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Earth System Dynamics Discussions

# *Interactive comment on* "Scaling regimes and linear and nonlinear responses of last millennium climate models to volcanic and solar forcings" *by* S. Lovejoy and C. A. Varotsos

**Anonymous Referee #3**

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The authors analysis output from millennium experiments with the Zebiac-Cane model and the GISS model. They conclude that both models underestimate variability at centennial scaled compared to observations, and also observe a phenomenon of 'sub-additivity' in the ZC model.

(1) One of the surprising findings featured in this article is the 'subadditivity' of the Zebiac-Cane model. When it is forced by both solar and volcanic forcings, the ZC model has a spectrum response close to the simulations with volcanic forcing only, as if the solar forcing had been ignored. The seasoned modeller would be tempted to attribute the result to a trivial mistake in the experiment design. Assuming that chances



of mistakes have beend checked and eliminated, we need to find an exlpanation to this result and discuss wisely its implications for our understanding of climate dynamics.

We remember that the ZC model was developed specifically to study tropical Pacific interannual variability, and in particular the ENSO phenomenon. It does not have deep ocean dynamics, nor extratropical atmospheric dynamics, which are two processes which may significantly interplay with interdecadal variability. Lacking ocean modes of motions active at times scales over a few years, the use of the ZC model in a study focused on long-memory processes and non-linearity at time scales of several hundreds of years is highly contentious.

The inadequacy of the ZC model for spectral analysis at scales over decades is a case for rebuttal of the article.

(2) Confirmation of a 'scaling' regime should require more than drawing a straight line on a log-log plot. If a frequentist approach is chosen, what are the statistical tests and how would one devise a p-value ? If a (somewhat preferable) model selection approach is chosen, what are the competing models and which evidence to we have that a 'scaling' model wins over a more usual Markovian model, e.g. an ARMA ?

(3) Besides this major comments, the form of the article presents several drawbacks as well.

There would be a need to :

(a) improve the accuracy of the abstract, in particular by distinguishing results that are specific to ZC from those that are valid for both models. For example, the material present in the article makes no case for subadditivity in the GISS model.

(b) reduce introductary material (and there will be room to focus on the physics of the response)

(c) use figure titles, improve the care of axis drawing (red marks on Figu. 2 (a) are ugly; why not use a standard logarithmic scale straight away ?) Figure 6 in particular:

legend text should be reduced as much as possible, but the figures should have titles and be more self-explanatory.

(d) too much recourse to parentheses. The text must be improved for smoother reading.

(e) avoid expressions such as "kind of bootstrap", "kind of central limit theorem" that give the air of approximate discourse.

(f) clarify the distinction between sampling uncertainty and stochastic uncertainty. It is elusive if the system is stationary and ergodic. The phrase "stochastic uncertainty" is in fact strange enough since any uncertainty can be expressed with stochastic quantities. By contrast, there is no mention of the structural uncertainty associated with model misspecification.

(g) clarify what "satisfy an estimate" (p. 1839) means.

(h) check the maths. Delta T mistakenly written for "T" in several formulae pp. 1825 - 1826

(i) revise p. 1838 around lines 10-15 (missing verb, and difficult to follow anyway)

Finally (j) regarding 10Be data : H is claimed to be around 1 over the scale range 1-50 years but this is not seen on the Figure

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