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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

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GENERAL COMMENTS

Results, their relevance, and their validity

The results presented in this discussion paper are limited to assessing the linearity/non-linearity of the temperature response in two climate models, one model of intermediate complexity for the tropical Pacific (designed to describe ENSO), and one AOGCM, where the authors have confined themselves to studying results for mean northern-hemisphere land temperatures. The motivation for choice of models is not carefully explained, and their representativeness is not discussed.

The linearity issue is investigated by two methods:

(i) By considering solar, volcanic, and solar + volcanic forcing, and testing the additivity of the responses.

(ii) By testing the intermittency of the forcing and responses, assuming that in a linear system the intermittency in forcing and response should be the same.

By method (i) it is found that solar and volcanic responses in the models do not add up on time scales in the range 300-1000 yr. The result is based on neglecting the estimated correllation between responses to solar and volcanic forcing, respectively (section 3.4 and Fig. 3). This approximation is justified from the the statistical independence of solar and volcanic forcing. But in the model experiments these forcings are given as deterministic and do not vary over the statistical ensemble, so the estimated ensemble average over the product of these forcings is not zero. This approximation is unnecessary and may be the cause of the non-additivity result. If the authors believe it is not, they should estimate the Haar fluctuation of the sum of solar and volcanic responses directly, without using this approximation, and demonstrate that it does not change their result.

Method (ii) is based on the theoretical fact that if the response is linear, the response kernel is a perfect power-law function, and the forcing is perfectly multiscaling, then the intermittency is the same for for response and forcing. If the intermittencies are different the authors take it as a proof of nonlinearity of the response. However, there are at least two differents tests that need to be done before one can draw this conclusion:

(a) Theoretical and estimated scaling is not the same. In order to test that the estimated intermittency is the same for the actual forcing and the response from a linear power-law response model, the authors should use such a model and apply the trace-moment analysis to the response computed using this model. If the trace moments are the same as for the forcing, they can proceed to the next step.

(b) In this step they should question their assumption of perfect power-law scaling of the linear response. It is well known that there must be a cut-off of this response at large time scales (Rypdal and Rypdal, 2014). A cut-off at scales from a few decades to a century can easily explain the difference in intermittency. The authors should test if introduction of such a cut-off (or use of other plausible response kernels) will change the trace moments in the linear model and make them more similar to the trace moments of the actual temperature signal.

Structure and style

The paper has the form of a broad review of work by Lovejoy and co-workers, spanning most of the 16 self-citations. Most of this material is irrelevant for the interpretation of the results developed in the present paper. There is hardly a need for another review of dr. Lovejoy's work in his field in addition to the monograph Lovejoy and Schertzer, 2013. In this review I restrict myself to those aspects that are relevant for the new results presented. It does not mean that I approve of everything that is not commented.

General judgement

The manuscript is not suitable as a research article in ESD in its present form. My reservations described in points (i) and (ii) above have to be addressed and proven wrong, and a drastic shortening of the manuscript is necessary. The authors should adhere to the principles for a regular research article.

SPECIFIC COMMENTS

Section 1

Page 1822, lines 1-3. The comment of Blender and Fraedrich (2004) to Vyushin et al. (2004) is mentioned without discussion of its relevance. This comment discusses earlier highly relevant papers on AOGCMs.

Section 2

Page 1823, lines 17-20. Here it is stated that the "ultimate goal of weather and climate modelling is to achieve Tsim(t)=Tobs(t)." This may be true for weather modelling within the predictability limit of about 10 days, but for weather beyond this time horizon and for climate prediction there is an inherent chaotic and unpredictable component (internal variability). It is not an "ultimate goal" to eliminate uncertainty that cannot be eliminated. A similar conceptual oddity is committed in Section 1, page 1820, line 16-23, where the authors end up stating that statistical agreement is not a sufficient condition for model validation, i.e., it is not sufficient that the model realizations and reality are shown to be independent realizations of the same stochastic process. The authors should choose their words more carefully, since this kind of reasoning is what forms the basis of the claims of a certain group of climate change deniers who contend that GCMs are wrong because neither individual model realizations nor model ensemble means correspond to an individual observation.

Section 3.1

The long passage on Page 1825, line 24 –page 1826, line 8 is very obscure, and the notation is a mess. What does expressions like $<T(t+\Delta t)T(\Delta t)>$ mean? My guess is that $T(\Delta t)$ is a temperature fluctuation on scale Δt. But in what sense? Moving average? Haar fluctuation? What does then $T(t+\Delta t)$ mean? Again my guess is that the correct notation is to write $T(t;\Delta t)$ and $T(t+\Delta t; \Delta t)$. But if this is the Haar fluctuation, what does then the difference $\Delta T(\Delta t)= T(t+\Delta t)- T(\Delta t)$ mean? The Haar fluctuation is already a difference, so this is then a difference of differences? And what is the relevance of writing up the expression for the variance of $\Delta T(\Delta t)$?

The last sentence, "...fluctuations at scale Δt are no longer determined by frequencies $1/\Delta t$ but rather by irrelevant low frequency detail of the empirical sample," seems wrong. Isn't it the other way around? I think the entire passage could be replaced by the sentence: For $H<0$ the high-frequency details dominate the differences and prevent these differences to decrease with increasing scale Δt.

Section 3.2

The discussion of statistical uncertainty in long-range dependent processes concludes that an explicit stochastic model is needed to obtain numerical realisations (Monte Carlo simulations). I agree with that. But then the authors write: "However, it is not the aim of this paper and thus it has not been done here." This is a very strange statement since the Haar fluctuation is plotted in five out of six figures for all scales up to the length of the data record. For the longest scales the statistical uncertainty is huge, and cannot be ignored "because it is not the aim of the paper." I haven't found a decent discussion of this uncertainty in any of Lovejoy's papers, so this point could be relevant to treat here.

The authors then proceed to a lengthy description of what they call "stochastic uncertainty." From their description I cannot find any difference between the statistical uncertainty that requires stochastic models and this stochastic uncertainty. This should be clarified.

Section 3.4

In the text the authors again do not define the meaning of $\Delta T(\Delta t)$, but in the caption of Fig. 3 it is written that that one is plotting the RMS Haar fluctuation, so as a working hypothesis I assume that $\Delta T(\Delta t)$ is the Haar fluctuation on scale Δt, at time t. On line Page 1834, line 18 it is assumed that $<\Delta Ts(\Delta t)\ \Delta Tv(\Delta t)>=0$, justified by the independence of the solar and volcanic forcing. The first thing is that I don't understand why it is necessary to make this approximation at all. Why not compute the RMS of the Haar fluctuation given in Eq. (4) directly? The second is that the neglect of $<\Delta Ts(\Delta t)\ \Delta Tv(\Delta t)>$ is highly questionable. The symbol $<\ldots>$ denotes ensemble average over 100 realisations of the ZC model. The responses $\Delta Ts(\Delta t)$ and $\Delta Tv(\Delta t)>$ are strongly correlated with the forcings Fs and Fv respectively. But in the simulations these forces are deterministic, i.e., they are the same in all realisations in the ensemble. So even if it were true that the deterministic component of the reponses we proportional to the

forcings, such that $<\Delta Ts(\Delta t)\ \Delta Tv(\Delta t)>\sim Fs(\Delta t)\ Fv(\Delta t)$, the product of the forcings is not zero. As mentioned in the general comments, the authors must find a way to demonstrate the validity of this approximation, or estimate the Haar fluctuation of the sum of solar and volcanic responses directly. The latter is just as easy computationally.

Page 1836, lines 16-18. Here the authors comment on Fig. 4, and write: "Since the ZC model (including volcanic forcing) has nearly the same statistics (as GISS), we may conclude that the combined solar and volcanic forcing is also quite weak." I don't understand this conclusion. The forcing is given in Fig. 1. We know what it is. Maybe the authors mean the combined response? But weak compared to what? We are comparing apples and oranges; an intermediate complexity model for ENSO (tropical Pacific) with a GCM result for northern hemisphere land. I don't get the message from this figure.

Page 1836, lines 20-24 and Fig 4. The slope of the fluctuation function of the control run is supposed to imply something about "the convergence of the control to the model climate." I don't understand. What convergence? What is the "model climate?"

Fig. 4. The multiproxies have high fluctuations on scales < 100 yr. This is associated with the warming since the little ice age (LIA), and contains an anthropogenic contribution. The last millennium GISS E-2-R simulation apparently exhibits a weak LIA, but his is not the case with several other AOGCM experiments over the last millennium (Østvand et al., 2014). How representative is the GISS E-2-R?

TECHNICAL CORRECTIONS

Page 1852, line 17. The reference to Vyshin et al. is is incomplete.

Figure 1. Panel b and c, use yr BP on the horisontal axis rather than date.

Figure 2b. Top and bottom are inconsistent between figure and caption.

Figure 3 (a). The curves for multiproxies are missing. Caption, lines 3 and 4. Fig. 2b and 2b should be 3a and 3b.

Figure 6 should be divided into several figures. The lines between the red points should be red, or at leat a different color from the regression lines. Why are the regression lines wiggly? (a), (b), (c) are used to label panels and also to enumerate a list in the caption. Very confusing.

Interactive comment on Earth Syst. Dynam. Discuss., 6, 1815, 2015.