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Climate of the Past
Discussions

Interactive comment on "On reconstruction of time series in climatology" *by* V. Privalsky and A. Gluhovsky

Anonymous Referee #2

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There are many situations in climate science where one wants to make inferences about one time-series (or field), x_1, from another time-series (or field), x_2. Usually there is a period of overlap between the two time-series and this period is used to train a model which is then used to "predict" x_1 in a period where only x_2 exists.

One area of research where such efforts are important is reconstruction of past climate from proxies such as tree-rings, sediments, historical records etc. There is a large literature about climate reconstructions and the different methods.

The present paper suggests that multi-variate AR models should be used for reconstructions and argues that such methods are better than the regression based methods usually used. The reconstruction problem is important and I would welcome any efforts to study the different methodologies and suggest new ones. However, the present

paper has serious shortcomings that prevents me from suggesting that the paper is accepted in its present form.

Major comments:

1) I don't see the arguments for preferring Eq. 2 instead of Eq. 1. There must be many cases where the x_2 does respond to x_1 at the same time and not just trough the lagged terms. If x_1 and x_2 are annual values of temperature and tree-rings then why should tree-rings depend on temperatures the previous years and not the same year? If serial correlations are important they could be included in Eq. 1, e.g., by letting the noise term be an AR-k process.

2) The paper insists that the new method is the proper method. I fail to understand that. In the example given the new method gives a better fit than the simple regression. However, this should be expected as the number of parameter is larger and as the models are tested on the same data as they are trained on. A fair way to test the methods and avoid over-fitting would be to test on independent data, e.g., using some kind of cross-validation.

3) There is actually a lot of relevant discussion about the reconstruction methodologies in the literature that the authors does not mention. These studies include Lee et al., Clim. Dyn., 2008; Christiansen et al., JCLIM, 2009; Smerdon et al., JCLIM, 2011; Christiansen, JCLIM, 2011; Evans et al., GRL, 2014, and many others. Many of those studies use climate model output to test the methods - the so-called pseudo-proxy methods (Smerdon, WIREs Clim. Change, 2012). Perhaps the authors should consider using this approach to compare the new method to older ones.

4) The example with the TSI and SSN seems strange. The strong periodicity would suggest a model with an external forcing. Also the improvement when using the new model seems modest (in particular given the possible over-fitting). There must be a better example.

5) There is too much jargon as the other reviewer mentioned. Is the historical part and the part about the information rate really relevant? Why not simply say that the connection between x_1 and x_2 may depend on the frequency and that the simple regression, Eq. 1, may therefore not be sufficient?

Minor comments:

Eq. 1 is not complete without information about whether the noise and x_1 or the noise x_2 are independent.

Section 4.1: If model orders of 32 or 33 are necessary then the model is probably not the correct one. Perhaps an ARMA model would be better.

Eq. 12: Perhaps the coefficients could be moved to a table and shown together with their uncertainties.

Caption to Fig. 6 and the text near bottom of page 4715 disagree on the period.

Second point on page 4712: It is not clear what is referred to here. The problem with serial correlations reducing the number of degrees of freedom? Multiple regression?

Interactive comment on Clim. Past Discuss., 11, 4701, 2015.