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Earth System Dynamics Discussions

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

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First of all I would like to thank the authors and all the reviewers for their contributions to Earth System Dynamics Discussions.

We have three referee reports. The authors have replied to the referees and went a bit ahead of the process by providing a revised version. It is however probably premature to send this revised version to the editors for reasons that I will outline below.

First of all let us summarise what are the possible good points and potential issues with this manuscript.

The article considers (local) incoming insolation and reanalyses surface air temperature (SAT) over the last 60 and 40 years (two datasets are used) to establish the degree

of 'linearity' and 'stochasticity' in the surface air temperature response to insolation. The description of 'linearity' and 'stochasticity' relies on measures: a lag-distance and the Shannon entropy of the distribution respectively. The reviewers expressed concerns about the relevance of these indicators.

1. the lagged distance will be non-zero for general forms of linear convolutions. This is a serious objection in principle, and the authors may not have fully acknowledged the point in their revised version since the sentence "if the response is perfectly linear [...] $d_i \approx 0$" remains. However, the smooth, harmonic character of the insolation forcing is such that this might be less of a concern in the end and it must also be noted that the authors provided, in their response, further sensitivity tests using a correlation distance metric.
2. the Shannon entropy has some counter-intuitive behaviours, as for example, a decrease in entropy in presence of extremes. The authors indeed analysed and explained why entropy decreases in their response, but the wider concern as whether entropy is indeed a suitable measure of "stochasticity" remains there. I believe that the authors could be more clear about what is meant here by "stochasticity" here. The very notion of stochasticity is attached to a modelling framework ("someone's noise is someone else's signal") Intuitively, we would be probably see it as a departure from a smooth response to insolation (even though smoothness and predictability are in principle not the same thing) and more arguments must be provided about why entropy could be a good measure of such well-defined stochasticity. Exploring alternative measures of such stochasticity would provide added value to the study.

Reviewer #2 is quite concerned about the overall added value of the paper. I share this concern. We probably need to admit that the physical message is not overwhelming (smaller lag over the continent; non-linearity in the monsoon regions: all this is pretty expected). Where non-trivial interpretation are mentioned (e.g.: a role for stratus clouds) they are not supported by a full investigation of relevant diagnostics. On the other hand, the methodological implications for climate network theory remain rather oblique. Consider in particular the last two paragraphs of the revised version:

“the entropy analysis also allowed to identify, in a well-defined region of the tropical western Pacific, a remarkable difference between the ERA and the NCEP datastets" [and a discussion about extreme values follows]. This comment seems more to relate to the weakness of the present choice of entropy, and does not relate to climate networks. Then:

“Our results suggest that SAT over the tropical oceanic and continental regions [...] may be the most sensitive to anthropogenic forcing because their evolution depend on a delicate coupling involving air-sea-land processes." This sudden apparition of anthropogenic forcing at the end of the manuscript is rather disconcerting, and it is quite unclear how the results shown here strongly support this statement beyond what is generally admitted.

Regarding the form, the authors extensively edited the first version of the manuscript but these modifications are in need of further editing. Typos, obscure or long sentences remain too numerous. E.g.: “Averaging over time scales longer than synoptic, it is thus expected for stochasticity to be large in the extratropics" or “... is primarily linear in terms of waves that result from sea and land-surface forcing", “We are interesting in assessing...".

As a final note, it is always slightly embarrassing to be informed by a reviewer that a manuscript has already been considered for review in a different journal. This is the reason why "Earth System Dynamics" invites authors to inform the editors of previous submissions, an option that has not been chosen by the authors in the present case.

In conclusion, I would recommend to revise the study (and not only the manuscript) more extensively than proposed by the authors. It will be hard to provide messages of

particular physical significance as long as only SAT is analysed. A suggestion could be to focus more on the implications for network theory, in which case a wider class of measures needs to be analysed and discussed. These recommendations imply a formal decision of “rejection" but the authors are nevertheless encouraged to resubmission.

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