Clim. Past Discuss., 5, C394–C398, 2009
www.clim-past-discuss.net/5/C394/2009/


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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Received and published: 29 June 2009

Are all climate reconstructions wrong? Well, this manuscript does not imply it, but it is a welcomed warning that reconstruction methods may be more complex than they seem

General comments

To reconstruct statistically past climates based on proxy records that are assumed to contain a climate signal, the proxy record are calibrated against an instrumental variable and are then subsequently applied to reconstruct that target variable over the period covered by the proxy record. I think it is important to realise, as this manuscript



nicely illustrates, that the usual application of statistical reconstruction methods in palaeoclimatology is not correct, as the required underlying statistical assumptions are not met. For instance, when using a linear regression model to reconstruct past temperature (predictand Y) from a temperature-sensitive proxy (predictor X), the estimation of the regression parameter by ordinary-least-squares (OLS) requires that the predictor is noise-free. This is clearly violated most of the times since time variations of proxy records are due to many other processes than temperature. The blind application of OLS in this context leads to an underestimation of the regression parameter, and thus to an underestimation of past climate variations. Although this has been known for many decades, it is a bit troubling that only now we, the paleoclimate community, are generally becoming aware of this caveat. (What are the implications for the interpretation of ice cores, for instance?) Other variants to OLS estimation, such as Total Least Squares require additional information or alternatively have to be embedded in quite convoluted iterative algorithm to estimate that piece of missing information from the data themselves.

The manuscript proposes a correction for this bias in an OLS setting, in the univariate and multivariate cases. This is to be welcomed since it would allow application of the simpler OLS methods. Another positive aspect of the manuscript is that the authors test this correction with synthetic data from a climate model simulation of the past millennium. This type of tests should be by now considered as an inherent part of any paleoclimatological reconstruction. A third reason why I welcome this manuscript is that it makes clear that even a 'simple' statistical method as univariate linear regression can be a mine field. Great care is needed in the application of this and more complex methods in paleoclimate reconstructions.

I have some suggestions that the authors may want to consider. In a short technical note the authors would find difficult to discuss them all in detail, but I think they they should be at least mentioned for the interested reader.

The manuscript seems to take for granted that the standard setting for OLS regression in paleoclimate would be one in which the predictor is the proxy (X) and the target is the temperature (Y). Although this is indeed the more widespread setting in paleoclimatic reconstructions , and the one that leads to the underestimation of the regression slope, it is not the only one possible. I think the authors should be careful with their notation, as this aspect can be the source of much confusion. The classical statistical literature has distinguished several possibilities to estimate OLS regression slope between two variables, in which the regression is used to reconstruct Y when X is known (Isobe et al.,1990): estimate alpha in Y=alpha X + noise; or estimate (1/beta) in X=beta Y+ noise. These two approaches receive different names. In a paleoclimatological context these two approaches are denoted direct and inverse regression, respectively. Unfortunately, in the statistical literature they are known by the terms inverse and classical calibration, respectively (Osborne, 1991). I personally prefer the the terms transfer function and response function for these two approaches, respectively, probably introduced in the chapter by Fritts et al. (1990) . Interestingly about 40 years ago the statistical community was engaged in a lively debate about which of these two methods, which in general yield different results, is better (Krutchkoff,1969). My take-home-message from that debate is that inverse calibration is better when the predictand does not leave the range of the calibration period, whereas classical calibration is better in a situation when one has to extrapolate.

The present manuscript exclusively deals with direct regression (transfer function or inverse calibration) and this should be stated explicitly to avoid confusion. Also, a bit of effort should be invested in making the notation as clear as possible, for instance by stating which variable would represent the proxy (X) and which the target climate variable (Y). It should be also mentioned that the alternative approach (response function or classical calibration) is theoretically also free of the underestimation bias (at least in the univariate case) and also burden by wider uncertainty bounds, in a similar way as ACOLS.

My second suggestion is related to the test of the method with model data. As fas



as I understood, this test is performed in the manuscript with perfect pseudo-proxies, i.e. the grid-cell model data are used as predictors to reconstruct the Northern Hemisphere mean temperature. This is a very idealized situation. Real proxies generally display correlations with their respective local temperature at interannual time scales of about 0.5 or less, not unity. In a better test the model data should be degraded with random noise to achieve realistic correlations with the grid-cell temperature. Moberg's suggestion, in his comment on this manuscript, to use white and red noise for this contamination should be also heeded. Although both methods, ACOLS and uncorrected OLS, are tested with the same data, it could happen that in a more realistic situation their difference is not as large as when tested with perfect pseudo-proxies. Other aspect is that the range of the Northern Hemisphere temperature simulated by the CSM model over the past millennium is not entirely contained in the range of the calibration period. It is not known if this is realistic, so perhaps both methods should be also tested in a control simulation as well.

A third suggestion would be to report the values of calibration statistics with and without the ACOLS correction, for instance the usual measures of skill in paleoclimatology Reduction of Error and Coefficient of Efficiency. I guess that RE and CE will be worse for ACOLS than for the uncorrected OLS calibration. This is important because if this happens, calibration RE and CE would not be trustworthy measures of the skill of a reconstruction method.

Particular comments

Abstract, line 19: ACOLS leads to an increased variance of the reconstruction. This would not be per se a negative property. I think the authors mean that an 'inflated' regression slope would lead to wider uncertainty bounds

Page 1646, line 23: 'uncertainty in Y' I would say a better expression is uncertainty in the estimation of Y

Figure 2. It seems that the vertical thin line separates the calibration period. Is this so?

Page 1648, line 24: 'In the simple linear case...'

Acknowledgement:Many of these statistical questions are being currently discussed within the European project Millennium (www.millenniumproject.net) , from which I have strongly benefited.

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