

# ***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***

**Anonymous Referee #1**

Received and published: 23 October 2018

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 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-

[Printer-friendly version](#)

[Discussion paper](#)



## Interactive comment

time naif, if he is interested to reach a wider readership among non-experts like myself.

Since I very much enjoyed reading the paper, as said, I have no major suggestions to provide, except the following two:

1) 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?

2) 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.

#### Detailed 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.

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?

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?

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.

[Printer-friendly version](#)

[Discussion paper](#)



---

Interactive  
comment

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.

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?

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

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.

Line 261: I guess “Fig. 1a” should be “Fig. 2a” here.

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.

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 light on them.

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.

[Printer-friendly version](#)

[Discussion paper](#)



## Interactive comment

Equation (9): here the Stefan-Boltzmann equation comes in, making the linear model to predict P based on DLR very different from the one directly using SST. Are the differences in Figure 4 (for example) explainable based on this difference of the two models?

Line 416 and Figure 4a: why is there bias in the simple regression models proposed in this paper? Shouldn't the calibration of the regression coefficients remove the bias? I guess it is more complex than that but I would suggest the Author to explain what could be the causes of bias in the GPCP models and in the proposed ones.

Line 431: here I would have expected differences because in one model SST comes in through the Stefan-Boltzmann equation, i.e. in a very non linear way. I am clearly missing something here. The Author may try to guide better the non-expert reader as me by explaining how the different models result in the same Pearson correlation coefficient of Figure 4d.

Section 4.2. Stochastic model at the monthly timescale: again, I miss the motivation on why stochastic modelling is needed and useful here.

Line 491: the result here, i.e. the fact that the proposed regression based models once downscaled outperform the GPCP models, is indeed remarkable. What could be the reason for that? I expect the Author to "speculate" on this in the discussion section.

Figures: it would be nice for the reader to see a plot of the timeseries of global variables (for P, SST, DLR etc), say at the annual (or larger) timescale for many years and at the seasonal timescale for a shorter period, to inspect visually their shape and potential relationship.

Figure 1: the heading of the second panel is missing.

Figure 4: isn't the RMSEbc simply the square root of the variance of estimation? If so, why calling it RMSEbc?

[Printer-friendly version](#)

[Discussion paper](#)



---

Interactive  
comment

