Again, this manuscript explores the question of whether recent extreme hurricane seasons can be attributed to changes in circulations and SST. To explore this question, the authors use a new statistical model to explore weather patterns. They find that increases in Atlantic SSTs have led to an increase in the probability of extremely active hurricane seasons. It is clear from the revised manuscript that the authors have improved presentations since the last submission. However, before the manuscript is suitable for publication in Weather and Climate Dynamics there are several minor comments that still need to be addressed:

We thank the reviewer for reviewing a second time. We appreciate the useful comments, questions and suggestion that in our view strongly improved the manuscript!

L7: From the results section, I am a little unclear of where the number 50% comes from. Figure 6e? This percentages should be directly calculated in the results section (i.e., L228 should provide the value for the 2020 counterfactual) to be mentioned in the abstract. Same is true for the "doubled the probability of extremely active tropical cyclone seasons" statement in the previous sentence.

We agree, that this number is a bit misleading, especially as the next number is given as a factor. As we are more concerned about extremely active seasons we removed this statement from the abstract. We nevertheless reformulated parts of the result section to avoid confusion with factors and percentage points:
'Consequently, more than one third of the seasons that are simulated to be above normal seasons in the 2020 scenario are below normal seasons in the 1982 scenario (an above normal seasonal activity being defined as > 126.1 ACE (CPC, 2021)). Similarly, the number of simulations that are classified as extremely active (with ACE > 159.6) doubles from 11% in the 1982 scenario to 22% in the 2020 scenario (see fig. 6g).'
The part in the abstract is now:
'For the year 2020, our results suggest that such an exceptionally intense season might have been made twice as likely.'

L56: ERA5 has a 30km horizontal grid resolution not 1 deg., correct? Please clarify.

That is correct. We used the option of the copernicus climate data store (https://cds.climate.copernicus.eu) to download the data on a 1°x1° grid. We agree, that this pre-processing step should be transparent in the manuscript. For better readability, we explain the procedure as follows:
'For the following pre-processing we transform the data from the original 0.28◦ x0.28◦ to a 1◦ x1◦ grid. In order to remove the direct influence of TCs in the reanalysis data we replace the 3x3 grid-cell square area encompassing the center of the storm with the average of its surrounding 16 grid cells. Finally, we transform the data from a 1◦ x1◦ grid to a 2.5◦ x2.5◦ grid and average 6-hourly data to daily data.'

L60: Note, that the MDR displayed in Figure S6 is 10N-20N, not 10N-30N as stated here in the text. This should be consistent. Which is used for the analysis?

We thank the reviewer for noting this inconsistency! The definition of the MDR that we use in the analysis is 10N-20N. We corrected this in the manuscript.

L87: Why 1982-2011? This is different than the period mentioned in Section 2.1.

For the standardization of the variables we want to use a period that is at least 30 years long. As we are planning to use the emulator for an analysis based on CMIP6 simulations we thought it would be convenient if the period that we use for standardization is fully covered by the historic simulations which only reach 2014. In that sense the period chosen here is an arbitrary 30 year period. We do not expect that choosing a different period for the standardization would significantly alter the results.

L95: Should this reference figure S7?

Indeed, we wanted to reference figure S7.

L170: The authors could consider including a large-scale predictor of these storm precursors. Hsieh et al. 2020 (https://doi.org/10.1007/s00382-020-05446-5) showed that vertical velocity is a good predictor for the number of convective cluster and that the ratio of the local Rhines scale and the Rossby deformation radius is a good predictor of the number of these convective clusters that become weak rotating systems. This might be a good direction for future work.

We thank the reviewer for this relevant reference. This might indeed be helpful for future work. In the current design of the emulator we do not resolve the location of storms which might be one of the major caveats. This makes the inclusion of indicators such as vertical velocities challenging. Now, that we have developed this simple emulator, it would however be interesting to think of ways that would allow to include the location of storms or more indicators that would help to better represent storm formations.

L206: Any comment on why the HadISST seems to have warmed less (Figure 5a should 0.23 K per decade) over this time period than the DOISST?

We agree that this is worth mentioning here, even though we already commented on this in line 220 of the previous manuscript. We added in line 206: 'This trend is slightly weaker in the HadISST dataset (figure 5a) for which also the global trend in SSTs over the period 1982-2020 is weaker than in other SST datasets (Yang et al., 2021).'

Figure 5c: The link colors for the 1982 and 2020 counterfactuals should be the same for Figures 5 and 6. Also, the legend should state 1982 and 2020, not 1980's and 2020's to be consistent.

We thank the reviewer for pointing out these inconsistencies. We updated the figure accordingly.

L220: I would again specify that this is true for the MDR: "…pre-industrial levels for the MDR."

In the revised manuscript, this is clarified as suggested by the reviewer.

Figure 6a: This panel is never referred to in the text and the caption states that the observed value is displayed in black, which it is not. Also, there is only two counterfactual scenarios not three as stated in the caption.

We updated the figure caption according to what is shown in the panel. We were also thinking about dropping the panel as the counterfactual SST scenarios are indeed already shown in figure 5c. We concluded that it is still helpful to have these SST time series in figure 6 to allow for an easier interpretation of the counterfactual simulations in the following panels.

Figure 6b: Why are some purple boxes a lighter shade?

These are years where the seasonal SST averages in the counterfactual scenario lie outside of the range of observed seasonal SST averages. We added a clarifying sentence in the figure caption: 'For years, where the seasonal SST averages in the counterfactual scenario are outside of the range of observed seasonal SST averages, the simualtions are shown in lighter shading.'

Figure 7: 1983 is not mentioned in the caption. Also, this figure should use different coloring than Figures 5 and 6, so as to not confuse the reader.

We agree with the reviewer and changed the caption and the colors accordingly.

L240: Consider adding a vertical lines to Figure 7a for "above normal season" and "extreme active seasons."

Thanks for the suggestion. We added these lines.

L244-246: What about for the observed 178 ACE?

The probability of finding 178 ACE with 1982's SST levels is 21\% according to our simulations. We clarified this in the revised manuscript.

L250-251: Why is it higher for all years?

If favorable weather conditions lead to many storm formations in a season there is a considerable likelihood of reaching 178 ACE irrespective of the SST levels. For instance, for 2005 half of the simulations with 1982's SST levels are extremely active seasons and roughly 40% of the simulations reach 178 ACE. While in our simulations warmer SSTs will further intensify these extremely active seasons, this will not affect the increased risk of getting more than 178 ACE. Therefore, the increased risk of finding seasons with more than 178 ACE due to SST warming is more pronounced for years with lower TC activity. (In the extreme case, where only a few simulations with 2020's SST levels reach 178 ACE and none of the simulations with 1982's SST levels, the calculation of the increased risk would require a division by zero.)
We included a comment in the revised manuscript:
'The increase in likelihood of finding 178 ACE is higher for seasons with weather conditions that are hampering TC formation and development.'

L270: What is this 0.5 K of global warming referring to? Global average surface temperature? Figure 5b. shows that the MDR has warmed by over 1.1 K over this period, right? This sentence should be clarified.

We agree, that this sentence could be misleading. In the revised manuscript we clarified, that we are writing about an increase in global average surface air temperature.