Earth System Dynamics Discussions

Earth Syst. Dynam. Discuss., referee comment RC2
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Comment on esd-2021-62

Anonymous Referee #2

Referee comment on "Climate as a complex, self-regulating system" by Roger N. Jones and James H. Ricketts, Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-62-RC2, 2021

General comments

The paper addresses an interesting topic on whether the climate is complex or complicated. After a very nice introduction and focus on the problem, the authors mostly refer to the companion paper (under review on ESD) for answering this question. The manuscript is well-posed in terms of concepts and references, it provides a good review on some outstanding problems in climate science. However, there are some unclear points that should be clarified before the manuscript becomes acceptable for publication. Furthermore, more quantitative results should be considered to support the authors' statements, instead of only considering qualitative discussions.

Specific comments

1. The authors promise to answer the question on the complex or complicated nature of the climate system. However, I did not find evidence for an answer but only a complex system-based approach to characterize the climate system. I really appreciate the efforts made by the authors in introducing different concepts from complex system science (e.g., self-regulation, self-organization, ...) but I would suggest to be more concise and to cut a bit Section 2. Moreover, I would suggest to reset a bit the main aim of the manuscript, not in terms of answering the question but instead of describing how complex system science can help understanding some features of the climate system.

2. Figure 1: why the authors state that CCC is the only one having a monotonic behavior? In my opinion also the others show a monotonic behavior that could be easily used to overcome the step-like fits performed by the authors. I would suggest to revise the corresponding text as well as also possibly add a monotonic fit on both HadCM3 and ECHAM3 data to compare with the step-like behavior.

3. Sections 3.2.2-3.2.3-3.2.4 could be easily grouped together since they are all referring to the companion paper.

4. Section 3.3 is a bit confusing and difficult to read. I would suggest to take care of setting it in a more fluent way.

5. Figure 3: the authors used a quadratic fit. Is there any justification for this type of fit? Moreover, I would suggest to add the parameters of the fit that would also allow to compare TEP/TWP and CMIP5 results.

6. Figure 4: I would suggest to perform a significance test for correlations. Indeed, having a certain correlation coefficient (also high) does not necessary mean that the correlation is significant. Moreover, the correlation used here is a linear correlation, based on peak-to-peak comparison between two signals. What about using a nonlinear estimator for correlation as tools based on information theory (mutual information or transfer entropy)? The latter could also enfore the results of the companion paper on Granger causality.

7. Figure 5: what is the reason of using different step-like fits for the surface latent heat flux from 2060?

8. Section 4: in my opinion this is the core of the paper but it is not really clear and exhaustive. I found that it is surely well-posed in the context but it lacks of new results and it is only speculative. Why the authors did not perform any kind of analyses or approaches based on complex system science to investigate self-organization, scale-invariance, teleconnections? I would suggest to carefully take care of this part of the manuscript by reducing the huge reference and description to previous works and by including some results based on complex system tools.

9. I would suggest the authors to take care of correct referencing of figures and references as well as to the style of the text (fonts and missing words). An overall revision of the manuscript in terms of style and form would be a benefit.