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Hydrology and Earth System Sciences Discussions

Interactive comment on "Utility of daily vs. monthly large-scale climate data: an intercomparison of two statistical downscaling methods" *by* E. P. Maurer and H. G. Hidalgo

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We are grateful to the reviewer for the thoughtful and careful reading of the manuscript, and the helpful comments. Our responses to each comment are listed below, following the reviewer's numbering.

1. The reviewer has pointed us toward a very good distinction between the two techniques that is now discussed in the text of the revised manuscript. While the lack of explicit bias correction in the "perfect prog" CA approach would be a strong distinction, it is limited in this context to neglecting biases in variability, since the CA process is based on anomalies (which was not clear in the original text, which has been corrected

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in response to comment 3 below). In particular, section 2.3 has been revised to include the following paragraph:

"This method is in essence a model output statistics (MOS) approach (Glahn and Lowry, 1972; Wilks, 2006) at the monthly level, providing a post-processing of model output to correct for model biases relative to observations, where biases may due, for example, to imperfect model parameterization of physical processes or inadequate topographic description in the model. The types of biases that can be accommodated by the BCSD approach are restricted by the method"s use of large scale precipitation and temperature, as opposed to, for example, remote predictors such as geopotential heights that could in theory be used to correct for model biases in large scale circulation. However, Widmann et al. (2003) note that using local predictors such as precipitation can perform adequately compared to other downscaling methods. When applied to GCM output in a climate change context, the BCSD method, as with any MOS-type approach, is model dependent and assumes that the relation between model scale and disaggregated, fine scale climate is constant; the method will be less effective where the relationship changes."

Further, a new first paragraph of section 2.4 has been added: "The CA method, described in detail by Hidalgo et al. (2007), is essentially a "perfect prog" method (Glahn and Lowry, 1972; Klein et al., 1959), and is thus fundamentally different from the MOS-type BCSD approach (Widmann et al., 2003; Wilks, 2006). In particular, CA, as with any perfect prog approach, makes no adjustment to correct for biases, but rather relates model-simulated variables (in this application, anomalies of daily precipitation and temperature) to predicted variables, using relationships established with observations. As discussed in section 2.1 above, this distinction bears on the interpretation of the intercomparison of CA with BCSD, which statistically corrects for model biases, since where Reanalysis exhibits substantial biases, attributing the differences to these biases or downscaling approaches may be problematic. Since CA is based not on absolute simulated values but on anomalies, the primary biases that would affect the downscal-

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ing results would be related to spatial and temporal variability, for which no adjustment is made to account for differences between the simulated (reanalysis) and observed values."

To explain more clearly our motivation in comparing these "perfect prog" and MOS approaches here, the revised last paragraph in Section 1 includes the following sentences: "By using these two techniques, and by using a Reanalysis as a surrogate for a best-case (unbiased) GCM, we aim in this study to explore the potential gain in downscaled daily skill by downscaling daily, as opposed to monthly, GCM output. While there are other candidate methods that could have been utilized in an intercomparison exercise, these two methods were selected since the have been implemented over the Western United States, and will be included in future work on climate change impacts in California (Cayan et al., 2007)."

In addition, the fourth paragraph of section 3.1 has been added to elaborate on the MOS vs. perfect prog distinction, stating: "The overall similarity of skills between the CA and BCSD techniques for downscaled monthly P and T should be interpreted in light of the distinctive properties of the methods. As noted above, the "perfect prog" CA approach makes no adjustment for possible biases in the large-scale spatial and temporal variability of reanalysis P and T, while the "MOS" BCSD explicitly corrects any biases at the monthly level. Thus, the similarity in skills for downscaling monthly T and P, where daily variability is not yet considered, indicates that the biases in reanalysis at this scale are not large enough to affect in a substantial way the downscaling skill."

2. The reviewer mentions two general shortcomings of the manuscript on the use of reanalysis data as a "surrogate GCM" in this study: the distinction between reanalysis precipitation and temperature; and the implications of this on the intercomparison study.

To address this and improve the paper, section 2.1 in the revised manuscript has been revised to include the following third and fourth paragraphs:

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"While the objective of these methods is to downscale actual GCM output, intercomparing these two techniques using GCM output would be problematic since the skill assessment could not distinguish between biases related to downscaling and biases related to GCM process simulation. Thus, using reanalysis data provides a more objective measure of the skill of the downscaling techniques.

"In the NCEP/NCAR reanalysis, observations of air temperature are assimilated into the model, resulting in reanalysis temperatures that are close to observations. Precipitation, on the other hand, does not benefit from assimilation of observations, and is a product of the reanalysis model, and can thus exhibit substantial regional biases (Maurer et al., 2001; Widmann and Bretherton, 2000; Wilby et al., 2000). Arguably, due to the assimilation of atmospheric observations, reanalysis represents the best possible simulation capability of a GCM. Because reanalysis T is strongly connected to observations, the comparisons of T skill will reflect differences almost exclusively in the downscaling techniques; because reanalysis P is not related to observations, the intercomparison will reflect differences between the downscaling methods, plus some influence of the reanalysis P bias. The P and T daily variability in the reanalysis has been shown to be plausible in some locations in the Western U.S. (Widmann and Bretherton, 2000), and the existence of skill in daily statistics of GCM output will be a major factor distinguishing the downscaling methods compared in this study."

3) The reviewer's comment, that the CA technique is inadequately described, is addressed in the revised manuscript, section 2.4. The revised description is now more than double the original length, and does not rely to any substantial degree on references to the Hidalgo et al. manuscript in review. The revised text includes the mention of predictor variables, and a sentence explaining that the method works on anomalies, not absolute values.

Minor Comments:

4) As suggested, "complete" has replaced "continuous in paragraph 2 of the introduc-

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tion.

5) Citation of the new IPCC AR4 chapter on regional climate change/downscaling is in the revised text, paragraph 2 of the introduction.

6) The word "continuous" has been removed from the 4th paragraph of the introduction to add clarity to the text.

7) To clarify the sentence, it has been rewritten to include: "...the monthly patterns are conserved but daily (intra-month) patterns are resampled randomly, and therefore the correspondence between downscaled and historic daily observations is not conserved."

8) Section 2.1, paragraph 1 has been revised to cite the Widmann and Bretherton article in which the rescaling to PRISM climatologies was developed.

9) Section 2.2, Paragraph 1, is clarified by including the following; "The PDO influences North American climate in a similar manner to the El Niño Southern Oscillation (ENSO), with discernable differences in precipitation, temperature, and streamflow, under different PDO phases (Cayan, 1996; Hamlet and Lettenmaier, 1999)."

10) Section 2.2, paragraph 1, a one tailed test was used because the hypotheses being tested were whether the later period was warmer than the earlier, and wetter than the earlier, not just whether the two periods were different. This is clarified in the revised paragraph, which now includes the text: "The magnitude of observed warming trends in the Western U.S. of 1-3°C over the second half of the 20th century are non-uniform through the region and are not fully explained by the PDO shift (Stewart et al., 2005). Precipitation trends over recent decades are even more non-uniform spatially and variable through time (Mote et al., 2005), though a large-scale wetting trend is evident through the last few decades of the 20th century (Groisman et al., 2004)."

11) To paragraph 2 in section 2.3, the following clarification has been added to the text: "Following Wood et al. (2004), screening of scale values for precipitation were

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applied to avoid unreasonable precipitation values. For example, if the randomly selected month has two or fewer days with precipitation and the scale factor is greater than three, another year with more wet days is randomly selected to avoid creating unreasonably intense precipitation."

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 3413, 2007.