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Climate of the Past Discussions

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Interactive Comment

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

Anonymous Referee #1

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Review of

On the verification of climate reconstructions G. Burger and U. Cubasch

This paper addresses a contentious issue. The NAS/North report has documented that in the past premature claims have been made about the performance of reconstructions methods; the Wegmann-report has shown that those who held the stage in the past years have not always been helpful to allow replicating their results by third parties. Thus, any paper dealing with the various methods and their performance should be welcome, as they help to address the various issues and to guide the community back to more "normal" open-minded discussions.

A commenter of Pielke's Prometheus weblog mentioned in this context: "The worse

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thing climate science and its journals could do would be to begrudgingly acknowledge a few faults and to send the guards back up on the fortress walls - I fear that is exactly what they are doing. And until those walls comes down, global warming skeptics need only ask the public "Many climate researchers say we are undergoing human caused warming, but given their on-going coverups, why should anyone trust them"?"

When looking at the present manuscript, we should keep this in mind; the response of reviewer #2 was obviously an attempt to defend the position taken by her/him or her/his group (as if the North-report would not exist). Instead, the editor and reviewers should adopt the position of Bob Livezey, who was for many years editor of Journal of Climate, namely to help the author to improve the manuscript so that it can eventually be published.

After this prologue, what do I think about this manuscript? I think the main conclusions are valid and useful; eventually the paper should be published - but the paper is not written well; it is simply difficult to understand in technical details, every now and then minor unnecessary re-routes are taken which are likely only understandable by insiders, sometimes uncommon wordings are used (see below).

A major point is the discussion in Sec #2 on stationarity and populations, CE and RE. First, I think this debate is not really required; instead, the authors should simply define RE and CE - and then acknowledge that we are not dealing with "populations" but with stationary processes, which may have very long memory. Means and variances can thus be very different in different periods - and their differences are not "completely random" - whatever that is supposed to mean. Maybe, this is merely a linguistic misunderstanding, but the authors should see to simplify their language and to clarify their message.

Another major point is that the authors should explicitly admit that the estimation of skill during the instrumental period is merely an estimate of the skill in times when no instrumental data are available; that additional assumptions have to be made, which

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can not really be tested (stationarity of link; homogeneity of proxies).

Now, the uncommon formulations:

1 special partitioning into calibration and validation parts - why is it “special”? Is it not just “chosen”?

2. What means “This Ě leaves the regression susceptible to nonsense predictors” ?

3. What means - “The problem is that few degrees of freedom are easily adjusted in a regression” - that a time series with a few degrees of freedom is easily described by a regression?

4. What does this term mean - “Stationarity digression”?

5. Maybe in “Before we explain our testing procedure, we recall one of the most basic estimation principles: that a regression/verification exercise is generally nonsense if calibration and validation samples are not drawn from one and the same population”, the formulation “Ěexercise assumes that c and v series a generated by the same stationary process”?

6. What means “are most abundant”?

7. The term “nonsense” may appear to some readers as inflammatory and should be avoided.

8. The term “flavor” is also uncommon - isn't it merely one of the many combinations possible?

Sometimes the next is overly mathematical without need to do so, e.g. $>>$Let the full, 127-year long P-T record be given as (Pi , Ti , i2I). A “random” calibration set is defined by picking a random permutation _: I!I and letting C(_) = _i 2 I | _ (i) _ n/2 (1) (n being the length of I). This divides the original record into two sets, calibration C(_) and validation V(_)=I $\backslash$C(_), of roughly equal size.$<<$

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The paragraph beginning on p. 359, l. 27 seems to be an unnecessary re-route, as it does not contribute to the core of the paper. Also on page 360, “the two critiques” (l. 11) - which are they?

The definition of RE and CE should be moved from page 364 to the introduction.

The two paragraphs beginning in line 17 on page 365 (until line 30) are an unnecessary re-route.

In the Figure - would it possible to add another case, namely the calibration period covering al early years and the verification period al late years?

I do not know if there is a space limit for this article. But it would be good if the authors would explain in more detail some of concepts entering the manuscript - not all are very familiar with the various lines of arguments introduced so far.

Ton conclude, an interesting paper, should be published, but the authors should work hard to improve the accessibility for the non-specialist.

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