Manuscript: Assessing climate change impacts on daily streamflow in California: the utility of daily large-scale climate data, E. P. Maurer, H. G. Hidalgo, T. Das, M. D. Dettinger, and D. R. Cayan, *Hydrol. Earth Syst. Sci. Discuss.*, 7, 1209-1243, 2010

Responses to referee #2 comments (original comments in normal type, responses in *red italics*):

RC C200: 'Review of paper "Assessing climate change impacts on daily streamflow in California: the utility of daily large-scale climate data, by Maurer et al., HESS-2010-35', Anonymous Referee #2

GENERAL COMMENTS

The authors apply three different downscaling methods (BCSD, CA and BCCA) to precipitation and temperature data from the NCEP/NCAR Reanalysis. The downscaled fields are used to force a hydrologic model. The output from the hydrologic model forced using downscaled data and observations are compared to evaluate the skill.

The paper addresses a relevant scientific question, since the development of new downscaling methods is currently an interesting scientific endeavour. The paper presents a new method, BCCA, and I find the results interesting. They show that improvements in the downscaling process can still be achieved.

I find particularly interesting the development of the new BCCA method and the comparison of its results with the ones by CA. The role of daily variability in the better performance of the downscaling models for extreme hydrologic events is another interesting piece of information yield by the study.

I find no major problems with the methods and assumptions, although I find that the verification can be more thoroughly assessed using alternative procedures, which I recommend for forthcoming studies.

However, I seriously think that authors must better describe their methodology, so that experiments can be repeated by other authors. The reader must sometimes guess what the authors have done. I find the manuscript must be improved in this regard, methodology should be better described.

Please see revisions noted below in response to this and subsequent comments related to description of methods.

I find the study interesting and I recommend publication after some (I think minor) changes are introduced in the study.

I find that the reader must guess a lot and refer to previous papers by the authors to understand what they do in their BCCA method. I think this must be specifically explained in this paper. It is mentioned in one phrase (page 1219, lines 20-22), but it must be explained in a comprehensive way.

In the revised manuscript, Section 2.2, especially the first paragraph, has been expanded to include more description of both the BCSD and CA methods. The fifth paragraph of section 3.1 contains the description of the addition of the 'BC' to the CA method.

I don't think that the results would change significantly, so, I don't think reworking the study is needed. However, for next studies I urge the authors to use cross-validation in the skill assessment (see, for instance several works by Feddersen and coworkers). The use of two separate periods is risky, since these two periods might not be very similar between each other due to low frequency variability affecting the results. Additionally, it would allow the authors to use longer databases for analogue searches.

We appreciate the excellent suggestion for improving future studies. A short paragraph has been added to the revised manuscript, the last paragraph of section 2.1, which mentions that a more robust evaluation of the methods could be achieved with cross-validation.

I think that the paper is convincing in the message that it conveys, in the sense that the verification is done against the results of the hydrologic model forced by observed gridded data. This way, the error (or different skill scores) is solely dependent on the downscaling method. However, I am curious about the performance of the whole system. This could only be evaluated comparing observed flow quantities with simulated flow quantities. Do the authors have any hint about these verification scores?

The revised manuscript, in the first paragraph of section 2.3, includes reference to a prior study, with the same implementation of the VIC model, where observed flows at four of the sites used in this study were compared with flows simulated using gridded observations. The model performed very well at those sites.

Additionally, all the verification scores used are concerned with the probability density function of the streamflows. However, as Figure 8 shows, there are other features that could be checked, such as the correlation coefficient, the root mean square error or the fraction of variance between "observed" streamflows (driven by observed fields) and simulated ones. These verification scores could point toward other weaknesses-strengths of the downscaling models. This could enlarge the amount of information yield by the study.

As the reviewer notes above, we focus on comparing the hydrologic model forced with observed meteorology to that forced by downscaled meteorology, to assure the dependence is on downscaling method alone. In response to comment 1 by anonymous referee #1 we added two additional statistical tests to assess the importance of location and scale explicitly in addition to testing for the entire probability density function. A summary of the test results are in the new Table 2 in the revised manuscript, and the new text discussing this is contained in the first paragraph of section 3.2 of the revised manuscript. Using these non-parametric tests, we conclude that a shift in location of the distribution is the primary cause of the shortcomings of the CA and BCSD method, as opposed to changes (or biases) in variability. Additionally, our preference for statistical tests in lieu of summary statistics like RMSE or correlation are explained in the first paragraph of section 2 in the revised manuscript.

TECHNICAL CORRECTIONS

I would suggest more emphasis on the methodological aspects rather than "climate change impacts" in the title. Perhaps just changing the order of the title would help, something like: "The utility of daily

large-scale climate data in the assessment of climate change impacts on daily streamflow in California". As written in the current version, it initially seems to indicate that the impact of climate change has been assessed, indicating a more applied perspective, whilst the paper mainly works methodological aspects of the problem.

The title has been changed as suggested.

p 1217, line 1. I don't agree with the authors when they state that CA needs gridded observations. There are plenty of studies using CA (see, for instance Zorita and von Storch, 1999 and many papers citing this one) which do not use gridded observations. Perhaps in the particular structure used by the authors, it is so, but not in general. it is not a specific feature of the method. I would remove this mention.

The term 'gridded' has been removed to avoid the incorrect implication that the analog method requires gridded fields of observations.

The color legend used in Figure 5 should be added to Figures 7 and 9 to help readability.

the legends have been added to both Figures 7 and 9 as suggested.

My English is far from perfect, but I think there are some evident typos.

p 1213, line 1: The methods takes -> The methods take

p 1214, line 1. Is there a difference -> Is there any difference

p 1214, line 21. In addition the -> In addition, the

p 1215, 1st paragraph. I would say that the use of "hydrology model" versus "hydrologic model" should be consistent. See line 1 and line 2.

p 1215, line 9. "comparable recent GCMs" -> "comparable to recent GCMs"

p 1216, line 3, "also a changes" -> also changes

We are grateful for the careful reading and for alerting us to these typos. All have been corrected in the revised manuscript.

References

Feddersen, H., A. Navarra, and M.N. Ward, 1999: Reduction of Model Systematic Error by Statistical Correction for Dynamical Seasonal Predictions. J. Climate, 12, 1974–1989.

Feddersen ANNALS OF GEOPHYSICS, VOL. 46, N. 1, February 2003

Zorita, E. and H. von Storch, 1999: The analog method - a simple statistical downscaling technique: comparison with more complicated methods. - J. Climate 12: 2474-2489