 and 3 mm/d, which for annual rainfall mean an average error of +-1095 mm/yr, which is quite significant and will have a significant impact in your simulations, this should be reconsidered.
- Is Figure 3 using the Bias-corrected EXPRESS-Hydro reanalysis? unclear

- Page 14, line 1-11: These are not your results should not be in the result section
- Unsure whereas what you are showing is from your analysis or Pieri et al. Please clarify.
- Include the corrections applied with the B.C. approach. How large these corrections where?
- Page 14, line 12-17: do this analysis at a basin-scale as that's the relevant scale of the study. I'm not convinced about that EXPRESS-Hydro "reproduces quite well" the observed precipitation. Need further analysis. How are the extreme precipitation events represented by the reanalysis? This is critical to properly represent peakflows. What about temperature?
- Can you evaluate the performance of the spatio-temporal downscaling?, this seems rather critical when assessing peakflow that may be generated from intense hourly rainfall-runoff events.

Distribution of the annual discharge maxima

- I think that Fig. 9 to 11 show that model representation of peakflow is somewhat weak, and often the simulations without B.C. show better results (see fig 9 Bisagno La Presa), which is what the Kolmogorov-Smirnov test show as well (only 60% pass the test). This problem can be due to the problems in representing annual rainfall (fig 3-7) and hydrological model!.
- Figure 12 should be replaced by a table.
- Page 17, line 1-23: This is methodology not results, move to the methodology section.
- I think you need to show that the model works well representing ADM at a basin-scale to perform the regional analysis. So far, the simulations at only 9 basins pass the statistical test (out of 15 from fig 12), but then the regional analysis is performed using all of them? Are those basins without streamflow records also included? (they should not). The relatively good agreement from the regional frequency distribution function analysis (fig 13) could be due to a compensatory effect.

Regional Analysis of the annual discharge maxima

- Page 17, line 1-15: This should be in the methodology section.
- I suggest expanding about the usefulness of the regional ADM curve, I could see the potential when dealing with prediction in ungauged basins, otherwise I'm not sure what's the purpose of computing this regional curve. The relatively good agreement of this curve against observations showed in figure 13 could be due to some compensatory effect between basins; need to expand on this as well.
- The authors argue that for small scale basins (<50 km2) the simulated ADM curve (Q(T) model) underestimates the regional curve, and attribute this to problems in the reanalysis precipitation quality. I think this analysis is wrong and the fact that the Q(t)model underestimates Q(T)reg (i.e. Ratio(T) <1) only suggests that the regional analysis is not representative of small basins, which could be due to several factors not address in the discussion. (1) If the number of points used to develop the regional curve comes in majority from larger basins, then I wouldn't expect the regional model to represent small scales basins; it is unclear how many large or small scale basins (or grid points) where use to construct the regional curve, this could help the discussion. (2) The role that the hydrological model is playing in representing peakflows is not sufficiently

explored. From the Kolmgorov-Smirnov test, only 9 out of 15 basins past the test (60%), which is not sufficient in my opinion. Problems with model parameterization and process representation can probably explain most of the ADM mismatch for the scenario with bias-corrected rainfall. The representation of peak rainfall events by the reanalysis is unclear, and those are the events that produce peakflows, assuming an entirely rainfall-driven streamflow, which is also unclear from the manuscript. (3) If the EXPRESS-Hydro reanalysis cannot represent the "small-scale" rainfall events, then what was the purpose behind the spatio-temporal downscaling? I think these issues should be better explored and described in the text, particularly regarding uncertainties with hydrological model.

Water Balance and Runoff coefficient

- I am not sure about the point of calculating what the authors refer to the "runoff coefficient". I would suggest looking at basin or sub-basin scale runoff ratio (runoff volume/precipitation) as a proxy to the water balance, that way you can avoid storage problems. I don't understand why if all the relevant mass fluxes are being simulated by the model, the authors don't calculate the mass balance directly. I would re-focus this section to a basin-scale mass balance analysis.
- Table 2 shows model streamflow bias, this should be part of a calibration-validation section. No need to show runoff coefficient for the scenario without B.C. as this will clearly be worst than the scenario with B.C.