Reply to Reviewer 1 comments

Manuscript number: hess-2013-221
Title of the manuscript: Distributed hydrologic modeling of a sparsely-monitored basin in Sardinia, Italy, through hydrometeorological downscaling.
Authors: G. Mascaro, M. Piras, R. Deidda and E. V. Vivoni.

Reply to Reviewer 1

First of all, we thank Reviewer 1 for her/his general and specific comments on our manuscript. In the following, the comments raised by Reviewer 1 are split into parts and copied in bold fonts to facilitate understanding of our answers.

After describing the paper summary and its main contribution, Reviewer 1 provides the following specific comments.

(a) On page 8, line 17 the authors quote a mean annual potential evapotranspiration (ET_0) of 750 mm for the basin, located in Sardinia. This figure is very low for Mediterranean climate, as is shown by the daily values of ET_0 shown in Fig 7b. These values range from 2 to 6 mm/day while the average daily value that corresponds to the figure of 750 mm/yr is only 2 mm/day.

The values reported in Fig. 7b are referred to the spring season (March, April and May). Thus, their average is not representative for the entire year. In addition, the estimates of daily ET_0 in the x-axis have been obtained by using the Hargreaves formula, which provides higher values (included between 2 and 6 mm/day in Fig. 7b) as compared to the Penman-Monteith formula (values included between 1.5 and 3.5 mm/day in Fig. 7b). The mean annual value of 750 mm for ET_0 has been obtained by: (i) applying the Penman-Monteith formula with the data observed by the meteorological station described in Table 3 of the paper, and (ii) comparing the values obtained with Penman-Monteith against the climatological estimates derived with the Thornthwaite method for the entire island, and published in a technical report of the University of Sassari (Italy): Pulina, M. A. (1986) *L'Evapotraspirazione potenziale in Sardegna in funzione dello studio del regime idrico dei suoli.* Studi sassaresi: organo ufficiale della Società sassarese di Scienze mediche e naturali. Sez. 3: Annali della Facoltà di Agraria dell'Università di Sassari (Italy), Vol. 32 (1985-1986), p. 96-109. ISSN 0562-2662. We added this reference in the new manuscript version (page 8, line 17).

b) On page 9, lines 12 to 14, the authors discuss the small differences in vegetation cover in the basin from 1954 to 2006. With 48% of agricultural land, I would expect that irrigation development had somehow transformed the vegetation cover (and the hydrologic behaviour) in the region during that period. Irrigation is later invoked to explain the discrepancy between model results and observations during low flows (page 22, line 17). If irrigation is present in the basin, a brief discussion of this issue is advisable.

We thank reviewer 1 for pointing this aspect out. In the Rio Mannu basin, the fraction of agricultural land that is irrigated is about 50%, while the rest of the crops are rainfed. The larger portion of irrigation is provided in July to the artichoke fields. As a result, irrigation mostly affects the low flows during the summer season. Overall, we can assume that the hydrologic behaviour of the basin is minimally influenced. These considerations have been added in the revised manuscript on page 9, lines 15-17.

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c) On page 12, lines 18-19: results obtained from the downscaling procedure at 45 min resolution are resampled at 1 hour resolution. This is strange. If rainfall runoff simulation was going to be performed at 1 hr resolution, why not select this resolution in the downscaling, dividing the 6 hr period in 6 steps instead of 8? On the other hand, if the downscaling procedure could not be adapted to 1 hr resolution, why not use the 45 min time step in hydrological modelling?

The downscaling model is based on multifractal cascades with branching number equal to 2. This implies that, for each disaggregation level, each "father" generates 2 "children", thus disaggregation can be performed only for powers of 2. From the practical point of view, a rainfall value at 6-h resolution is disaggregated first at scale 6/2 = 3 h, then at scale 3/2 = 1.5 h, and, finally, at scale 1.5/2 = 0.75 h = 45 min. As reviewer 1 correctly underlines, the downscaled rainfall values at 45 min were readily resampled at 1 h resolution to be used as input for the tRIBS hydrologic model. This was done because, in its current structure, tRIBS can be forced with spatial rainfall grids at 1-h resolution, which is a common resolution for several internal routines. While we recognize that we could have modified the model code itself to ingest the 45-min rainfall inputs, we believe the benefit would have been negligible, if any, since the model is mass conservative.

d) Page 19, lines 15-19. Values of RMSE and bias for the difference between MAP_O and MAP_D are very large compared to the mean value. Mean Annual Precipitation is 680 mm/yr, which corresponds to around 2 mm/day. The values of RMSE are around twice the average value of the signal, while the bias is around 50% of the signal. This is hardly a "slight" underestimation.

We thank reviewer 1 for this comment. As a matter of fact, the values of RMSE and Bias reported in Table 7 are referred only to "rainy days", as indicated in the table caption, but not in the text. In the revised version, we added this information in the discussion to avoid confusion. Indeed, to properly evaluate these metrics, we should consider the number of rainy days per year, which is about 70-80 in this region. For example, as explained in section 5.2, for the year 1930 the downscaling tool generates an average MAD_D of 848 mm, as compared to an observed MAP_0 of 902 mm, thus resulting in an overall bias of 54 mm, which is about 6% of MAP_0. This total bias can be derived from Table 7 by multiplying the value of -0.64 mm times the number of rainy days, 85, for this year. To better illustrate these concepts, we added a few lines on page 20, lines 5-8 in the new manuscript version.

e) Page 21, lines 16-18. I agree with the positive evaluation of the fact that the model captured individual peaks measured by the Italian Hydrographic Service. However, the utility of the model to analyze flood frequency under climate change in conditioned to the hypothesis of stationarity of the rainfall process, because the downscaling procedure derived from data at the end of the 20th century was applied to the period 1930-32.

We completely agree with Reviewer 1. As most studies on the effect of climate change based on downscaling strategies, the use of these tools calibrated in the present to infer prediction in future

periods is subject to the hypothesis of stationarity. In the new manuscript version, we explicitly explained this concept (page 23, line 23).

TECHNICAL CORRECTION From the formal standpoint, the paper is very well written, correctly organized and adequately illustrated with tables and figures. Figures 9 and 10 could benefit from the use of colours, if possible.

While, in general, we agree that the use of colors help improving the figure interpretation, in Figure 9 and 10 we are only comparing one line (the observed discharge) with a shaded area (the ensemble simulations), and circles (the measured discharge). As a result, we prefer using gray scale colors, also to facilitate the use of black and white printers.

Although I am not a native English speaker, I believe the following expression should be corrected:
On page 23, line 10, This holds promising for a subsequent... (holds promise?).

We corrected the sentence in the new manuscript version.