

Interactive comment on “An emergent transition time-scale in the atmosphere and its implications to global-averaged precipitation control mechanisms, time-series reconstruction and stochastic downscaling” by Miguel Nogueira

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Author Comments on Review by Anonymous Reviewer #1

I've found the reading of this paper very interesting and revealing. I am an engineering hydrologist and the problem tackled in this paper is somehow far from what I am usually looking at, which is in general at a more local spatial and temporal scales. Nevertheless, it is interesting for us engineering-hydrologists to better understand the connections between Clausius-Clapeyron and precipitation, since C-C is sometimes used

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to explain changes even for local and extreme rainfall. I'm not an expert on climate dynamics and stochastic climate, therefore my review has to be taken as an outsider evaluation of the work. I hope the Author will find my remarks useful, although some time naïf, if he is interested to reach a wider readership among non-experts like myself.

R: I want to thank Reviewer #1 for his detailed, very useful and timely review of the manuscript. Responses to all Reviewers' comments are provided below. All changes to the original manuscript are highlighted in yellow in the main document.

Major comments:

Title: based on my reading, the emergent transition timescale is not an original finding of this paper. The paper rather demonstrates the relevance of including the energy constraints of Equation (2), the atmospheric energy balance, to understand the elasticity of global precipitation to covariates. Shouldn't the title reflect this main focus of the paper?

R: This is a good point. I've changed the title to "The multi-scale structure of the atmospheric energetic constraints on global-averaged precipitation" which should better reflect the main focus of the present manuscript.

It is unclear to me, at least it is not evident, why would one need the model proposed in Section 4 of the paper. To do what? To investigate how climate change may affect global precipitation? To build long-term global precipitation timeseries for the past? I would suggest the Author to be more clear on the usefulness of this part of the paper.

R: After re-reading Section 4 and the conclusions on this Section with the this comment in mind I agree that this point is not clear in the manuscript. The model has two separate parts: the first is a direct application of a linear response of multi-year precipitation fluctuations to fluctuations in the atmospheric radiative fluxes (or temperature). This linear relation is suggested by the respective strong correlations found at multi-year time-scales. The main goal of employing this linear model is to demonstrate

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the validity of the correlations reported and how they are directly translated in a direct climate response (sensitivity) of precipitation to radiative fluxes (or temperature): for example, a fluctuation in DLR at multi-year time-scale has a direct response of precipitation which is, to a very good approximation, linear. The second part is based on the multi-scale stochastic scale-invariant properties of fields. The scale-invariant properties of precipitation and other atmospheric also show transition at a similar range of time-scales (~ 10 -days to 1-month in the atmosphere and ~ 1 -year in the oceans, references in the manuscript), separating two different scale-invariant scaling regimes. Thus, the stochastic scale-invariance should be intrinsically connected to the correlation structure emerging from the results. Furthermore, stochastic scale-invariance has very high potential for stochastic downscaling applications, since it establishes simple relations between the statistics at different time-scales. This potential is demonstrated by the present results in this manuscript. To make the messages clearer, I've split Section 4 in two Sections (4 and 5), the first presenting the linear fluctuation model results and the second presenting the stochastic scale-invariant downscaling. I've also added an introductory paragraph to each of these Sections and edited the abstract and conclusions to make the message clearer.

Minor Comments:

Line 22: I cannot figure out what is the “new perspective” provided by result (v). I would suggest the Author to be more explicit here.

R: There is large spread in the estimates of precipitation sensitivity to temperature fluctuations, for example amongst CMIP5 models. The spread in the correlations here between different datasets also suggest spread in precipitation sensitivity to temperature fluctuations. However, I recognize this is not this simple and, additionally, not really explored in the present manuscript so I decided to remove this sentence.

Lines 92-93: is the improvement of climate simulations and future projections one of the final aims of the research conducted in this and related papers?

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R: As stated in the beginning of the introduction “even the long-term response of global-averaged precipitation is still poorly understood, constrained and simulated (Collins et al., 2013; Allan et al., 2014; Hegerl et al., 2015), largely due to the limited knowledge on the complex interactions between the key components of the atmospheric branch of the water cycle and its forcing mechanisms.”. The line of work presented here allows to disentangle some this complexity, to evaluate how models reproduce the observed variability and the key mechanisms controlling the variability at different time-scales. This is the idea of this sentence.

Lines 94-104: the Author has already looked in previous publications at the topic in the title of this paper. Therefore my suggestion to focus in the title on what new is in this paper. Is the main contribution the one stated at lines 105-106?

R: Good point, the title was changed accordingly.

Lines 183-184: here a moving window procedure is applied. Presumably, what is evaluated in subsequent windows is highly correlated in time (because of the moving windowing). Isn't this a nuisance in the methodology, for instance when calculating the covariance in equation (6)? My experience is that doing statistics on moving windows is trickier than on non-overlapping ones. Maybe the Author could add a line to comment on eventual difficulties here.

R: This is an interesting and difficult question, which is not directly answered here. First, notice that at the suggestion of the Reviewer, the multi-scale analysis was changes from DCCA to Haar fluctuations. Nonetheless, the correlations of Haar fluctuations are also obtained considering overlapping windows. I've added a note of caution at the end of Section 2.2 on this point. The key argument is that the present investigation uses a previously developed and demonstrated methodology (Haar fluctuation correlations) and assumes it accurately represents the correlation structure. Additionally, it is shown that the Haar fluctuation correlations are identical to the cross-correlations derived using DCCA (another well-established methodology DCCA, see Section 3). Thirdly, the

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derived correlations have some physical meaning (energetic constraints of precipitation, Clausius-Clapeyron,...). Furthermore, Podobnik et al. (2011) have compared overlapping and non-overlapping windows in DCCA and established the significance of both. Finally, a more empirical argument is that overlapping boxes are a widely used method that allows us to obtain better statistics because the data points are finite.

Lines 184-188: maybe it is just me, but I did not understand this “local trend” removal. Why is it needed? I can see that this is what makes the cross-correlation analysis different at different time scales, but I would suggest the Author to explain its meaning also in plain words, for the non-technical readership.

R: This comment by the Reviewer highlights the importance of the major revision suggested by the other Reviewer, changing DCCA to Haar fluctuations. It is true that DCCA is not very transparent. At Reviewer #2 suggestion I've changed the multi-scale correlation estimation methodology from DCCA to Haar fluctuations, which I believe are easier to understand. Nonetheless, the key idea in both methods is to disentangle the fluctuations at a given time-scale (and repeat the procedure at a wide range of scales). Removing the local trend is, in plain-words, removing lower-frequency variability in a nonstationary time-series.

Lines 201-203: what is the null hypothesis against which correlation significance is desired? Isn't it possible to obtain the right values for correlation significance, e.g. through simulations? Why is it difficult to do? Is it because of the overlapping moving windows used in the procedure?

R: Podobnik et al. (2011) developed and employed two statistical tests for the significance of DCCA cross-correlations as function of time lag n , based on the assumption that a series is either uncorrelated or power-law correlated. They show that to test the significance of DCCA cross-correlation and reject the null hypothesis it is necessary to compare it with a critical point, which are obtained using the detrending approach depends on the level of confidence required (e.g. 95%), time-series length and length

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of the overlapping window considered. For the goal of the present paper, it seems to me that it is sufficient to see that there is a clear transition in the correlation magnitudes between time-scales, from strong to weak. The weak correlations are within the values previously shown to be negligible for this type of methods. Adding further complication and computing the critical points for my case, would make the paper more obscure without a clear gain in my opinion. I've tried to make this point clearer in the methodology Section and refer to this previous work.

Line 235: is the “large spread” in the results obtained using different datasets?

R: You are correct, the sentence was reworded.

Lines 252-254: would one obtain larger correlations at short timescales if time lags would be used between variables? I would suggest the Author to discuss the time lag issue, if relevant. R: No. The correlations at different time-lags was tested in Nogueira (2018) and no relevant change of the correlation structure was found. This reference was included in the text.

Line 261: I guess “Fig. 1a” should be “Fig. 2a” here. R: Yes. This was corrected, thank you

Lines 311-following: I would suggest here in the summary paragraph to restate the full names together with the acronyms. This is just my personal preference. As a non-expert reader, I am overwhelmed by the acronyms at this point.

R: I have removed most of the acronyms throughout the manuscript, replacing with full names, to make the text clearer.

Lines 334-344: this is a very important part of the paper, i.e., what are the implications of the results obtained here. I would suggest this part to be extended and maybe moved to the discussion section. I cannot really understand what “the more fundamental transition in the atmosphere” is. Also, the discussion between fast and slow P sensitivities deserves more space and discussion, if this paper is really shading more

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light on them.

R: I have moved this discussion to the final section and elaborated on how the correlation structure relates to the fast and slow components. The “more fundamental transition” refers to ocean-atmosphere coupling as seen by SST vs Tland correlations. This is also discussed in the final section.

Section 4. Stochastic model: I miss the motivation on why stochastic modelling is needed and useful here. I guess to demonstrate the robustness of the regression based models, as stated at line 550. I would suggest the Author to explicitly comment on the usefulness of the section at its beginning.

R: I've separated the stochastic model in Section 5 and added a paragraph in the beginning to explain its motivation.

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