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We appreciate the comprehensive and constructive questions and suggestions from the reviewers. Below are our responses in blue italics. Any necessary revisions made to our manuscript are also indicated below.

Reviewer # 2

The authors focus on the characterization of weather regimes that favor tornado outbreaks, and the persistence of such regimes. While an interesting presentation, their work does tread a similar vein to a number of recent studies, which aren't fully acknowledged in the introduction. There are several points where it is difficult to follow the methodological tests taken, and the applications of statistical significance tests are not clear as written. A few of the figures also need to be adjusted for accessibility. Finally, while the authors claim that these results will help with seasonal prediction, I'd actually like to see a little more of a connection as to how they envisage this improving on the existing paradigm. While these issues exist, they are not overly burdensome, which leads me to recommend a set of minor revisions.

Thank you for your constructive feedback. The seasonal prediction is an aspect that we are presently working on and are excited to report in due time.

Minor Comments:

Introduction, First Paragraph: I'm not convinced the approach taken by the authors here really fits with the manuscript. Talking about trends and variability does not connect well with their primary focus on the occurrence of weather regimes favoring tornado outbreaks at least as presented. The authors could stand to connect their work to analysis of trends and identifying what the gaps may be, which would help the flow of the text.

The first paragraph was meant to start the introduction into our motivation for going in this direction. Our previous study (Graber et al. 2024) *found trends in tornado days and tornado outbreaks in the Great Plains and Southeast. The warm-season trend throughout the CONUS had a noticeable decreasing trend. We lack a physical explanation for why this is occurring though, and the WR approach is our attempt to provide a physical explanation for this decrease. The modeling is done as a means of being useful to key stakeholders that value the results we provide.*

Line 48: The literature cited here portrays an incorrect precedence of the understanding derived. Arguably, any such reference to ENSOs influence on tornadoes should include Cook and Schaefer (2008), which was the first study to show a wintertime signal, and a reference to Allen et al. (2015), which showed the first statistically robust connection to the environment in springtime, 500 hPa geopotential height and inferred cyclonic track, and observations, and developed the first seasonal prediction algorithm – which is directly relevant to the analyses presented here.

Thank you for making note of these important references from the literature. Both studies have been added into the revised version of the manuscript:

"Cook and Schaefer, (2008) examined winter tornado outbreaks in relation to the phase of the El Niño – Southern Oscillation (ENSO) and found that a La Nina phase favored tornadoes in the Southeast and a neutral phase favored tornadoes in the Great Plains. Allen et al., (2015) further found that La Niña (El Niño) years typically coincide with more (fewer) tornadoes in the spring across the central CONUS, and that the winter ENSO phase can be used to predict tornado frequency during the spring."

Paragraph beginning Line 73: It would seem appropriate to reference that the European community has long used weather regimes to look at relationships with significant severe weather events. See Punge and Kunz (2016) and references therein.

We have cited Grams et al. 2017 in the revised manuscript who we believe was the first study to investigate year-round weather regimes which were done in Europe.

"Lee et al., (2023) applied the year-round WR method (Grams et al., 2017) over North America and defined four year-round WRs."

Paragraph beginning Line 62: It seems strange to me that the literature cited for weather regimes does not include Tippet et al. (2024). The subsequent references to this paper also seem to neglect that study considering variability.

(Tippett et al. 2024) was referenced in line 99 to discuss their findings in the year-round weather regime analysis and how they did not have any significant results in June-August. However, they did not discuss persistence in their study, which ours does. This paper came out not long before we initially submitted this manuscript to WCD. Lee et al. 2023, from the same research group, was also cited. It also important to cite Grams et al. (2017) here, which we have now included, since this was the study to first introduce the concept of year-round WRs. Grams et al. (2017) did year-round WRs over Europe and Lee et al. (2023) did it over the CONUS.

"Lee et al., (2023) applied the year-round WR method (Grams et al., 2017) over North America and defined four year-round WRs. Tippett et al, (2024) identified statistically significant relationships between these year-round WRs and tornado activity in all months except June through August, but Tippett et al. (2024) made no consideration of WR persistency."

Line 108: It is not clear what is meant by the authors by the statement 'and to avoid too many near-zero CP values in a daily mean', suggest clarifying.

A daily max was used for convective precipitation (CP) because there may be many hours in a day where no CP is occurring, thus if we were using hourly data we would probably get very low *values for almost every day since many hours would be equal to 0 mm/hr. This way, days with an hour or two of significant convection are easier to pick out.*

"Daily maximum values of MUCAPE and CP at each grid point were used to represent the daily peak instability and in turn amplify the signal for days with the potential for significant convective storms."

Line 116: What mean long term trend was used for the detrending procedure exactly? Given you are interested in variability, to what extent is the trend removed influenced by the ending year?

*This has been made clearer in the revised manuscript: "*The 500H data were then detrended by removing the linear trend of the seasonal mean (AMJJ) 500H averaged over the entire Northern Hemisphere (Fig. S1). The detrending approach removed the positive trend of hemispheric mean 500H caused by climate change while preserving the spatial patterns and potential changes of WR frequency or persistence."

Paragraph beginning Line 154: It would seem more appropriate to compare to the weather regimes of Tippett et al. (2024), given that these are tornado focused.

Both Lee et al. 2023 and Tippett et,al. 2024 use the same weather regimes, but Tippett et al. 2024 would be a better reference to put in this paragraph given that they specifically focus on tornadoes. Lee et al. 2023 was the paper that originally introduced the annual weather regimes.

"Some WRs are similar to the year-round WRs in Lee et al., (2023), which were subsequently used by Tippett et al., (2024). More specifically, WR-A features spatial similarities to a Pacific Trough, WR-B and WR-D show warm and cool phases of a Pacific Ridge associated with ENSO, and WR-E is characterized by an Alaskan Ridge. WR-C features spatial similarities to a Greenland High as well. It is worth mentioning that our study focuses on a different region, a specific season and chooses a different k value, and there are thus noticeable differences. WR-A features two anomalous highs over the two coasts as opposed to one anomalous high over the central-CONUS. The anomalous low in WR-B is more pronounced than in Lee et al., (2023). The anomalous high in WR-C is wavelike unlike the Greenland high in Lee et al., (2023). The dipoles in WR-E are further south than they are in the Alaskan Ridge in Lee et al., (2023)."

Line 221: How is the significance t-test performed here, and how is it indicated on the diagram and for what specifically? This is unclear.

A one-sample t-test was performed between the climatological CP and the WR CP (so each day that was assigned that specific WR) at each gridpoint to test whether the WR mean is significantly different than the climatology at each gridpoint. The null hypothesis is that the sample mean of the anomalies is equal to zero.

*The figure caption now read: "*Significance is tested at each grid point using a one-sample t-test with the null hypothesis that the sample mean of the anomalies is equal to zero, and anomalies with p-values$\leq$0.05 are regarded as significant."

Line 362: The extent to which the WR would help with skillful seasonal forecasts is an interesting one. Given that existing seasonal prediction models do not incorporate weather regimes, and offer skillful forecasts, particularly when climate variability is strong (e.g. Allen et al. 2015, Lepore et al. 2018), I would encourage the authors to discuss what advantages applying the WR approach would achieve over existing models, and how that may contribute to more skillful forecasts rather than the current abstract stateme'nt.

The following statement has been added at the end of the conclusion:

"Furthermore, although not explored in this study, WRs and tornado activity may both be modulated by large-scale, low-frequency climate modes. WRs could potentially act as an intermediary between large-scale climate modes and tornado activity, while the low-frequency modes may be important sources of predictability for the interannual variability of tornado activity."

The importance of this statement is that WRs offer an intermediate between climate change and tornado activity, which is a difficult connection to make considering the large difference in spatial scale size. The forecasting and predictability piece is currently being investigated by our group and will be published in due time.

Figures: There is some colorblindness suitability issues with the figures, particularly green and red overlapping contours. Please address this in the revision.

Thank you to the reviewer for pointing this out. This has been addressed in the revised version of the manuscript. Figure 1 CAPE shaded anomalies are now in the seismic color palette and the S06 anomaly contours are in gold. Figure 2 CP anomalies are now in the viridis color palette, and the tornado day probability anomalies are now magenta and cyan, respectively, for positive and negative anomalies.

References:

Allen, J. T., Tippett, M. K., & Sobel, A. H. (2015). Influence of the El Niño/Southern Oscillation on tornado and hail frequency in the United States. *Nature Geoscience*, *8*(4), 278-283.

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Our References

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Tippett, M. K., K. Malloy, and S. H. Lee, 2024: Modulation of U.S. Tornado Activity by year-round North American Weather Regimes. *Mon. Weather Rev.*, https://doi.org/10.1175/MWR-D-24-0016.1.