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Discussions

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

Anonymous Referee #1

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General comments The paper investigates the results with respect to decadal and monthly runoff of applying three different methods of downscaling of daily air temperature and precipitation data from GCM (2.5° spatial resolution) to watershed scale (watershed area around 1000 km^2); Four watersheds belonging to the western Mediterranean region are studied. A reference period of 20 years is considered (1986-2005). The downscaling is relative to inputs from reanalysis data as well as two GCM data. Thus, the analysis deals with nine series of results (3 input data sources * three downscaling methods). A rainfall runoff model is calibrated using in situ input data and runoff observations. Two downscaling methods are found to perform better than the third one in reproducing the simulated runoff obtained when using in situ data as inputs. The illustrations are of good quality. My general comment is about the lack of explana-

tion of downscaling methods used to build the comparison. Also, the discussion of intermediate results (downscaling comparison) is quite missing. My principal critic is about the omission of comparing runoff generated using downscaled data to observed runoff. Specific comments - Authors say that 818 and 264 stations rainfall and climatic stations are available in Ebro basin. What is the size of this basin? What is the link with Segre and Iraki basins (they said they are upstream; are these basins nested?) - Compared to these basins the network of the basin from the South Mediterranean sea is too sparse while authors adopted a 5*5 km^2 grid in the sparse network basin and 8*8 km^2 in the well observed basins. What is the idea behind that?

- page 10074 line 16: The sentence "The calibrated SDMs were forced with three different datasets: NCEP reanalysis data over the 1976–2005 calibration period and with the IPSL-CM5A-MR (Dufresne et al., 2013) and CNRM-CM5 (Voldoire et al., 2013) GCMs, regridded at a 2.5° spatial resolution, over the GCMs historical (or CTRL) period (i.e. 1986–2005). Âż is not clear. First it was necessary for authors to state that 1986-2005 was adopted as control period in line 12 of page 10073. Secondly, authors should begin by explaining the differences between NCEP/NCAR daily reanalysis data and IPSL-CM5A-MR and CNRM-CM5 data. These kind of data are "by essence" different. So a differentiation should be adopted from the beginning. A brief description of the science behind these data should be included (domain, model, data, assumptions ...). Also authors should specify the chosen GCM control period and how it was defined. Why do they use NCEP reanalysis data over the 1976–2005 while the period of control is 1986-2005? - - Page 10074. How are positioned the studied basins in comparison to the 240 grid points of the GCM? It may be important because of frontier effects in interpolation. How many grid points in each basin? A fig. and/or a Table should be added. - For the ANALOG model I would like to see some intermediate results. What are the neighbor days for a given day? Are authors satisfied with this classification? Did they examine its results? - The validation period should be specified before presenting the three methods. The sentence "Thus, two sub-periods of 10 years each divided according 5 to the median annual precipitation for the period were used either for calibration and for validation" should be reported page 10076 otherwise the reader is not aware about the existence of a validation period; .

- How do authors define large-scale atmospheric situation XANA. ? page 10076. Also what do they mean by "the anomalies of the predictors with respect to the seasonal cycle"? Do they look for the most similar situation given the season? they said that ANALOG was calibrated and run on season basis. - For ANALOG method it is important to describe how authors split from the identification of the closest day (from anomaly perspective) to the downscaled data. - In CDFT model what do authors mean by predictor? How do they use these predictors? It is important to specify this aspect and also to compare the maps of daily results obtained from the three downscaling methods. What about persistence aspects? - In 3.2.3 title, authors should add "of hydrological model", because one may think that they are assessing the downscaling quality which is not performed here. - In 3.3 the sentence "the quality of runoff simulations forced by statistically downscaled climate simulations was evaluated" is not reflecting what authors are doing. In effect authors are evaluating to what extent outputs are similar to simulations of runoff forced by observed data, which is not the same thing. Can authors report the discrepancy with observations? - Page 10076. What is the link with "reanalyze grid scale (0.44° spatial resolution)"? - Data in Herault basin were extracted from the SAFRAN 8 km_8 km meteorological analysis system. The key word of reanalysis should be used here in the text.Authors need to write a sentence about the method Âń Safran is a gauge-based analysis system using the Optimal Interpolation (OI) method described by Gandin (1965). From Vidal et al. 2010 https://hal-meteofrance.archives-ouvertes.fr/meteo-00420845/document " One may find in this document conclusions about validation of these data sets. - What is the link between SAFRAN et Xie et al.?; Also authors need to add the reference of Obled et Creutin which is very important point of departure of many works in the same field Âń Creutin and Obled (1982) examined several well-known schemes and recommended the optimal interpolation (OI) of Gandin (1965). Âż that's what said Xie and al.

- Xie et al. said that "while similar performance statistics can be achieved by other inverse-distance interpolation algorithms if the anomaly, instead of the total, is interpolated" . Now it is the anomaly which is interpreted in the present case?

- The daily climatology used in Xie et al. is not the median value "First time series of 1978–97 20-yr mean daily precipitation are calculated for the 365 calendar days for all stations with 80% or higher reporting rates. Fourier truncation is then performed for the 365-day time series of raw mean daily precipitation, and the accumulation of the first six harmonic components is defined as the daily climatology of precipitation at the stations." What did authors do exactly in the present work?

- " interpolating the ratio of total rainfall to the climatology, instead of the total rainfall itself, the OI is capable of better representing the spatial distribution of precipitation, especially over regions with substantial orographic effects [Xie et al., 2007]."in Chen et al. ftp://ftp.cpc.ncep.noaa.gov/precip/CPC_UNI_PRCP/GAUGE_GLB/DOCU/Chen_et_al_2008 ; Did authors interpolated the ratio ?

- the IDW method should be documented and reported with the key reference of Shepard. Its quality assessment (See Chen et al. 2008) should be reported.

- In Eq 1 and 2 Do authors have an idea about the statistical properties of the anomaly defined in this way?

- In Eq 3 Xd and Xc are not specified. What is meant by "large scale situation"? Do authors map the anomalies before describing large scale situation?

- ANALOG method: the approach of Yiou should be briefly presented. Also, this approach has been criticized. Authors should report about these critics.

- In CDF method it is improper to write that it is from "local scale observations". because authors don't use gauging data (observations) but interpolated data. What is done in Vrac et al. 2012 should be briefly reported here. Otherwise a normal reader of the journal will spend a lot of time in reading the bibliography cited.

Authors have to simplify the reading by giving the methodology you used and not always refer to the other works. - The choice of the predictors should be explained as it is the case for example in Vrac et YIOU 2010 (paragraph [13]) http://onlinelibrary.wiley.com/wol1/doi/10.1029/2009JD012871/full

- Page 10083 "To facilitate interpretation and to limit biases in hydrological modeling when comparing downscaled climate-based hydrological simulations, in the following, the whole period hydrological simulation is used as a reference instead of the observation time series." This is a critical point. Why do authors do so? Bias in hydrological modeling is generally related to the difference between observations and predictions (of runoff). May authors present the results when the observations are used as reference? - The GR4 (six parameters) was calibrated on 10 days time step. (page 10080). It is important to say this in the abstract. - The reference Dezetter and al. 2014 is not possible to download.

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