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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Sensitivity analysis of runoff modeling to statistical downscaling models in the western Mediterranean" *by* B. Grouillet et al.

Anonymous Referee #2

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The presented study of Gruilett et al. (2015) is focussing on the analysis of three different statistical downscaling methodologies as boundary conditions for the lumped hydrological model GR4J (Génie Rural à 4 paramètres Journalier). The presented procedure is introduced as a framework to analyse different downscaling products for climate change impact studies with a sensitivity analysis procedure. Therefore the authors used the reanalysis data set of the National Centres for Environmental Prediction/National Centre for Atmospheric Research (NCEP/NCAR) and two general circulation models (GCM's) the CNRM-CM5 from the French National Centre for Meteorological Research and the IPSL-CM5A-MR of the French IPSL Climate Modelling Centre as input data. The data sets of precipitation and temperature were downscaled with the following three statistical downscaling models (SDM): "analogs of atmospheric circu-

lation patterns" (ANA) "cumulative distribution function - transform" (CDFt) "stochastic weather generator" (SWG). Because of lag of meteorological observation data in the Marroquin catchment Loukkos a simple module to estimate potential evapotranspiration is implemented in the hydrological model framework. That equation is based on extraterrestrial radiation and temperature. Four Mediterranean catchments located in the western Mediterranean Sea are firstly calibrated/validated with observed station data of 20 years (1986-2005) on a daily time step based on an aggregation of different objective functions (Nash-Sutcliffe, the log version of the Nash-Sutcliffe, the cumulative volume error and the mean annual volume error) with cross calibration - validation scheme of differential split sample testing. Seven parameters were optimised with the shuffle complex evolution algorithm to the complete time series and to dry and wet years. The validated model setups were driven by the BC of the three SDM's of the two GCM's and reanalysis data set plus the pure data sets of GCM's and reanalysis data (RAW). The hydrological outputs are finally analysed based on different quality values (cumulative volume error, RMSE based on sorted data, and a seasonal, high and low flow Nash-Sutcliffe) in comparison with the simulated runoff of the reference period (1986-2005) driven by observed precipitation and temperature.

The manuscript needs improvement in different directions. The authors present a complex scheme, with a lot of information. Here they should reduce the presented data set to a value where the readers still can follow. The Pyrenean catchment Segre was not well calibrated and the reason therefore can be anything. What is the reason that the Pyrenean catchment Segre is responding during the winter and spring period so different from Irati and Herault? I guess it is more affected by snow processes, than the other three. Higher mountain ranges and the more linear morphology of the channel network could be a reason. That would be a hint of the low quality of the observed runoff data or less representative meteorological stations describing the input signal. Here they can start to reduce the presented material. A short description of the two GCMs (CNRM-CM5 from the French National Centre for Meteorological Research and IPSL-CM5A-MR of the French IPSL Climate Modelling Centre) is missing

in the manuscript. Abbreviation should be explained. The figures are very complex and need more explanation. Scientific English has to be improved and should be reviewed by a native speaker. The authors tend to use long sentences, which were hard to follow. One major point is that they don't show the differences between observed reanalysis data sets and GCM's. It is important to understand the uncertainties, which arise in the meteorological drivers, before analysing the hydrological response. They already discuss that in the manuscript at P10091 25-29. The other point is that it is rather unfair to compare one bias corrected SDM (CDFt) with two uncorrected ones. It is like comparing apples with oranges. For a revised manuscript all SDM should be treated equivalent. They should think about reducing the amount of study sites and maybe integrate one or two additional hydrological models to give a broader view on the uncertainties, which arise through hydrological modelling via the model framework.

Specific comments: P10070, 26-29: Prove English P10073 L20 It is more important to show how many stations of the measurement network could be used for the catchment, than how many stations are available in the complete Ebro catchment. P10073 L22 and L27: How are the laps rates estimated or from which source are they taken? P10074 L16 and P1076 L8: DJF, MAM, JJA, SON is not helpful and can be deleted P10074 L16-20: It is hard to follow that sentence. It needs improvement. How has the regridding been conducted to the GCM to a resolution of 2.5°? How many km are 2.5°? Explain the abbreviation CTRL. P10075, 3-4, 10-11: Check English P10075, L12: Figure 2 is hard to interpret. I cannot identify that 50 % of the catchments respond similar in time. But is that information important for the manuscript? P10075 P17: It is statistically not perfect to use the combination of median and standard deviation and could lead to irritations. Why do they use the median and the standard deviation and not the average with the standard deviation or median with MAD? P10076, L 1: What is 0.44° in km? 3.1.4 and 3.1.3 3.1.4: is only important for the SWG SDM. For sake of simplicity I would merge the two parts and start with the modelling of the occurrence of precipitation. P10077 L15-18: I cannot follow. The SDM is calibrated with the GCM and there is a link to a bias correction? Pleas clarify for all SDM's, how they are calibrated

and validated, which data was used, etc. P10080 L15: What is the reason for the average over 10 days for calibration? The model was not able to represent small runoff effects in time? P10082 L8: What are the criteria's of a dry and wet year? P10082 L11: the hydrological year after American and British system is from the first October to the 30iest September. Just to prevent confusion, the specific system which was used (France?) should be provided or the standard should be used. P10082 L21-23: The difference between what? Validation to calibration? In the figure 4 only calibration or validation is presented. In text and caption the information is missing what they present. I would present both calibration and validation. P10083 L9-15: Prove English, split sentences. As far as I understand the authors correctly they use the simulated runoff data instead of the observed data to minimise the errors. P10084 L10 Equation of the NRMSE is missing. P10085 L6: Check English P10085 L6-L11: That block is already in the caption of the figures. P10085 L15-17: That is not presented in the manuscript, but would be essential to prove the results of meteorological drivers. In figure 6 only the data of the reanalysis is shown, which gave no hint about the effect of the two GCM's. P10086 L6: Unclear, add a table. P10086 L15 the section is hard to follow. An additional table with the specific values would be helpful to check the mean statistics of the volume performance. P10086 L16-18: The outliers' are not clear for me, does that mean in case the simulated absolute value per time step increases 50 % of the simulated runoff driven by observations is classified as an outlier and in that case not taken into account? These values need to be presented in the figure or a table. But in the presented form it is unclear. P10087, L23-25: Improve English, hard to follow. SWG is the worst of the SDM's but it outperforms still the raw data sets and it tends to overestimate the volume. P10088 L13-14: Why is only CDFt affected by snow processes? P1088 l9 and L18: The explanation of the achievement of the NSE criteria is missing: 0.5 for high flows and 0.8 for low flows. Is that information important? There is no additional use of those criteria. P10090 L16-20: It is not clear for me if the method CDFt has an automatically bias correction including that a similar procedure is not used for the other SDM's. In case of the SWG which is the weakest approach it is unfair to

use not bias corrected data sets. P10090 L21: Although P10090 L21-26: But in that study GCM-SDM tandem is not used to predict data and the Nash of 10 days does not allow such interpretations due to the smoothing. That is part of the description of the model not of the discussion conclusion. P10091 L26 I would not write gas emission scenarios, which are the old IPCC scenarios. I would keep it broad and general to all scenario types. P10091 L25-29: That sentence needs simplification, modification and splitting. Here arises the question, why the uncertainty of the GCM's compared to the reanalysis data set is not presented. The uncertainty of the boundary conditions could be used to clarify the range of the uncertainty of hydrology, by expecting that GR4J is a perfect model. They could easily show the uncertainty in the drivers and the used model. Figure 4: The differences are hard to prove especially for the low flows. A log scale here would be helpful. The line in the parameters suggested that they are related, which they are hopefully not. They should use point symbols instead of lines.

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