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Interactive comment on "Inferring climate variability from nonlinear proxies: application to paleo-ENSO studies" *by* J. Emile-Geay and M. P. Tingley

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I enjoyed reading the manuscript, both for its educational content and for the pragmatic approach that the authors have taken to, en the end, offer a useful advice to the practitioners of climate reconstructions. The manuscript is sometimes dense but it is well written and can be readily followed, as the authors have taken care of not getting very much entangled in theoretical formalism that may had put off some readers. I am happy to recommend its publication in Climate of the Past, although I have some minor suggestions that could be addressed in a slightly revised version.

Your constructive feedback is much appreciated.

1) I was a bit irritated by the equivalence that the authors assume between non-linear link between climate and proxy and a non-gaussian distribution of the proxy record. These two concepts are only equivalent when the climate record is itself normally distributed. The authors more or less explicitly acknowledge this caveat in the text, but this caveat is some what hidden and appears a bit too late to my taste. Later, this conditionality - that the climate record has to be normally distributed- is just assumed. Whereas this might be true in most situation, I guess that some readers may get initially confused. Also, it might be not true in for some climate records. In those cases, the proxy record should be transformed to the same distribution as the climate record, and not to a gaussian distribution. I think the whole argument would gain clarity if this were explicit stated some where in the manuscript, better sooner than later.

We apologize for the irritation. In various versions of this manuscript we tried to make that point clear, but apparently failed. Here are relevant quotes from the revised version:

In the abstract we now state: "The results hold implications for how **univariate, nonlinear recorders of normally distributed climate variables** are interpreted, compared to other proxy records, and incorporated into multiproxy reconstructions. " (L18-20). This should make the scope clear from the outset.

"These proxies can feature statistical distributions that are different from that of the climate phenomenon they purport to record, and different from one another" (L38-39) Makes it clear that the effects of nonlinearity are not limited to normality.

L44-54 explain in great detail in which sense nonlinearity has non-normal effects. In particular, "Even when the target climate quantity is well approximated by a normal distribution, nonlinearities often manifest themselves as non-normality in the proxy distribution. This is because a normally- distributed proxy is only expected if the proxy is a linear recorder of a normally-distributed climate variable, as the linear transform of a Gaussian random vector is also Gaussian". This should make it clear than in any other case one would not expect normality.

Finally, the introduction closes with: "This article draws attention to the pitfalls of ignoring nonlinearity in the proxy-climate relationship, and explores a number of approaches to using nonlinear proxies to infer climate variability when the underlying climate obeys normal statistics." (L68-70)

Again, this makes it clear that we are focusing on a specific type of effect, namely how nonlinearity affects the inference of a normally-distributed climate variable (surface temperature), but that this is not the only possible effect.

2) The assumed non-linear relationship describes only some type of non-linearity. The title is thus a bit too general, as some researchers from the tree-ring community and maybe from other communities, could assume that the meant non-linearity would be of the non-invertible type , e.g. a value of tree-ring-width corresponding to two possible values of temperature striding the temperature of optimal growth. The manuscript clearly does not deal with this type of non-linearity.

This effect is indeed important, but we like to think of it as distinct from nonlinearity. It is non-uniqueness stemming from multivariate controls on proxy systems. To make it clear that this is not the focus of the paper, the introduction states: "Here we focus

on the related challenge of inferring climate variability from proxy archives that are nonlinear, univariate recorders of the target climate variable. " (L74-75).

However, the reviewer is correct that the power-law type of nonlinearity is a fairly restricted kind. We say as much in the discussion's closing, and also mention the multivariate non-uniqueness problem:

"Finally, while this article has focused on nonlinear proxy records with power-law type relation- ships, it is worth pointing out that a number of other valuable climate proxies may deviate from linearity in other ways. In particular, records based on proportions (e.g. pollen counts, lithological fractions, fractions of certain faunal assemblages), being in the range [0, 1], also involve a nonlinear transform of Gaussian inputs like temperature, and therefore require specific inference tools. Another key factor complicating inference from climate proxies is the existence of multiple influences on the measured variable, e.g. temperature and soil moisture controls on tree-ring width (e.g. Anchukaitis 490 et al., 2006; Vaganov et al., 2006; Tolwinski-Ward et al., 2011; Evans et al., 2014) or temperature and seawater composition controls on the oxygen isotopic composition of biocarbonates (e.g. Thompson et al., 2011; Russon et al., 2013; Dee et al., 2015). We will explore solutions to these problems in future work." (L483-494)

3) The authors are candid when discussing the apparent superiority of the Bayesian approach, as it the study assumes the exact knowledge of the data generating process and even of the values of the model parameters. I was wondering how the results would look like if the value of the non-linear exponent β in the data generating process were slightly different from the assume value, or in other words, how the uncertainty in β would influence the skill of the Bayesian approach. I guess that in real situations, the value of β will have to be estimated, as the authors also recognize and this may *require computing intensive sampling methods, but maybe the authors could conduct more simple calculations in which $\beta_{\mathrm{generatingmodel}}$ and β_{bayes} are slightly different.*

We considered this idea in earlier versions of this work. While it is indeed possible, the main point of the Bayesian example is to show an oracle-type scenario, one that is impossible to achieve in practice. The fact that the ITS method does nearly as well as this idealised case is, we believe, a strong rationale in its favor.

4) The authors seemed to have followed the American spelling, at least I could spot a few ' modelings'

We have tried to address this in the new version, but please point out American spelling heresies if we inadvertently left some behind.

5) The following are examples in which a non-gaussian distribution is equated to non-linear proxy without any caveats: 2006; Tolwinski-Ward et al., 2011), karst effects in speleothem ? O records (Baker et al., 2012; Jex et al., 2013), and hydrodynamic effects in flood proxies. Nonlinearities are especially pronounced in terrestrial proxy records from the tropics, where tempera- ture experiences its lowest dynamic range and precipitation its highest dynamic range, resulting in distributions that are non-normal, with strong positive skew. These records Nonlinearities often manifest themselves as non-normality in the proxy distribution, de- spite the target climate quantity being well approximated by a normal distribution.

These should now be addressed. Please let us know if not.

Interactive comment on Clim. Past Discuss., 11, 2763, 2015.