Hydrol. Earth Syst. Sci. Discuss., 4, S1290–S1295, 2007
www.hydrol-earth-syst-sci-discuss.net/4/S1290/2007/

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

Anonymous Referee #1

Received and published: 26 October 2007

This paper presents a comparison of two downscaling methods: 'Constructed Analogues' (CA) and 'Bias Correction and Spatial Downscaling' (BCSD). CA is a modification of the analogue method, using linear combinations of several historical analogues. BSCD is a quantile-based mapping of simulated probability distributions onto observed distributions. CA is applied to daily data, whereas BSCD is applied to monthly data, which in a subsequent step are disaggregated into daily values using rescaled, randomly selected historical months.

Both downscaling models are fitted using the early part of the NCEP reanalysis and

then applied to the later part and validated against gridded observation. Skill measures include correlations of monthly data, seasonal correlations of daily statistics for wet and dry days, as well as some other measures.

Both downscaling methods are potentially useful for downscaling GCM simulations for future climate, and the results that are presented are interesting and give a good indication of the skill of the methods in the reanalysis set-up. The manuscript is thus in principle suited for publication in HESS. However, there are substantial problems related to the explanation of the conceptual basis and to putting the study in a larger context. In addition some key aspects of the methods are not satisfactorily explained. In my opinion major revisions are need to make the manuscript suitable for publication.

MAJOR COMMENTS

1.) The authors do not mention the most fundamental difference between the two downscaling methods. CA is a 'perfect prog' downscaling method, which uses statistical relationships that hold in the real world and applies them to simulated predictors. This approach yields only meaningful results if the simulated predictors are realistically simulated (which led to the name 'perfect prognosis'). In contrast BSCD is a Model Output Statistics (MOS) approach, in which unrealistically simulated variables are partly corrected by applying a statistical model that use these variables as predictors and observations as predictands. The MOS methods thus do usually not describe real world relationships. This difference is discussed for instance in Widmann, Bretherton and Salathe, J. Climate 2003, but also in many other texts on statistical downscaling.

It should be noted that the BSCD method only uses local predictors, which considerably restricts the type of simulation errors that can be corrected. It should also be noted that BSCD can in principle be applied directly to GCM simulations other than reanalyses, which is not always the case for MOS methods (see Widmann et al. 2003). As MOS downscaling is model-dependent, this is crucial for the application to climate change simulations, which the authors have in mind. However, some of the differences

HESSD

4, S1290–S1295, 2007

between the distribution of a variable simulated in a standard forced GCM simulations and the observed distribution may be due to the differences in the simulated and observed synoptic-scale situations (which are essentially random), and thus the fitting of BSCD to GCMs is not without problems.

These are crucial conceptual issues and the revised manuscript should include a discussion on fitting BSCD to standard GCM simulations rather than reanalyses.The authors should also clearly motivate why they compare a perfect prog with a MOS approach and consider the fundamental difference when discussing and comparing the skills of the two methods. As there are many other possible perfect prog and MOS approaches, the authors should also motivate their specific choice of downscaling methods.

2.) The authors state that the application they have in mind are GCM simulations for climate change. In order to assess the skill of the downscaling methods in a situation most similar to the intended application, one would need to apply them to forced GCM simulations, or instance for the second half of the 20th century, using the first half for fitting the BSCD MOS and for defining the CA library. The problem with this approach is that the skill assessment can not distinguish between errors directly related to the downscaling methods and those related to random differences between simulated and observed synoptic situations. I suppose that this problem is the reason why the authors have decided to use a reanalysis-based skill assessment.

It should be noted that temperature and precipitation, which are the two variables considered in this study, are fundamentally different in a reanalysis. Temperature observations are assimilated, keeping the simulated temperature in many cases close to the observations (although their may be differences, for instance due to unrealistically simulated soil moisture and/or precipitation) whereas precipitation is not assimilated and thus can have substantial errors. The consequences of this difference for the usefulness of the validation, in particular for comparing BSCD-corrected temperatures, which use simulated temperatures based on assimilated observed temperatures, against ob-

HESSD

4, S1290–S1295, 2007

servations need to be clarified.

Unfortunately the authors do not comment on their general skill assessment approach and the reason why they chose a reanalysis to test the downscaling models. There is only a very vague comment that the reanalysis is a 'surrogate GCM', but the questions why a surrogate rather than a GCM is chosen remains open. This aspect should be clearly addressed in the revised manuscript.

3.) The CA method is poorly described.

Firstly, there is almost no information on which meteorological variables, and which atmospheric levels and spatial domains are used to define the analogues. The reference to Hidalgo et al (2007) is not sufficient, because the choice of predictors is one of the most important aspects of any downscaling method, and thus has to be described well within a paper. In addition Hidalgo et al (2007) is still is in review and thus is currently not accessible to the readers.

Secondly, the statistical downscaling method itself is not clearly described. If I understand correctly, the product of the four Z matrices in eqn. (1) are simply the regression coefficients obtained when using multiple linear regression to estimate Z_obs from the 30 pedictor patterns included in Z_analogues. P_downscaled is then obtaiened by applying these regression coefficients to the 30 precipitation analogues. This approach is perfectly justified and straightforward. Unfortunately the text does not explain the basic ideas behind the approach and the reader is left at guessing from eqn. (1) what has actually been done.

In my opinion it would also be better to have either no square bracket at all in eqn. (1) (it is not needed), or enclose all four Z matrices in it, so it contains the regression coefficients and the basic structure of regression coefficients (covariance/variance) becomes obvious rather than obscured.

It should also be said that all matrices are based on nomalies rather absolute values,

HESSD

4, S1290–S1295, 2007

(if my understanding is correct).

MINOR COMMENTS

4.) introduction, paragraph2

Wrong use of 'continous', which is the opposite of discrete. A better word might be 'complete'

5.) introduction, paragraph2

The downscaling chapter of IPCC AR4 should be included in the citations, it is one of the best overviews currently available.

6.) introduction, paragraph4

Again wrong use of 'continous'. In addition it is not clear whether the comment refers to space or time.

7.) introduction, paragraph5

The meaning of 'daily correspondence' schould be clarified.

8.) 2.1, paragraph1

As far as I know the scaling of gridded observations to match PRISM climatologies was first proposed and described by Widmann and Bretherton (2000), so the paper should be referenced in this context.

9.) 2.2, paragraph1

What means 'the PDO influences North American climate in a similar manner to ENSO'? Be more specific.

10.) 2.2, paragraph1

Why is a one-tailed rather than a two-tailed test used?

HESSD

4, S1290–S1295, 2007

11.) 2.3, paragraph2

The method of estimating daily variability in the BSCD approach should be discussed more. For instance, what happens if a randomly chosen dry month with only a few days of precipitation gets scaled to match a wet monthly mean. Is there not a problem with getting unrealistically high daily values in this case?

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