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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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The paper by Amman et al addresses the important issue of how to correctly estimate the amplitude of the underlying climate signal in a climate reconstruction based on noisy proxy data. Your proposed method of applying an attenuation correction of the slope in the regression Y = b0 + b1 X + e (where Y is instrumental, and X is proxy), to account for the fact that in practice the regression is done on W = X + U, where U is proxy noise, rather than directly on X, is appealing. You demonstrate in Fig. 2 a situation where the ACOLS approach more faithfully, compared to OLS, predicts the 10-year smoothed NH temperatures. Clearly, the ACOLS method does a better job in

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this simple experiment.

The point I would like to make, though, is that in the experiment presented in Fig. 2, the noise is to a large extent represented solely by what we can call 'weather noise', i.e. noise due to the result of synoptic-scale temperature variability at the 12 selected model grid points, which cause the 12 grid-point series to not being perfectly correlated with the target NH-mean temperature. Intuitively, this noise mainly affects the time scales shorter than about 10 years. Consequently, the signal-to-noise ratio (SNR) at each grid point is probably quite large at time scales longer than 10 years. This can explain why the ACOLS method gives not only the correct amplitude, but also a correct time evolution of the true simulated 10-yr-smoothed NH-mean temperature (at the expense of too high variance at shorter time scales, as seen in Figure S1).

So far, no problem. But, in the real world we are dealing with proxy records which can (and do) have much more complex noise structures. We cannot then simply assume that we have a higher SNR at low frequencies compared to high frequencies at the individual site records. If there is much noise at low frequencies in the original proxy series, then I would intuitively guess that ACOLS would result in an artifical inflation not only of the high-frequency noise component in the final reconstruction (as shown in Figure S1), but also inflate the low-frequency component of the noise. In such a situation, one would likely not see the same very tight fit between the smoothed target and reconstruction time series as shown in Fig. 2.

It would therefore be an interesting exercise to make an ensemble of similar experiments as the one shown in Fig. 2, but with a range of SNR-values in the local 'pseudo-proxy' series and with a range of types of noise, e.g. different degrees of red noise as expressed by different AR(1) models. I would hope that this comment could stimulate the authors to undertake a few such experiments and add to the paper. I am aware that a Technical Note cannot be very long, but perhaps a few illuminating cases could be presented as supplementary material, and just briefly commented in the main text.

CPD

5, C349–C351, 2009

I take the opportunity to also ask for a few clarifications:

p.1648, lines 1-3. Please explain more, or give a reference, to how k can be found and what a ’5-fold cross-validation’ means here.

p.1648, line 5. What is n here?

Fig. 2. How did you define the grey error bands for each of the two methods. Are they 95% prediction intervals calculated from the regression calibration? How do you calculate this with the ACOLS method? How is the width of the bands adapted to the 10-yr Gaussian filtering?

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