**Climate
of the Past
Discussions**

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

**Anonymous Referee #3**

Received and published: 22 August 2006

Several erroneous statements and misconceptions about elementary statistics in BC's latest reply I do not want to let stand uncorrected. (The numbers again refer to the numbers of the specific comments in my earlier review and in BC's replies.)

2. BC's reply does not address my criticism, which was that broad methodological claims and conclusions were based on an analysis of narrow scope. The criticism still stands.

3. BC's statement that there is no consistency problem with detrending as apparently practiced by some is based on an erroneous justification.

   Detrending proxies and instrumental temperatures, as the authors state, amounts to using the stochastic model (1) in my previous discussion contribution. An

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alternative to the model (1) in my previous contribution that makes detrending of proxies and temperatures explicit is to use the stochastic model

$$T - B_{21}t = B_0 + (P - B_{22}t)B_1 + \varepsilon, \qquad (1')$$

where $B_{21}$ and $B_{22}$ are now trend coefficients of temperatures and proxies, respectively. It is trivial to see that the trend coefficient $B_2$ in my previous model (1) is related to the trend coefficients in the above model (1') by $B_2 = B_{21} - B_{22}B_1$; hence, the stochastic models (1) and (1') are related by a simple transformation of parameters. The authors' claim that my Eq. (1) is wrong therefore is nonsense.

If the authors have a different stochastic model in mind, they must state it. Otherwise, it is again unclear, in their reply as in the paper, to which model and estimation procedure their "degrees of freedom" and other vague justifications refer to.

Detrending is not on the same footing as principal component analysis as a pre-processing step. Detrending amounts to using a different stochastic model. PCA amounts to estimating coefficients in the same stochastic model in a regularized way.

While it is true that the authors do not use detrending in their paper, they discuss it in the paper and in their replies. It also appears to be a point of more general confusion. This prompted my comments about the consistency problems with detrending.

5. BC's reply to this point shows misconceptions about elementary statistical concepts. Missingness at random has nothing to do with how many values are missing. It is not instrumental temperature values that are missing (or this is not the crucial part), but, as pointed out earlier, the historic temperature values that are being reconstructed. These may not be missing at random. Please consult standard statistics textbooks for clarification of these concepts.

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To iterate, the reason I brought this up is that missingness at random is the central necessary condition that must hold in reconstructions of historic temperatures. Stationarity is not central, as BC misleadingly suggest in the corresponding section of their paper. In fact, the time dimension is not modeled at all in their study (a potential area for improvement of currently used paleo-reconstruction methods in general), which means that we can reorder temperature-proxy pairs in any way we want without affecting the analysis. In particular, we can sort the time series, for example, in order of increasing proxy values, such that it will certainly not form a stationary time series, without affecting the regression analyses BC consider.

7. My original point here was that BC's statement that "the informational flow goes strictly from GLB through RSC" [362, 15]" is not correct for iterative methods such as the EM algorithm and its variants. This still stands.

   Consistency between imputed values and estimates of mean values and covariance matrices are not an "option" in the regularized EM algorithm, as BC suggest (and consistency should not be optional in any other method). If they are consistent, reconstructed temperatures and estimates of mean values and covariance matrices are mutually dependent, as I pointed out in my original review.

9. As I pointed out earlier, that other papers may have methodological errors is no justification for the authors' making the same methodological errors, without comment, in their paper.

10. What is under consideration here is BC's paper, not other papers.

Although I favor the greatest possible openness in scientific discourse, the open discussion format in this case does not appear to be conducive to achieving the greatest accuracy and clarity in a scientific publication. It seems to have turned the review process into a public circus in which the authors try to make excuses for erroneous and/or

sloppy work, rather than providing new data, tools, or results, and providing clarifications of their implied stochastic models, assumptions, and inferences.

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