Comments on

"Technical Note: Multi-centennial scale analysis and synthesis of an ensemble mean response of ENSO to solar and volcanic forcings"

by J. Sánchez-Sesma

Recommendation: accept with major revisions

The manuscript applies a novel methodology (the curve mass method) to the analysis of NINO3 timeseries simulated with an intermediate complexity model (the Zebiak-Cane model, hereafter ZC) forced by best estimates of volcanic and solar perturbations during the past millennium (Mann et al., 2005). Without clearly explaining the choice of parameters, the author obtains a decomposition that describes a large fraction of the variance of the timeseries. This statistical model is then used to make 21st forecasts, which raises a number of issues that would need to be addressed before consideration for publication.

General Comments:

1. **An unclear goal.** Isn't nice to read papers that ask a question and attempt to answer it? In this case, one is hard pressed to decipher the author's intention. The ZC model has been used in the context of the past millennium to provide a theoretical basis to the idea of a La Niña-like Medieval Climate Anomaly (i.e. lower than average NINO3) vs an El Niño-like Little Ice Age (i.e. higher than average NINO3). Never can it be used for prediction more than 2 years ahead (Chen et al 2004), especially by leaving out anthropogenic forcings. So what are we after here? The author should make it more clear than he is studying the ZC model, not ENSO, and ask clear questions about the model while explaining why his analysis would be able to provide new insight into those questions.
2. **Arbitrary Methodology**: The author's method allows him to find oscillations in the ZC model output, but as his well known to any timeseries analyst, one can find almost any periodicity provided that they decide on a judicious choice of parameters. How sensitive are the estimated parameters to the assumptions? How much does this change the results? It seems that for such a simple calculation a sensitivity analysis would be a bare minimum. Further, what is the physical interpretation of the curve mass method, and why would it reveal features that MTM spectral analysis, singular spectrum analysis, wavelet analysis, detrended fluctuation analysis, or other standard method would fail to reveal? What justifies the very strong assumption: "the transformed variable can be decomposed in two components of linear and non-linear oscillations (low and high frequency, respectively)"? Why is "multi-linear regression" used in lieu of a Discrete Fourier Transform to find the FS component?

3. **Model Interpretation**. The manuscript comprises a number of inaccuracies and errors of interpretation about the ZC model. Specifically, it is said that the model is "computationally able to evaluate ensembles of multiple realizations of millennial forcing scenarios that intrinsically arise from tropical Pacific climate mechanisms", which suggests that each ensemble member corresponds to a distinct forcing realization. This is not the case: each one corresponds to a different initial condition (assigned randomly) with identical forcing. More disturbing is the claim that the model has been "validated" (p 2062): while the ZC model is known to offer a low-order paradigm of ENSO variability, its ensemble-averaged output cannot be expected to reproduce the instrumental record, partly because it is known that the forcing is no longer "natural" (greenhouse gases and anthropogenic aerosols are part of it), and partly because the initial conditions of the observed record are unknown. The only comparison that would make sense post 1850 is in terms of the low-order statistics (mean, variance, spectral density, skewness) - not the phase of the oscillations. More disturbing even is the use of the timeseries analysis method to predict future model behavior, while it would be trivial to just run the model forward to obtain an exact prediction (in a "perfect model" sense). Of course, the model is far from perfect, and without providing it with realistic scenarios of climate forcing until 2130 AD, one cannot expect it to meaningfully predict future ENSO behavior.

4. **Explaining periodicities**. I can hardly imagine a more senseless pursuit of science than using hardly motivated methodologies to hunt for periodic signals, find some, and then play the period-matching game to "explain" said periodicities. The entire point of a dynamical model is to enable the testing of hypotheses on the causes of observed variability. I recommend that the author use the same analyzing method to decompose the forcing, and feed this to the ZC model so that he can evaluate which portion of the forcing accounts for which portion of the output. Otherwise, period-matching is just as bad as wiggle-matching, and surely no one needs a technical note to get clear on that pervasive paleoclimatological practice.

Specific Comments:

- p2056: "thousands of millions" is usually called "billions"

- p2057: "moving average of 13 months" is at odds with the figure caption citing 21 months.

- p2059 " j is an index component term". Hopefully all readers of CP are familiar with the notion of a summation index! I would leave this out. Note, a and b are no "constants" but the sequences of Fourier coefficients.

- p2060 Although I cannot blame the author for using an all-too-standard terminology, I really wish our field quit implying that timeseries "explain" each other's variability, instead of simply "describing" it. Components 1 and 2 don't actually give any causal explanation, but do describe a large fraction of the variance in simulated NINO3.

- p2061: the author improperly describes the results of Emile-Geay et al 2007, citing a 2008 article. Perhaps it is because the latter (cited below) is actually more relevant to ENSO over the past millennium?

- p2061: the Gleissberg (not Glaissberg) cycle is generally thought to occur at periods closer to 88 years (Peristykh & Damon, 2003).

- p2062: “non-linear with a self-similarity behavior” : non-linear in what variable?

In summary, I recommend publication once all the aforementioned issues have been addressed.

References:

D. Chen, M. A. Cane, A. Kaplan, S. E. Zebiak, and D. J. Huang. Predictability of El Ni ˜ no over the past 148 years. Nature, 428(6984):733–736, 2004.

J. Emile-Geay, R. Seager, M. Cane, E. Cook, and G. H. Haug. Volcanoes and ENSO over the Past Millennium. J. Clim., 21:3134–3148, 2008.

M. E. Mann, M. A. Cane, S. E. Zebiak, and A. Clement. Volcanic and solar forcing of the tropical Pacific over the past 1000 years. J. Climate, 18(3):447–456, 2005.

A. N. Peristykh and P. E. Damon. Persistence of the Gleissberg 88-year solar cycle over the last 12,000 years: Evidence from cosmogenic isotopes. J. Geophys. Res. Space Phys., 108:1–1, Jan. 2003.