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Interactive comment on "Identifying global patterns of stochasticity and nonlinearity in the Earth System" *by* F. Arizmendi et al.

Anonymous Referee #2

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I reviewed an earlier version of the present manuscript for the journal Physical Review E. For reference, the two reports are attached below. Most of the criticisms raised in those reports still hold and need to be taken into account seriously in a possible major revision of the paper:

* An ad-hoc measure for nonlinear relationships between time series is used, ignoring well established measures that are readily available in the literature. The present results should be compared to those obtained from well-established measures such as mutual information.

* The motivation and aim of the study is not clear: the paper sets of in the introduction with discussing climate networks in the first paragraph. But the rest of the paper does not have anything to do with climate networks.

* The relationsships between the ad-hoc measure for nonlinear relationships and entropy are discussed only vaguely without supporting statistical analysis. Scatter plots between both quantities would help to support the claims made in the text. Further analysis should follow to substantiate them.

What has improved to some degree in my opinion is the discussion of the physical mechanisms possibly underlying the observed patterns.

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First review for Physical Review E: ——————————————

In their article, Arizmendi et al. present their results obtained by applying methods of uni- and bivariate time series analysis to climatological data. Their focus is on quantifying the nonlinearity of the relationship between solar forcing and surface air temperature (SAT) time series as well as the stochasticity of SAT time series. For measuring stochasticity, the well established Shannon entropy is computed based on the time series' empirically estimated probability density function. For measuring the "nonlinearity" of the relationship between two time series, the authors propose a "nonlinear distance" measure (Eq. 1) that only vanishes if the (phase-shifted) time series are identical. Hence, all non-identical (modulo phase shifts) pairs of time series are effectively counted as nonlinearly related. Even for linearly related time series $y(t)= ax(t) + b$, the "nonlinear distance" would be non-zero. For these reasons, I regard this measure as clearly flawed, putting into question some of the main conclusions drawn in the paper concerning "nonlinearity".

The paper contains only minor (if any) methodological advancements and its scope is limited to the field of climatology. The methodological approach and results are not put into a wider context in the paper. This lack of general scope and wider applicability puts into question the paper's relevance for the wider physics readership of PRE. However, pending methodological improvements (see below), the paper contains some results that might be of interest to a climate science audience. Hence, I do not regard the

paper to be suitable for publication in PRE, but suggest that it could be resubmitted to a more specialised climatological journal.

Further comments:

* Regarding the quantification of nonlinear relations between time series, more established and theoretically well-founded methods should be applied, e.g., linear and nonlinear regressions, information-theoretic or recurrence-based approaches etc.

* Starting in Section III, similarities between spatial patterns (maps of scalar measures defined on grid points such as entropy) are discussed a lot in a purely qualitative way. Here it would be advisable to use quantitative means for comparing such patterns (e.g., pattern correlation, scatter plots, mutual information, etc.) to foster a more substantial discussion and conclusions on the relationships between measures and data sets.

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Second review of revised manuscript for Physical Review E: ————————————————————————————

I appreciate the effort invested by the authors in revising their paper. However, I did not find their response to the reviewers' comments persuasive. I still have two main criticisms:

1. The authors introduce an ad-hoc measure of nonlinear relationship between time series, but neither a comparison to established methods for this purpose such as mutual information (or the plethora of other measures in the literature, e.g., Reshef et al., 2011) is given nor is a good motivation for choosing this particular measure other than that it is "simple". Putting the method into context in this way is essential and cannot be considered to be "out of scope" for the paper, particularly since PRE offers sufficient page space to report on such extensive studies.

2. I miss a convincing and well-founded discussion of the physical mechanisms underlying the observed results. The paper could potentially profit much from a detailed

review by an expert in climate physics.

References:

Reshef, D. N., Reshef, Y. A., Finucane, H. K., Grossman, S. R., McVean, G., Turn-baugh, P. J., ... & Sabeti, P. C. (2011). Detecting novel associations in large data sets. science, 334(6062), 1518-1524.