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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 Shaun Lovejoy

This paper addresses the important question of the consequences of anthropogenic warming on global precipitation. Following Lovejoy et al 2017 (incorrectly cited as Lovejoy et al 2018), the author uses the cross-correlations of fluctuations to show that there is a relatively abrupt transition from weak to strong correlations with a transition at scales of around 1-2 years. He applies and develops this idea to quantities related

to the precipitation energy budget. He thus clarifies the variables that become correlated with precipitation and the time scales at which this occurs. Without such clarity about the appropriate time scales, any relationships between variables will be either questionable or spurious. Systematic approaches such as those presented here are therefore urgently needed; this paper has the potential of being a major contribution to the field.

R: I want to thank Shaun Lovejoy for his insightful review, which I believe has helped to improve the quality of the manuscript. Responses to all Reviewers' comments are provided below. The Reviewer's comments are in black and the author's responses are in blue. The citation Lovejoy et al. (2018) was corrected to Lovejoy et al. (2017), thank you. All changes to the original manuscript are highlighted in yellow in the main document.

To date, a technical issue that is important in the macroweather regime has prevented clarity. The macroweather regime covers roughly the lifetime of planetary structures (_10 days) and continues up until scales dominated by anthropogenic warming – i.e. up to the climate scales (currently at 20- 30 years for the temperature, a little longer for precipitation). In the climate regime, fluctuations begin to increase rather than decrease with scale. It is the ill-appreciated fact that standard correlation analyses suffer from low frequency biases due to the climate variability. The mathematical issue is that when fluctuations increase with scale – as they do in the climate regime – then correlation functions have low frequency divergences. It would be worthwhile for the author to mention this since this motivates his avoidance of the problem by the use of fluctuations.

R: This is a good point, I've included a mention to this issue in Section 2.2.

In order to determine the fluctuations and their correlations, the author uses detrended cross-correlation analysis (DCCA). The DCCA is an adaptation of the detrended fluctuation analysis (DFA) technique and suffers from the same drawbacks and limitations.

These are unfortunate consequences of its ad hoc nature: the fluctuations in the DFA and DCCA are defined in an unnecessarily obscure and complex-to-analyze manner (in terms of RMS residuals from polynomial regressions to the running sum of the original series). The resulting obscure definition leads to an inability to fully exploit the information contained in the fluctuations. For example, typical published DFA analyses do not even bother to put units on their fluctuation function because the function has no simple meaning! Only slopes to lines on log-log plots are considered interesting so that almost all the information contained in the fluctuations themselves is effectively discarded. In the present paper, there is a similar waste of information: only the correlation coefficients at different time scales are given. The obscure meaning of his fluctuations prevents the author from directly making statements about the coefficients of the linear relations that are obtained at each time scale. This is a pity. If the author had used Haar fluctuations (simply the difference of the averages of the data over the first and second halves of an interval), the interpretation would have been nearly trivial. The author could have fixed the time scale and then, at that scale, display meaningful and insightful scatter plots of fluctuations of one quantity against another including linear (or other) regression relationships.

R: Thank you, this is a good argument, which was corroborated by the doubts on the DCCA fluctuations and detrending technique raised by the other Reviewer. Hence, I've decided to use Haar fluctuations instead of DCCA. I've added the description of Haar fluctuations in Section 2.2, changed the results presentation in Section 3 accordingly (including Figures 1, 2 and 3), and made also made the respective changes to abstract and conclusions. As expected, the resulting correlation at different time-lags are remarkably identical to the results previously obtained by DCCA (now presented in supplementary material), providing robustness to the correlation structure presented. Since Haar fluctuations are easier to interpret, I decide to keep the latter.

By comparing plots at say Dt = 1 month, 1 year, 10 years, one could then visually notice that the regression lines tighten up at larger Dt and one could directly note the physically significant slopes at the longer (highly correlated) scales. One could then use standard statistical goodness of fit criteria and uncertainty estimates for the resulting regressions (correlation coefficients are not optimal for uncertainty analyses). Another advantage is that the author could also use multiple regression - simultaneously between fluctuations of several variables. At the moment, he is forced to make a series of awkward sequential tests of fluctuation pairs, trying to find the most significant relationships. While the author's main conclusions are likely to be similar, the resulting paper would be more accessible and convincing. At the moment, most atmospheric science readers will simply see the DCCA as a "black box" and fail to appreciate its significance.

R: After the major change to methodology, I decided to keep some of the manuscript structure. I believe that Figures 1, 2 and 3 (now obtained from correlations of Haar fluctuations) are good for highlighting and illustrating a relevant transition in the governing mechanisms of precipitation (and precipitable water vapor) mechanisms between sub-yearly and multi-year time-scales. The variables in these correlation plots are guided by Clausius-Clapeyron and radiative constraints of precipitation, and hence their choice is not random. The suggested scatter plots would in fact have relevant information, but this information is partly in the correlations and partly in the ability of the tested simple linear model in reproducing observations: i.e. the fluctuations in precipitation can in fact be derived from DLR (and somewhat worse from SST) fluctuations using a sensitivity coefficient. Adding the scatter plots would make the manuscript too extensive without too much added value, in my opinion.

The number of acronyms was enormous and I was constantly searching through the text to remind myself of the more obscure ones. Perhaps the author could provide a convenient table for this purpose?

R: I've reduced drastically the number of acronyms throughout the manuscript, replacing by the full names in most cases.

The fGn simulation is obtained by filtering Gaussian white noise. In principle this is fine, but there are potential high and low frequency numerical issues and it would probably worth using a packaged routine (available now on a number of platforms)

R: The fGn generation algorithm employed does not seem to have low or high frequency numerical issues for the considered. The Figure attached is the ensemble spectrum of the 100 realizations of beta=0.3 interpolating time-series (black) with the respective reference beta=0.3 line (red)

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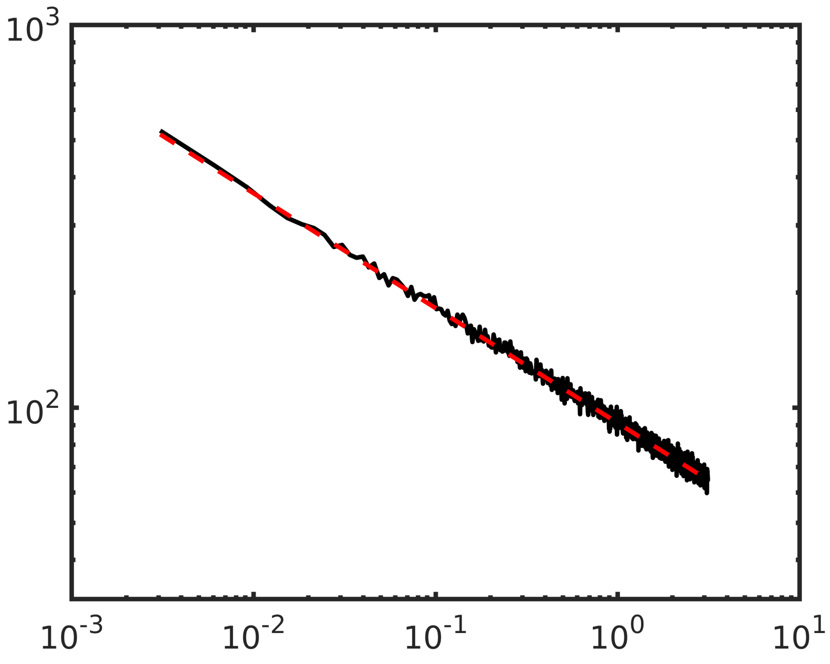

Fig. 1.