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Climate of the Past Discussions

Interactive comment on "On the verification of climate reconstructions" *by* G. Bürger and U. Cubasch

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The rev. acknowledges that our approach to the verification of climate reconstructions via resampling (bootstrapping) is well suited. The rev. also states that the paper contains no novel concepts, ideas, tools, or data, and that its presentation lacks focus and diligence in presenting that approach.

We emphasize that such a verification attempt has been made for the first time. Moreover, several of the rev.'s criticisms are based on a misunderstanding of what was actually intended and done, which we attribute to a too condensed and incomplete explanation of methods on our side.

We are confident that our response to the specific comments below will resolve most of the outstanding statistical issues.

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Answers to the specific comments:

1. Yes, we should explicitely mention RE as the used skill. "degrees of freedom" was used in an exploratory manner. If that is confusing, it can of course be removed.

2. That "subset of data" represents what is available to reconstruct the past millennium (back to AD 1400). It serves as the main source for the millennial reconstructions of Mann et al., 1998/9, Rutherford et al., 2005, Mann et al., 2005. To clarify our subject, title and abstract should contain the term "millennium".

3. "detrending". - See the comment of Eduardo Zorita (S180). Moreover, it is not per se impossible that interannual proxy-temperature covariations, which are far more verifiable, have the same structure as decadal or centennial.

4. "error growth". - That paragraph summarizes the critique raised by von Storch et al., 2004 and Bürger and Cubasch, 2005. The wording is taken from the abstract. This can be expanded to become more self-contained.

5. "population". - That depends on the time scale by which "climate" is defined. Population as the key notion for hypothesis testing is used, for example, when a double CO2 experiment is compared to the basic state, which is often the climate of 1961-1990, or the preindustiral climate etc. Therefore, for that limited time horizon samples might very well belong to different populations if they are drawn from different periods.

The number of missing temperature grid points was meant as an example of a nonsense predictor; we could have used other trended series. And since we have solely worked with the subset of temperature grid points (219) with very few missing values (a total of 55 out of 30660), missingness is not an issue at all.

The case that temperature data are not missing at random, e.g. if one uses the full grid, is interesting in itself but incompatible with our setting (which requires that the set of available grid points is stationary/constant in time). Do we interpret the rev. correctly that if missingness is not dealt with adequately, such as in Mann et al, 1998 or

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Rutherford et al., 2005, a key condition is not met to successfully reconstruct climate?

6. Calibration/validation is done basically as in Rutherford et al., 2005. That is, for all times of the validation period the temperature records are set to missing in the corresponding calibration. Note, however, that Rutherford et al., 2005 have done initial infilling of temperature data which introduces a positive verification bias (see our response to rev. 2).

7. The RegEM iteration belongs to the second step (COV). RegEM was used solely for the estimation of mean and covariance. The infilling was done using the models defined under MDL. If RegEM converged, applying the same model as during the iteration (in our case: ridge regression with normalized variables) is equivalent to using the values imputed by RegEM in the COV step. But for the MDL step we specifically allowed for other models.

8. The definition of R is in fact missing. It should be introduced before via T = R * P.

9. We fully agree with the rev. that rescaling is not the optimum way to attain realistic variances (although we would not call it a misconception). We have included rescaling as it was one of the steps used by Mann et al., 1998. Regarding the mentioned alternative, the rev. is possibly aware of the variance attenuation observed for RegEM, as mentioned in Schneider, 2001.

10. A statement about the effective rank of the regression problem would have been useful when RegEM was first used for millennial climate reconstructions. Besides, the predictor proxies are quite well behaved and have full effetive rank (22). So, it remains unresolved why RegEM was used instead of the much simpler EM.

It is true (see below) that in our MDL step we used a fairly strong regularization for TTLS, being guided by verification performance. It is an interesting issue (which we have not checked as it is internal to RegEM) how this relates to the degree of regularization in the RegEM iteration.

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The entire RegEM discussion should be placed in the main text.

11. "convergence of RegEM". - We have used the default stagnation tolerance of 5e-3 from the RegEM code, so this might have been to harsh and prevented convergence. Choosing a larger tolerance (as the rev. recommends) should have a similar effect as decreasing the iteration limit to 50 (which we did).

12. We have used the defaults offered by the RegEM code, that is:

OPTIONS.regress = 'mridge'; OPTIONS.stagtol = 3e-5 ; OPTIONS.maxit = 50 ; OPTIONS.inflation = 1; OPTIONS.disp = 1 ; OPTIONS.relvar_res = 0.05; OPTIONS.minvarfrac=0.95; OPTIONS.neigs = 100 ;

Using 5 as a TTLS truncation parameter was guided by tests perfomed with the classical verification. For many of the flavors, 6 was an optimum, and sometimes smaller values. We agree that this check should have been mentioned somewhere. Note that here we are in conflict with our own general warning to use independent selection criteria for such parameters, as otherwise the verification is biased.

Interactive comment on Clim. Past Discuss., 2, 357, 2006.