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Hydrology and Earth System Sciences Discussions

Interactive comment on "Improving runoff estimates from regional climate models: a performance analysis in Spain" *by* D. González-Zeas et al.

Anonymous Referee #2

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General comment

The paper by Gonzales-Zeas et al. focuses on the assessment and evaluation of monthly runoff time series derived from the bias-corrected output of 10 regional climate model simulations, comparing the results of a variety of simple interpolation methods against 'observed values' (which in fact were estimated through applying the hydrological model SIMPA, due to a lack of gauging data reflecting natural flow conditions) for Spain's mainland, comprising 338 sub-basins. For further comparison to the application of direct surface runoff products from RCM, five functional descriptions of the aridity index and the UNH/GRDC global runoff data set is used. The rationale of this

study is to reveal the applicability of direct and processed RCM outputs of runoff series for large scale areas, for which no reliable calibration of distributed hydrological models is feasible. The value of this paper for the scientific community can be seen in the demonstration that reasonable runoff estimates from RCM data seem achievable, even in applying only rather simple techniques. Overall, the paper is well written, generally well structured and relatively concise in its description of applied methods and results. However, I think that in some parts it is a little bit too simplistic and straightforward so that certain elements could and should be addressed in more detail (while others could be left out with no harm), to actually explain the limitations and inaccuracies and specific conditions under which the proposed approach(es) can be beneficial.

Specific comments to the authors

Abstract: I do have a problem with the first sentence to start with, because this is clearly nothing you address any further in your paper – monthly time series of present state runoff estimates are by itself not at all important to assess the impact of climate change on (future) water availability: The methods applied to obtain good estimates from RCM are (as you claim)! But once bias-correction is involved, you carefully need to argue whether you trust that BC-terms/factors can still be valid for future climate, especially when considering the loss of (physical) data consistency through bias-correcting only one or few variables from an RCM (but all this is a much more critical debate then the one you should raise here). I am fully in line with you that your proposed methodology proves valuable and helpful when trying to provide estimates for runoff conditions in current state climate and thus value-adding RCM outputs for an application in water resources management (models) (as explained later on).

Introduction: You provide a very comprehensive overview on current issues and problems in large scale hydrology and explain reasonably well the potential and limits of RCM data with regard to relevant literature. Again, I am not so sure whether you should try and make that link to CC impact analysis.

Chapter 2.2: Can you please comment on the performance of the calibrated SIMPA model over Spain, i.e. what is the quality of the reference you are using? Since it has been calibrated (validated?), I assume such information should be given to the reader without a need to find the Spanish reference.

Chapter 2.3: You have been using 10 RCMs (eight models, 1 in three members), all driven by the same GCM. Can you please comment on why you made that choice and why you didn't consider to use a set of different RCMs driven by different GCMs? It may well be that this has been considered to be outside the scope of your paper, but I think that it is quite crucial to understand your intention behind using 10 RCM model outputs, especially since you seem to average them later on, which I think is not really valid.

Chapter 3.1: a) The fact that you actually considered the impact of the two available coordinate systems is good and thoughtful. b) The interpolation due to the scale mismatch between RCM and hydrological units is obviously necessary, yet the schemes you apply are really the most simple ones available. I trust that this is ok for the purpose of your study, but I think you should indicate the vast availability of more sophisticated methods (e.g. of purely higher statistical order or even mass/energy-conserving approaches considering the underlying topography that comes into play when you turn fluxes into flows) by ways of quoting relevant literature.

Chapter 3.2: a) Did you ever look at PET in the RCM output data? If no, why not (not available to you?), if so, how does it compare to your Hargreaves estimate (Eq. 3)? If there should be a mismatch (which I would definitely expect), what does that imply on your runoff estimates with regard to closing the water balance?

Chapter 4.1: Is there a reasonable explanation why O-D outperforms the other interpolation schemes (Fig 4a and b)? If yes, please do explain. As you mention, the deviances among the four alternatives are markedly small (probably not significant?) – are they arbitrary?

Chapter 4.2 + 4.3 With regard to Eq. 6 and in the light of the monthly time series you are providing and evaluating later on, have some doubts that this simple approach is feasible here. In applying only an annual correction factor, which you then superimpose on each monthly runoff value, you are accounting only for the bias in runoff volume. In ranking according to best performance later on, your approach totally neglects that certain RCMs may perform bad in terms of a bias in volume (which is fairly easy to correct), but are capable to represent the annual hydrological cycle (which would be much more difficult to compensate, if the seasonality is not preserved. . .). This becomes evident in Figure 7, for which you claim that the 'corrected RCM series adequately represent the seasonal cycle' (on page 191, line 19). This has not much to do with your correction method, does it? Please comment.

Page 190, line 26: the bias in RCMs is not 'inherent' per se. please avoid this term.

Fig. 6 and following explanation on page 191, line 23: You cannot draw a serious conclusion from averaging runoff from 10 different RCMs.

Page 192, line 1-2: The NS values you indicate are referring to what? A time series of monthly runoff values or the cumulative probability distributions? In the latter case, I don't think that NS is the appropriate objective function and you should find a more appropriate measure.

Page 192, line 11f. and Fig. 8): Again, I don't think you should show the average of 10 RCMs, but much rather I advise to use a boxplot, indicating all 10, but also the mean and median of the 10.

Figure 8: Can you also show the NS values when referring to the lines given in Figure 7? Does that make a difference?

Chapter 5: a) Page 193, line 12-18: The link that is made here is not clear to me. You claim that the simple disaggregation scheme O-D has been applied in earlier studies, but proved to be not very efficient due to the absence of a bias-correction step. I think

that these processing steps are to be seen independently, there is no causal link – and the impact of bias-correction clearly dominates the choice of interpolation scheme, no? b) Page 193, lines 26 and 27: contents repeated c) In your 'conclusion', it would be helpful to add some more concluding explanation to the description of performances. What do we actually learn from your findings, if in some cases the RCM data, sometimes the Schreiber and mostly the UNH data provide best results? d) The regional differences in your results are quite interesting and I think you present some logical arguments (e.g. catchment size) to explain the findings. Still, I am not convinced that there isn't more in this. Is there any geographical context and some clear causes for these obvious differences? Could you provide an answer on why Schreiber's formula works best in certain basins while it fails in others?

I kindly ask you to thoroughly consider these aspects in your revision. I am convinced that it would help me (and other readers, I suppose) a lot in better understanding the findings of your study.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 175, 2012.