

“Regional flow duration curves for ungauged sites in Sicily”

by F. Viola et al.

Reply to Anonymous Referee #2

Dear Referee #2,

we are very grateful for your prompt and constructive comments on our manuscript. Following the suggestions of the Editor, we hope to have provided a response to every comment and observation of all the Referees.

Below you can find the replies to your comments. In particular, we used the grey italic format to report all the questions raised by the reviewer and the standard format to report our comments and the actions taken.

General comments:

The importance of Flow Duration Curves is highlighted along the paper and new strategies to estimate them is worthy, especially if they are dealing with some complex phenomena as the ephemeral rivers. So, I found the paper quite interesting and publishable.

But I would like to get some clarifications and I am kindly asking to the authors to perform some few changes in the manuscript in order to produce a better product.

Replies to the specific comments:

1) Figures are not clear, the legend is missing or confuse in Figure 1. Figure 2 is not interesting and Figure 4 should be shown separately.

We partially agree with the reviewer.

We will modify slightly the figure 1 adding a legend (but not for the colours of the different basins) and adding the meaning of thick black line in the figure caption.

With regard to the figure 2, we consider it important since it shows the overall variability of the daily flows in the considered basins. With regard to the figure 4, we'll split it into two different figures (improving them) in the reviewed paper.

2) Authors mentioned that the maximum record length used was 43 years with a mean value of 20 and the minimum of 10 years, I would like to see these values on table 1, but mostly I would like to get a justification of the 10 years of data, I mean, statistically speaking it is not enough to use just 10 years... Why do the authors think it will be useful? (Some references would be appreciated).

This approach has been commonly used in most of the referenced papers (i.e. Croker et al., 2003 used a minimum of **9** years, Castellarin et al., 2004 used a minimum of **5** years, Ganora et al. 2009 used a minimum of **7** years, Fennessey and Vogel, 1990 used a minimum of **12** years). This is due to the fact that this approach can not be assimilated to the traditional approach of flood frequency analysis in which a greater sample size is sometimes required.

3) In page 7064, line 11, Thornthwaite (1948) is cited but it is not referenced.

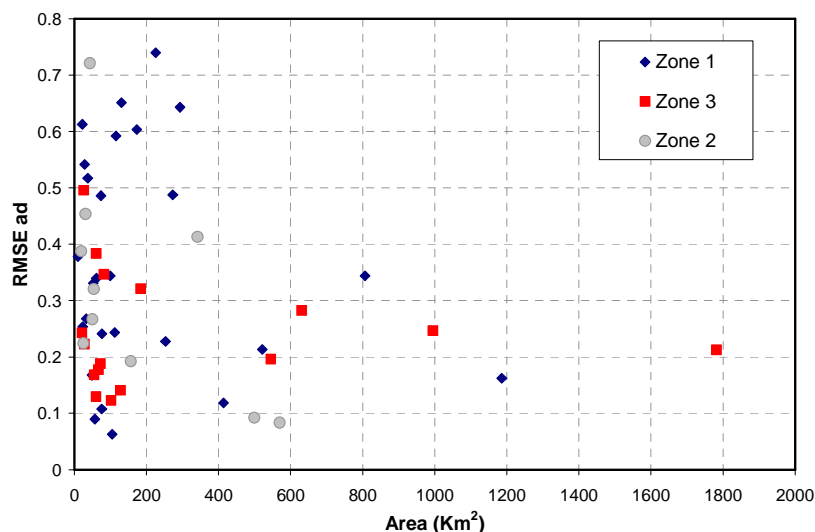
That's true. We forgot to insert this reference. We will reference it in the final version of the paper.

4) In page 7064, line 12, "Table 1 shows relevant morpho-climatic catchment characteristics...", I consider that slope, drainage density, stream length, etc... are relevant characteristics which were not involved in this study. I think the selection of the characteristics deserve a better explanation, specially using statistical regression analysis, when do you have to try to explain the process looking for the most relevant variables. Is the Curve Number representative? Because the range of CN usually is low, Is Aridity Index related to rainfall? It must be... so I would like to get some explanations about the selection of the variables.

Actually we used other morpho-climatic catchment characteristics in addition to those listed in table 1. These characteristics come from the SIRI (as mentioned at p.7064, line 6) which is a geodatabase containing many morpho-climatic catchment characteristics and climatic data relative to the Sicily. Initially we tried to relate each of the three parameters of proposed FDC (a , b and D_w) to about 20 main morpho-climatic catchment characteristics of the examined basins (and among these, we used basin slope, main stream length, mean areal annual temperature, shape coefficient and others) and in a second time we decide to limit the number of characteristics to show in the table 1 on the basis of results of stepwise analysis. We will better explain this selection in the reviewed paper.

5) *I could be interesting to see if there is a relationship among performance index and area as it is expressed in page 7066, lines 18 and 19. In order to explain the high performance index in small basins and the reduction of the performance index at large basins. Are these effects related to the different sub-zones?*

Unfortunately there is no a clear trend and the adimensional RMSE seems to be not dependent by the considered zone (see figure below). We made the plot suggested by the reviewer but we do not consider it worth to be inserted in the paper.



Relationship between basin area and adimensional RMSE for each considered zone

6) *Finally, using just 3 basins during the validation process is not enough, it is required to perform validation at different basin sizes, and please justify the selection of the 3 basin to be calibrated, it was a random selection?*

The choice of the three basins used for the validation of the procedure was more and less randomly. We mean that we choose randomly three basins, one for each zone, which were representative of the basins size distribution [we choose one basin with the area lower than 60 km² (15 basins), one with area ranging from 60 to 160 km² (14 basins) and the third one with area greater than 160 km² (14 basins)]. The choice of two basins for each zone would have caused the use of only 8 basins in zone 2 for the calibration of regional method.