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Title: Observation based temperature and freshwater noise over the Atlantic Ocean

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Point-by-point reply to reviewer #2

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We thank the reviewer for their careful reading and for the useful comments on the manuscript.

Overview

This study considers the statistics of monthly-mean 2m temperature and E - P fluxes in ERA5 and CMIP6 Earth System Models. Focusing on the standard deviation, skewness, and reduced kurtosis the authors consider how well different noise models (both principal component based and pointwise) reproduce the observed statistics. It is found that the best fit to observed variations is obtained from local Normal Inverse Gaussian (NIV) distribution fits. Assessment of CMIP6 model performance demonstrated both systematic differences as well as substantial inter-model spread.

The study is interesting and the manuscript is clearly written. It is my assessment that a number of points require further elaboration and contextualization, and aspects of the presentation should be clarified. As such my recommendation is that the paper be accepted after major revisions. Detailed comments follow.

Major comments:

1. *Throughout the manuscript, discussions of skewness and kurtosis are provided without accounting for sampling variability. While the record length of $O(10^3)$ is not short, sampling variability of skewness and particularly kurtosis will be nonzero. The fact that two independent simulations of the same ESM and the same length as the Reanalysis data generate substantially different higher order statistics (LL 251-253) indicates that sampling variability may be appreciable. In order that the*

paper focus on robust non-Gaussian structures the revised manuscript should provide an estimate of the statistical significance of the skewness and reduced kurtosis relative. As a null hypothesis I recommend fitting the monthly E - P and T_{2m} data to Gaussian AR(1) processes and then generating sampling statistics from the fit model. This approach would account for the reduction in the effective number of statistical degrees of freedom in the assessment of statistical significance. Another approach that similarly accounts for serial dependence of the data would also be appropriate.

Author's reply:
We agree with the reviewer that the sampling variability may play a role here. We appreciate the suggestion of the test of statistical significance.

Changes in manuscript:
We will follow the suggestion of the reviewer and test the statistical significance.

2. *The non-Gaussianity of sea surface temperatures (admittedly not T_{2m}) was previously considered in Sura and Sardeshmukh (2008). I recommend relating the results of the present study to this earlier one. Sura and Sardeshmukh considered daily variability but it may be the case that for SST differences between daily and monthly variations are modest.*

   Author's reply:
   We thank the reviewer for pointing out the paper of Sura and Sardeshmukh. Even though there are definitely differences between SST and T_{2m}, and daily and monthly data, we think it is important to include this in the discussion to put our results in a more complete perspective.

   Changes in manuscript:
   In the discussion we will relate the results of Sura and Sardeshmukh to the results found in our paper.

3. *While the fields of statistical moments provide important spatial information about variability it has been my experience that valuable comple-*

mentary information is provided by inspecting probability density functions at representative locations. As the fields of skewness and kurtosis generally show large-scale spatial variations it should be possible to find regions with representative pdfs. I recommend that the revised manuscript include figures showing such representative pdfs.

Author's reply:
We agree with the reviewer that finding regions with representative PDFs would add value to the results.

Changes in manuscript:
Following the suggestion, we will look for representative PDFs and include them.

4. *LL 138-139: I do not understand the description of the PC(1) method. My assumption is that it involves sampling the same time point from all PCs, but it is not clear if sampling is with or without replacement and in either case the number of potential samples is much greater than 996. In fact, reference is made later in the manuscript to generating realizations of 10000 members using this method. The revised manuscript should include a clear description of this method.*

   Author's reply:
   All PCs are 996 months long. For the PC(1) method we uniformly sample one integer from 1 to 996. We apply this integer for all PCs. For example, if our integer is 7, then we sample the 7^{th} month of each PC to construct the noise model. Using this method we therefore have in total 996 different realizations to sample from. The sampling is performed using replacement. In the manuscript we have used a timeseries of 10,000 realizations, which means we sampled the integer 10,000 times to consequently construct the noise fields.

   Changes in manuscript:
   A clearer explanation of the method will be provided in the revised text.

5. *I completely agree with the interpretation that the PC(N) model fails to capture non-Gaussianity as it breaks any dependence that might exist*

between the PCs so PC-marginal non-Gaussianity will be suppressed by central limit theorem type reasoning. A complementary interpretation is that there is meaningful dependence between PCs which plays an essential role in generating the pointwise non-Gaussianity. In principle, a noise model could try to capture some of this dependence. The revised manuscript should include a discussion of accounting for dependence between PCs and how this could lead to a noise model with correct pointwise statistics but also allowing for spatial dependence (cf LL 284-286).

Author's reply:
We thank the reviewer for this complementary interpretation and the suggestion to discuss a possible noise model to capture some of the dependence between PCs.

Changes in manuscript:
We will add a discussion on this issue in the last section of the revised paper.

6. *The Normal Inverse Gaussian distribution is one among many possible parametric models that could be used to fit the data (e.g. the skewed-t distribution). Why was the NIG distribution chosen for this particular application? Particularly given the fact that this parametric model cannot always well capture the kurtosis of the data (cf. Figs 6,7) the revised manuscript should justify the sole focus on this particular parametric distribution.*

   Author's reply:
   We have tried several (more than 10) different distributions, among which the skewed-t distribution, but none of them performed better than the NIG distribution.

   Changes in manuscript:
   We will clarify that we chose the NIG distribution after testing several different statistical distributions because it performed best.

7. *Section 5 is quite long, and much of the material repeats what was said*

in previous sections (e.g. identifying where there are biases between the statistics of the data and of the MMM). I recommend revising this text to reduce its length and reduce overlaps with other parts of the manuscript.

Author's reply:
We agree.

Changes in manuscript:
Section 5 will be shortened following the suggestion of the reviewer.

Minor comments:

1. *LL 49-50: ERA5 is based on observations but as the authors note as a reanalysis product it is a model simulation. I recommend avoiding use of "observations" in describing reanalysis products (particularly for a quantity like E - P which is not assimilated).*

   Author's reply:
   We thank the reviewer for pointing this out.

   Changes in manuscript:
   We will use different wording for the ERA5 data.

2. *Section 4: It is implied but not stated explicitly (that I noticed) that ERA5 data are used for the noise model. This point should be explicitly stated.*

   Author's reply:
   The noise models are indeed based on ERA5 data.

   Changes in manuscript:
   We will explicitly state that the ERA5 data is used for the noise models.

3. *It appears that Figures 5 and 6 were swapped in the submitted manuscript. This should be corrected in the revised manuscript.*

Author's reply:
The reviewer is correct.

Changes in manuscript:
Figures 5 and 6 will be corrected.