Reply to Reviewer 1 comments

Manuscript number: hess-2014-11
Title of the manuscript: Quantification ofhydrologic impacts of climate change ina Mediterranean
basin in Sardinia, Italy, throughhigh-resolution simulations.
Authors: M. Piras, G. Mascaro, R. Deidda and E. V. Vivoni.

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Reply to Reviewer 1

First of all, we thank Reviewer 1 for her/his 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.

Reviewer 1 provides first the following general comment.

The paper is well written, evaluating the climate change impacts on water resources in the Mediterranean basin is a challenging tasks as noted by several authors before. The introduction is giving a clear overview of the proposed approach; however recent work in the same area with very similar methods should be acknowledged and discussed: Such as Camici et al., 2013, who considered stochastic rainfall generators for catchments located in central Italy or Tramblay et al. 2013 who evaluated high-resolution climate model outputs for hydrological impacts studies in semi-arid conditions. My main problem with the manuscript of Piras et al. is that it lacks validation of the different steps of the modelling chain. These days, a wealth of papers are published about the hydrological impacts of climate change, however prior to make projections there is a need to ensure that the methods are robust and able to provide projections with some skills and the associated uncertainty. The only uncertainty that is taken into account in the present work is the climate change signal, by using different climate models and different scenarios. What about the uncertainties at the different steps of their quite complex downscaling approach? And the hydrological model?

Given the significant number of studies focused on the hydrological impact of climate change, in our manuscript we cited those that we considered more related to our work, and, specifically, those (of which we were aware) that focused on Mediterranean areas. In this study region, the number of contributions available in the literature is rather low, as revealed by searching in Web of Science, Scopus and Google Scholar. Thus, we thank Reviewer 1 for the suggested references, which are pertinent and have been added in the new manuscript version (page 5, line 8).

Regarding the comments that Reviewer 1 raised about the validation of the different steps of the modeling chain, we would like to point out that the different components were previously calibrated and validated against observed data in the study region in Mascaro et al. (2013). In the following we summarize these calibration/validation efforts:

    a) The precipitation downscaling model Space Time RAINfall (STRAIN; Deidda, 1999, 2000) was calibrated and validated using observed rainfall data collected by 1 min-resolution rain gages of the Sardinian Hydrological Survey (page 8500 lines 20-23 of Mascaro et al., 2013). This effort was built upon the work of Badas et al. (2006), who applied the STRAIN model in the study region with the same dataset mentioned above. These authors (i) showed the model capability to reproduce observed rainfall statistics at high spatial and temporal resolution, and

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      (ii) introduced a modulation function, dependent on orography, to improve the representation of spatial heterogeneity in rainfall probability distributions.

b) The downscaling strategy for potential evapotranspiration was calibrated and validated with data from a meteorological station within the Rio Mannu basin (Mascaro et al., 2013; page 8502 lines 4-5).

c) The hydrologic model was calibrated and validated against observed discharge data with reasonable accuracy (Mascaro et al., 2013; page 8503 lines 12-17).

The validation of the different steps of the modeling chain has been mentioned on page 4, lines 22-23; page 9, lines 6-9; page 10, lines 22-23; page 12 lines 16-18 (pages and lines refer to the revised manuscript).

Regarding the comments related to the uncertainty, we acknowledge that a complete approach would require the use of several combinations of Global and Regional Climate models, multiple downscaling methods, and multiple hydrologic models. Currently, there are very few studies that have adopted this type of approach, mostly because of high computational requirements. In our case, to simulate the complex hydrologic process of a Mediterranean basin, we selected a physically-based hydrologic model to conduct high-resolution simulations. As it is well-known, this type of models requires a significant computational effort when run on multidecadal periods, as those involved in climate change studies. For example, the 256 years of simulations took us 880 hours of CPU time over 64 processors. Thus, with the computational infrastructures that we have currently in hand, we could not consider multiple combinations of downscaling/hydrologic models. In a future study, we are planning to compare outputs of the hydrologic models that have been applied on the RMB by other research groups within the CLIMB project. This will allow addressing somehow the uncertainty of hydrologic simulations. This has been added in the paper conclusions (page 23, line 8-10).

Specific comments:

1) The observed data is not presented in the paper. The reader can find page 8502 line 5 that "The procedure was calibrated in the RMB using meteorological data observed in one station over 1995–2010". Which station? What parameters are recorded? Are there rain gauges in the catchment with available data? Or the authors rely solely on E-OBS? Is there discharge data recorded and for what time period? In addition, what is the relevance of the E-OBS dataset in south Sardinia? Are there enough stations in E-OBS covering Sardinia, or E-OBS data in that area is pure interpolation, as in several parts of southern Europe with low density monitoring networks?

The observed data were described in the paper of Mascaro et al. (2013). Specifically:

The hydrometeorological dataset used to calibrate the hydrologic model included:
- Daily discharge data at the Rio Mannu basin outlet for 11 years from 1925 to 1935. These data were acquired from the technical reports of the Italian Hydrologic Survey. After a quality control based on the analysis of the stage-discharge relations published every year and other

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notes present in the reports, we were able to identify three years that we judged as those with the most accurate data.
- Daily rainfall data from 12 gages within or near the basin in the same period 1925 -1935.
- Daily minimum and maximum temperature from one thermometric station located close to the basin in the same period.

The precipitation downscaling model was calibrated using:
- Precipitation records at 1-min resolution from 204 automatic rain gages observed during the years 1986–1996 in the coarse grid of 104 km x 104 km shown also in Fig. 1b of this manuscript. Note that this domain entirely includes the catchment.

The downscaling algorithm that allows deriving the hourly potential ET was calibrated using:
- Hourly meteorological data from 1 station over the period 1995–2010. The observed meteorological variables include all the variables required to apply the Penman-Monteith formula.

While the detailed characteristics of these datasets were not repeated in the present paper to avoid redundancy, their use has been mentioned in section 3 (page 9, lines 6-9; page 10, lines 22-24; page 12 lines 11-12).

The E-OBS dataset was used by Deidda et al. (2013) in all the CLIMB study sites because it was a common basis for auditing the climate models and for applying a large-scale bias correction. As correctly pointed out by Reviewer 1, since the E-OBS dataset is based on a network of gages with a relatively low density, in our study we applied a local-scale bias correction of rainfall grids to correct the residual bias using observed data from 13 gages within the catchment in the period 1951-2008. This is reported in lines 6-11, page 10 of the new manuscript version where we pointed out that the 13 rain gauges are located within the Rio Mannu basin.

2) In Mascaro et al. (2013a), one could read that the discharge data is available only for the years 1925–1935: the year 1930 was selected for calibration, the years 1931 and 1932 for validation. This is not sufficient to validate the hydrological model in a robust way to conduct a study of the projected climate change impacts: The hydrological model is not evaluated during the reference period 1971-2000. The authors are using a "physically" based model, however there is no guarantee that a validation on two years (1931 and 1932) will ensure the stability of the results over long time period (30 years). In addition, all "physically-based" models behave conceptually at the grid scale, therefore it is still required to validate such models. In Refsgaard et al., 2014 are presented several guidelines to evaluate the hydrological impacts of climate change, in particular the need of long data series to validate the models on contrasted climate periods.

We agree with Reviewer 1 that a longer period of calibration and validation would increase the robustness of the hydrologic model. The Rio Mannu basin was selected as one of the study sites of the CLIMB project (funded by the 7th Framework Program of the European Union) because of the presence

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of agricultural fields and of an experimental farm where several data of crop productivity are being collected. One goal of CLIMB is, in fact, to evaluate the impact of climate change on local economic activities. This basin possesses then ideal characteristics to accomplish the project goals. Unfortunately, in our watershed, observed discharge data were only available in the period 1925–1935. Furthermore, as mentioned in the answer to comment 1, after a quality control, we were able to identify only three years (1930-1932) that we judged as those with the most reliable data. As a matter of fact, the problem of lack of discharge data is common for other basins in Sardinia and for most Mediterranean catchments. Thus, if we want to estimate the impact of climate change on water resources and local economic activities in these study regions, the limitation of observed discharge records needs to be accepted most of the times. As a final consideration, we would like to point out that, even if short, the period 1930-1932 includes a variety of hydrologic conditions (wet and dry periods, and floods), and, thus, we believe it allowed a relatively robust calibration of the hydrologic model.

3) The authors performed a large scale bias correction (section 3.1.2) and then a local bias correction with a stochastic generator of multiplicative multifractal cascades (section 3.1.3). The latter downscaling approach was calibrated in the previous study of Mascaro et al. 2013a over the period 1986-1996 in a coarse (104x104km) spatial domain. Is this calibration valid for the catchment of interest? What about the validation of such an approach, and the stationarity assumption of bias correction over different time periods? The strong assumption behind bias correction is that model bias is stationary in time. Maraun 2012 showed with ENSEMBLES RCM runs that the precipitation bias is stationary for most parts of Europe, but strongly affected by variability in arid and semi-arid regions. Tramblay et al., 2013 also failed to validate a daily quantile-mapping approach of bias correction under semi-arid conditions.

We point out that the local-scale bias correction is a component separated from the stochastic downscaling. In fact, the local-scale bias correction was applied to eliminate the residual bias after the large-scale bias correction. We confirm that the precipitation multifractal downscaling model was calibrated in a coarse spatial domain that entirely includes the Rio Mannu basin. The domain is shown in Fig. 1b.

Regarding the hypothesis of stationarity of bias correction and more in general calibration relations and model parameters, which was assumed to hold in current and future climate, we acknowledge that, in principle, it may not be valid. However, as a matter of fact, it is assumed in the great majority of the climate change studies. Restricting the discussion only to downscaling and bias correction of precipitation, an important reason that justifies this assumption is that the spread between the rainfall climatology produced by climate models and the observed climatology is significantly large (see e.g. Fig. 3 in Deidda et al., 2013). Thus, it is very likely that the application of non-stationarity bias correction   could   not   really   markedly   affect   the   simulated   data.

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Reply to Reviewer 2

First of all, we thank Reviewer 2 for the comments on our work. In the following, the comments raised by Reviewer 2 are split into parts and copied in bold fonts to facilitate understanding of our answers.

Reviewer 2 provides first a general comment.

The manuscript presents an investigation of climate change impacts for a Mediterranean basin, Rio Mannu, located in Sardinia. The study is based upon a set (four) of GCM-RCM combinations that in turn are used to drive a physically-based hydrological model, tRIBS, for past and future conditions under the A1B emissions scenario. Climate data are spatially and temporally downscaled and bias-corrected using statistical techniques whose skills have been exhaustively demonstrated in previous literature studies.
Overall, the study is well designed and the methodology is scientifically sound. The illustrations are all very high quality, and well organized. The issues discussed in this paper should be of interest to the scientific community, and is suitable for HESS. I recommend this manuscript being accepted with some minor/moderate revisions. Most of the issues that I have just need a bit clarification, with the first point listed below requiring the presentation of few additional simulation results.

We thank Reviewer 2 for this general summary and comment on the paper. In the following, we provide detailed answers to the specific comments.

1) I agree with authors that a reliable assessment of climate change impacts, especially in the Mediterranean area, depends on the use of high resolution information. In this sense, the novelty of the paper stems from the implementation of a downscaling procedure that generates an atmospheric forcing term on an hourly time step and over different points of the catchment. The improvement achieved with this setup, however, is not completely disclosed throughout the manuscript. Authors should therefore define a sort of base line simulation driving the hydrological model with spatially coarser (e.g., one point of the original RCM grid or a weighted average of the contributing points) and temporally (daily) constant climate information. To this aim, authors could arbitrary select one member of the ensemble and make a one-to-one (coarse vs high-resolution setup) comparison. This extra analysis will better highlight the value of the adopted methodology in reproducing changes in the different aspects of the hydrological response of the basin. This additional effort will eventually convey a stronger message to the scientific community.

We completely agree with Reviewer 2 on the importance of showing a comparison between model simulations forced by downscaled versus coarse-resolution forcings. However, we prefer to include this comparison in a future study that we are currently conducting with the aim of evaluating the impacts of climate change on extreme events in the Rio Mannu basin. We believe that differences between model outputs under downscaled versus coarse forcings will be particularly significant when focusing on

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extremes, because of the change in the runoff generation mechanism when rainfall intensity is changed from coarse to disaggregated products. We also point out that conducting a new set of simulations requires a significant amount of time and costs (for the simulations presented in this paper, 880 hours of CPU time over 64 processors were needed), in particular since the funding project of this study has concluded.

2) I found the analysis over the different sub-basins quite interesting. Some additional information, however, could improve the discussion. It is important to define the points of the atmospheric grid contributing to the response of each sub-basin. Indeed, considering their small size some of them are probably driven by the same atmospheric forcing term. In so doing, authors will be able to better distinguish their response in terms of soil properties and atmospheric variations. Moreover, to acknowledge the lack of the buffer effect due to a deeper groundwater table, it is necessary to inform the reader about the range of water table depth within the catchment and between the different sub-basins.

We thank Reviewer 2 for this useful recommendation. To address this comment, in Fig. A (this reply), we have reported the variation in the mean annual precipitation, ΔMAP, as a function of sub-basins contributing areas, A_c. It is apparent that the changes are quite similar among the different sub-basins (mean decrease of about -12%, as also reported in Table 2 of the manuscript for the entire watershed). This suggests that the change in sub-basins response is mostly due to their specific surface and subsurface properties, including the position of the groundwater table. To explore this last issue, Fig. B shows the mean depth of the water table, Nwt, in FUT period. Sub-basins 1–4 and 9, located in the northwest of the basin, have higher Nwt (i.e., deeper groundwater table) as compared to the rest of the sub-watersheds. This supports our interpretation on the reduced buffer effect due to a deeper groundwater table in this group of sub-basins (lines 2-3 on page 17).

Based on this, to address Reviewer's comment:
1) In lines 18-20, page 16, we added this sentence to report the similar variation in mean annual precipitation of all sub-basins:
   "We first point out that the mean annual change in *P* is expected to be fairly constant in all sub-basins (not shown), suggesting that spatial differences may be mostly ascribed to surface and subsurface properties". (We judged not necessary to show also Fig. A from this reply.)

2) We added the plot in Fig. B (this reply) in an additional panel in Fig. 7 to show the mean *Nwt* in the sub-basins and provided comments in the text (line 3 on page 17).

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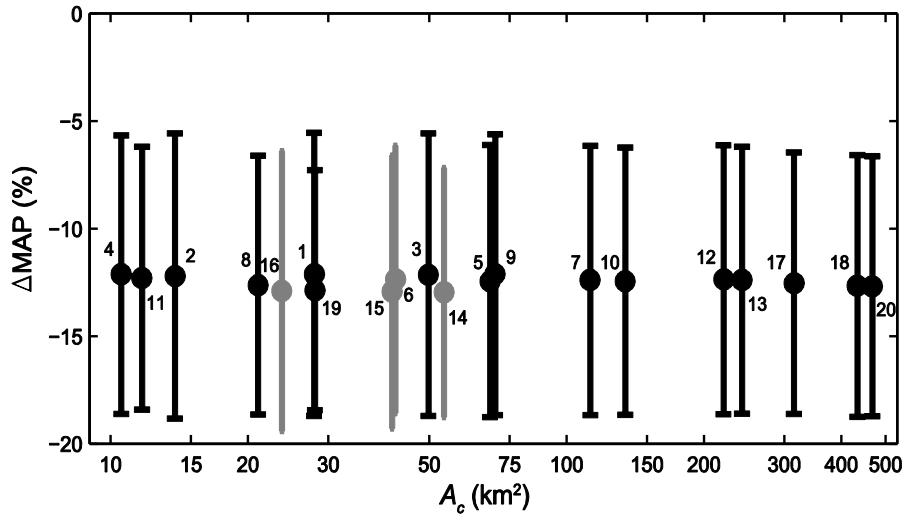
Fig. A. Relation between the change in annual MAP, *ΔMAP*, and sub-basin contributing area, A_c. Bars represent mean ± standard deviation across the CMs. The number of each sub-basin as reported in Fig. 2b and Table 3 of the manuscript is also indicated.
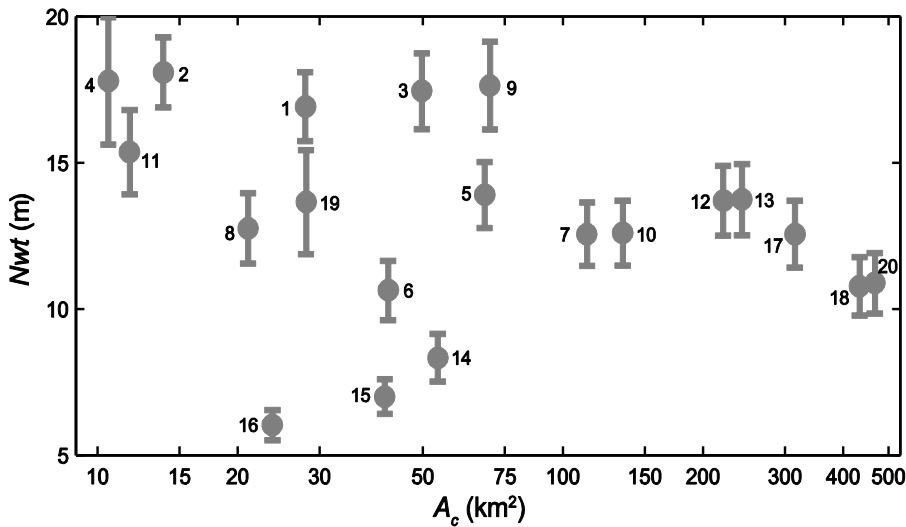

Fig. B. Relation between the mean groundwater table depth, *Nwt,* and sub-basin contributing area, A_c in the FUT period. Bars represent mean ± standard deviation across the CMs. The number of each sub-basin as reported in Fig. 2b and Table 3 of the manuscript is also indicated.

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3) How do authors explain the consistent decrease in Q over winter months shown in Fig.6a without a significant decrease (increase) in P (ETr) illustrated in Fig. 4a (Fig.12a)?

The percentage of variation in mean monthly Q during winter months is affected by the considered CM forcing, ranging from slightly positive (+8% in December for ECH-RMO) to highly negative (-56% in February for HCH-RCA). The reduction of Q occurring in winter months, despite the negligible change in P and ET_R, can be explained as follows. As shown in Fig. 8a of the paper, groundwater exfiltration (GE) runoff accounts for the largest percentage of the total Q. This is true for all months, including winter. Here, we have reported in Fig. C the monthly changes of each runoff type: the GE component is expected to decrease across all year. As a result, since this represents the largest component, the total Q also decreases. This result can be also interpreted as a consequence of the "memory" of the system. The marked decrease in P in all months except for winter leads to a gradual depletion of the groundwater table, which in turn causes a reduction of GE. The small variations predicted for P in winter are not able to affect this process. Thus, Q in winter diminishes as a consequence of what has been happening in the basin before and after the winter months.

To address this comment, in the new manuscript version, we have added this sentence in lines 12-14 on page 15:

"Note that the decrease of Q in months with little variation in P can be mostly ascribed to the diminution of the runoff portion due to groundwater exfiltration occurring throughout the year, as better illustrated below".
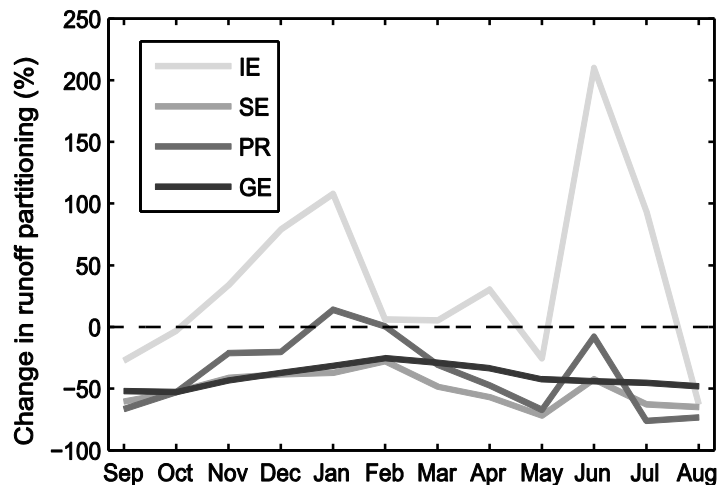

Fig. C. Monthly changes in partitioning of Q at the RMB outlet among the four runoff generation mechanisms. For each month, the mean of the four CMs is reported.

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4) The discussion around the groundwater dynamics seems a bit too short. Additional plots, showing for instance variations in the seasonal groundwater head values, could be useful and shed more lights on the involved processes.

As recommended by Reviewer 2, we inspected the monthly variation of the mean *Nwt* in the basin for each CM. Results are here reported in Fig. D. For each CM, it is clear that the drop of groundwater table is fairly stable for all months, with slight higher values in April and May. Clearly, each CM leads to different magnitudes of the drop, depending on the change in *P*. These considerations were added in the manuscript in lines 5-6 on page 21. We preferred not to add an additional figure due to the relatively limited information of Fig. D and the large number of figures (14) that are already part of the paper.
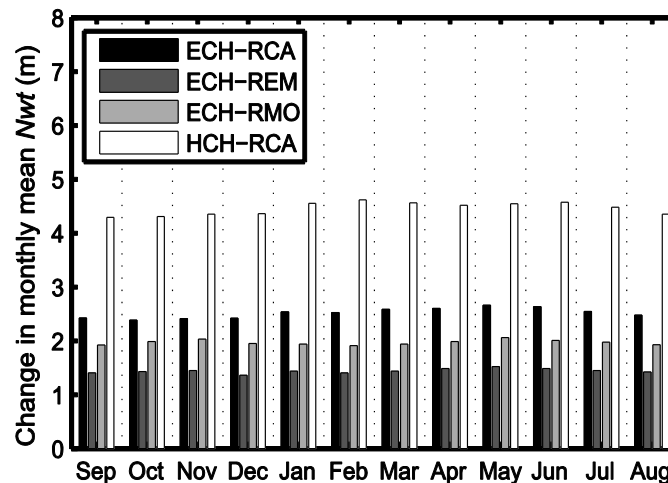

Fig. D. Relative change between FUT and REF periods in mean monthly *Nwt*.

5) In a similar vein to the previous comment, vegetation effect seems completely disregarded by authors. Some comments on this point will be useful as well.

In our simulations, vegetation is involved in two processes: (i) rainfall interception, and (ii) calculation of actual evapotranspiration from potential evapotranspiration computed off-line (this procedure is described in sections 3.1.4 and 3.2). Vegetation parameters have been derived for the land cover classes of Fig. 2a of the manuscript, based on published values for similar land cover classes, including the study of Montaldo et al. (2008) in a similar landscape in Sardinia. This is described in Mascaro et al. (2013), where the parameter values are reported in Table 8.
These considerations were added in manuscript in lines 14-16 on page 12.

Technical corrections
- Please replace throughout the text "real evapotranspiration" with "actual evapotranspiration"

We substitute "actual evapotranspiration" throughout the revised manuscript.

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- Groundwater exfiltration and perched return flow seem more related to the conceptualizationused in the model. Please try to define them (at least the first time in the text) in a more understandable way for the reader.

We provide a definition of the components groundwater exfiltration and perched return flow in paragraph 3.2 of the new manuscript version (lines 16-19 on page 11).

- Please check the y-label in Fig. 12a

We changed the label as "ET_0 or ET_a (mm)".

- Please check "Delrieu" citation.

We corrected this reference.

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