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Climate of the Past Discussions

Interactive comment on "Technical Note: Correcting for signal attenuation from noise: sharpening the focus on past climate" *by* C. M. Ammann et al.

Anonymous Referee #3

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I am late to the table for this review and there have already been insightful comments posted for this manuscript. As such, I am not going to reinvent the wheel and much of my review will simply emphasize some of what has already been said because it bears repeating.

As Ammann et al. point out, the utility of the ACOLS method is to correct for the presence of random noise in the predictors. The discussion provided by the authors of how this comes to pass is actually quite nicely presented, and it is one of the more straightforward theoretical presentations of this issue in the paleoclimate literature of which I am aware. With this as background, however, it is surprising that the authors

test ACOLS in a pseudoproxy context using no noise in their predictors. This issue is critical and Moberg and Zorita have both pointed out the need to evaluate ACOLS in the context of different noise realizations in the pseudoproxy tests using CSM. Moberg correctly points out that Ammann et al. have only tested the impact of 'weather' noise, leaving out from their evaluation the noise associated with the proxy-climate connection - arguably the much larger contribution to noise in typical paleoclimate problems. The authors should be required to at least test ACOLS using the CSM data as they have the synthetic case in Figure 1 using Gaussian noise realizations spanning SNR values of 2 to 0.25. An additional test with autocorrelated noise would also be instructive.

I also am surprized that the OLS method does not yield better results for the CSM experiments, given that perfect pseudoproxies are used. In almost all other pseudoproxy tests the no-noise case (often called the infinite SNR case) does remarkably well at reproducing the NH mean. In some of Ammann's own papers (e.g. Mann et al. 2005) he and his coauthors test multiple CPS and CFR methods that all reproduce the NH mean skillfully in the no-noise trials. It therefore is surprising that the implemented OLS reconstruction is subject to such large errors when free of added noise. I am not sure why this is the case, and the authors should explore an explanation in their discussion. To reiterate, the issue is not whether or not other methods are subject to the biases that Ammann et al. report – they definitely are – but for this specific noise level (i.e. no noise added to the predictor series) OLS appears much more susceptible to biases than any other method tested in the literature.

In the context of the noise discussion, Brohan's points are also particularly cogent. The presentation as it stands gives an incomplete impression of the effectiveness of the method and the authors should take some time to discuss caveats and limitations. Brohan's discussion is spot on and the authors should deal carefully with his points. I simply want to add one additional point of caution. At the end of the day, ACOLS is an intelligent and objective way of applying a variance correction to the regression coefficients based on an estimate of sigma_U. But an accurate estimate of sigma_U is essential and the authors propose deriving this estimate from an OLS regression of W on Y. This of course will be done during the calibration interval and one runs into all of the usual limitations associated with making regression-based estimates over a limited time interval when basic 'good behavior' assumptions are violated. The authors might specifically want to consider how their sigma_U estimates vary as a function of the added noise and the length of the calibration interval. This is particularly a concern in the strongly trending 20th-century period.

A final point highlighting previous online comments is necessary regarding the representation of the literature, as discussed by Christiansen. I think Christiansen's argument is a little narrow, but I would agree that Ammann et al. have not done a particularly good job at characterizing the arc of the variance loss and bias discussions within the literature. It is, for instance, surprising to see the Mann et al. (2007, 2008) papers cited as acknowledging the need for attenuation correction. These papers are part of a series (e.g. Rutherford et al. 2005; Mann et al. 2005) dating back to the Mann et al. (1998) publication that have argued vehemently for the 'low-amplitude' reconstruction originally reported in that paper. While latter studies test new methods, the thrust of the arguments throughout these papers has been that there is likely no variance loss or biases in their reported results. To imply that the need for variance corrections has been advanced by these studies is therefore a rather serious mischaracterization. I also disagree that these problems have only recently been discussed (pg. 1649, ln. 23). Variance losses and biases have been the subject of discussion for at least a decade now in the paleoclimate literature. These issues have various lines of origin. Standardization issues in tree-rings, and the associated potential for low frequency losses, is one such example (only culminating in the broader discussion with the RCS results presented by Esper et al. 2002). Borehole reconstructions have also consistently presented an alternative picture of large-scale temperature change (i.e. larger) since the LIA, beginning with numerous publications in the late 1990's. The authors of course mention the Moberg et al. 2005 result, but not in the context of the attenuation debate as it is originally presented in their paper. With regard to regression

methodologies and their potential for infusing biases and variance losses, Christiansen correctly points out that there are many studies that should be attributed to addressing these problems directly, including various collections of papers by von Storch, Burger, Zorita, Hegerl, Christiansen, Smerdon, Luterbacher, etc. (some are mentioned, some are not). The bottom line is that all of this work could be presented in a more balanced way. The authors should be required to make this adjustment, given that it essential for the context of the problem that they address.

Specific Comments

Pg. 1648, ln. 1-11: This section has already been flagged as problematic. It could be greatly clarified. Some additional confusion here is regarding the difference between minimizing the MSE and just the bias. How do the values of alpha separate out the collective influence of variance and bias on the MSE which can be expressed as Variance + (Bias)ˆ2? It is not at all clear how a given alpha choice allows you to minimize one or the other. Furthermore, small values of alpha effectively minimize the adjustment of sigma_Uˆ2. Why is this advantageous? The authors should also clarify what is meant by "insensitive to values around this choice." Insensitive in what statistical sense and for how large a deviation in the values of alpha?

Pg. 1651, ln. 15-18: The authors point to low-pass filtering as one means of increasing the signal to noise ratio in the predictors. They ignore, however, the fact that this will also greatly reduce the degrees of freedom in the calibration – not necessarily an advantageous step in the regression procedure.

Pg. 1651, ln. 27: The authors argue that variance gains are "mostly concentrated at the interannual scale." There is no reason why this should be so and red noise realizations will undoubtedly inflate lower frequency parts of the reconstruction. The decadal smoothing of reconstructions is therefore no panacea. Furthermore, the annual resolution of these reconstructions is in some cases the principal interest. Smoothing for decadal or lower frequencies therefore removes some of the information that makes annual reconstructions desirable.

Pg. 1652, ln. 14: “Keeping a watchful eye on the variance” is meaningless. What do the authors mean? How would this be done? This statement should be clarified in terms of the results the authors have presented and they should give a means by which to evaluate the differences between bias corrections and variance inflations.

Summary

This is a reasonable technical paper that brings to light a potentially useful means of correcting for biases in the estimated regression coefficients derived for paleoclimate reconstructions. To validate the method more effectively, however, the authors must include some additional tests that include different noise realizations in their pseudo-proxy tests using the CSM data. The authors could also do a better job putting their work in context, both in terms of the statistical work on ACOLS and the understanding of reconstruction limitations in the paleoclimate literature. These issues are of course surmountable and could all be addressed with a solid but major revision of the manuscript.

Interactive comment on Clim. Past Discuss., 5, 1645, 2009.