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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.

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This paper addresses the topical and important issue of bias removal in regression-based proxy reconstructions of climate. It proposes a new method for adjusting regression-based reconstructions, and demonstrates and tests that method on a pseudo-proxy.

The main issue with the paper is that both the derivation of the method, and the pseudo-proxy tests, are done with simplified and idealised forms of contaminating noise. A clear implication is that the methods will work similarly well for real proxy data, where the contaminating noise is more complex; this is not likely to be so.

The regression model considered is Y=B0 + B1*W + E, where W = X+U, and Y=instrumental observations, X=climate signal in the proxy record, and U=contaminating noise (non-climate signal) in the proxy record, E is the residual, and B0 and B1 are constants. The conventional approach to solving this for B0 and B1 is to choose the values that minimise the RMS of E - it is now well established that this often results in biased values of B1: that it mis-represents the true relationship between the proxy and the climate.

Equation 1 is the proposed fix for this bias. It is not clear from the paper under what circumstances this equation is a good approximation, but it is evident that it isn't always appropriate. Consider the case where U=-X/2: this will cause B1 to be about twice as big as it should be, and applying equation 1 will only make things worse - whatever value is chosen for sigma_U. This is obviously a contrived example, but something similar could occur with real proxy data - for instance if a tree grew faster as the temperature increased, but more slowly because of increasing ozone levels, and both temperature and ozone increased over the 20th century.

I suspect that both the method derivation and the pseudo-proxy examples make the assumption that U is a series of independent random variables taken from a normal distribution of constant width. This is a standard assumption in mathematical statistics, and the paper does an admirable job of demonstrating the value of ACOLS where it holds. But for real proxy data this assumption is grossly violated: U will be auto-correlated, correlated with X, non-normal, and sigma_U will vary with time. It's not reasonable to require a calibration method to cope automatically with all these problems (probably no method does), but the value of the proposed method is not how well it behaves in the idealised case, but how well it will do in the real case. It will inevitably do much less well (like all other methods), but by not discussing this problem at all the paper is likely to give a false impression of the power of the method. The paper needs to explicitly state the assumptions made in the derivation of the method, and to explicitly discuss the effect these assumptions will have on real proxy reconstructions (where they are violated).

More minor points.

1) I couldn't tell whether the ACOLS name and method is a new discovery, or an established statistical method new in application to climate reconstruction. It doesn't affect the value of the paper; but if equation 1 and the method for estimating sigma_U are new results then more needs to be said about their derivation; if they are established results then they need to be more clearly referenced.

2) The kernel of the method is the process for estimating sigma_U. I didn't understand the description of how this is done, or get any feel for how well it works and where it might hit problems. This needs to be expanded - in particular the sentence ".. a correction of the form ..., must be made where k>=0 is determined by 5-fold cross validation on the calibration period based on the objective of minimising the prediction bias." left me baffled; and the remainder of that paragraph is not much more comprehensible.

3) More needs to be said about the advantages of ACOLS over other possible methods of debiasing. I wasn't persuaded by the comparison with TLS: TLS isn't particularly difficult to implement, and estimating the two variance components is essentially the same problem as estimating sigma_U, which ACOLS requires.

4) The trade-off between bias and variance: the statement "This variance increase is mostly concentrated at the interannual scale, and thus decadal smoothing of the reconstruction results essentially compensates for this." is misleading. ACOLS scales the reconstruction by a constant factor, so all frequency components of the reconstruction are increased equally - there is no concentration at high frequencies. Often, the noise in the proxy will be blue-shifted compared to the climate signal, and smoothing will improve the signal-to-noise ratio, but it will also degrade the climate signal, and is not always desirable.

Summary:

A superficial reading of the paper would leave the impression that ACOLS is always the best method for proxy calibration, and it removes bias completely for real proxy series. A more balanced discussion of its strengths and limitations is required. But it is an interesting new approach, and potentially a valuable addition to the suite of tools for proxy reconstruction.

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