Manuscript ID: egusphere-2024-3216

We appreciate the editor giving us a chance to respond and make further edits. Below, the reviewer's comments are in black, and our responses are in blue.

The authors have made only a few substantive changes to the manuscript. It seems to me that a fair amount of the authors' response is arguing why they don't need to respond to comments or simply not responding. In my view, the most egregious problem is that authors refused to either provide their classification data or to make quantitation comparisons: with previous work, including their own.

We appreciate the reviewer for taking the time to review our revised manuscript. However, we respectively disagree with the statement that we "made only a few substantive changes" or refused to provide the data. We made many changes to the manuscript in response to the comments of both reviewers. When we chose to stand by our methodology, we provided justifications both from previous studies and additional new analysis.

Our methodology preference is supported by prior studies, which have indicated that some of reviewer 1's suggested changes may not be necessary given the specific objectives of our work. However, we appreciate the opportunity to further clarify and justify our work. We value the reviewer's insights and are willing to make modifications where necessary.

Firstly, the reviewer claims we made only a few substantive changes to the manuscript following the previous set of revisions. Below we outline the major changes that we made to the manuscript from the previous set of revisions:

1) *Regarding the comparisons with previous work, we had added the following text in the last revision.*

*We added lines 48-54 in the last round of revision: "*For example, 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 Niña 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 years typically coincide with more tornadoes in the spring and El Niño years with fewer tornadoes across the central CONUS, and that the winter ENSO phase can be used to predict tornado frequency during the spring."

The prior text was added to give better precedence to prior work done on ENSO in relation to tornado outbreaks.

We added lines 89-93 in the last round of revision: "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."

The prior text was added to give more credit to WR work done by Lee et al. (2023) and Tippett et al. (2024) given their work done involving annual WRs and tornado activity.

2) More information was provided about our methodology.

*We added lines 118-121 in the last round of revision: "*Daily anomalies of a variable were calculated by subtracting each calendar day's mean from every calendar day, following:

$$\boldsymbol{H}'(\boldsymbol{d},\boldsymbol{y}) = \boldsymbol{H}(\boldsymbol{d},\boldsymbol{y}) - \overline{\boldsymbol{H}}(\boldsymbol{d}) \quad (1)$$

where *y* is year, *d* is calendar day, *H* is the variable or parameter of consideration, and the overbar denotes the long-term mean."

The prior text was added in the methodology to describe the anomalies of MUCAPE, S06, and CP in figures 1 and 2. It was an initial concern by reviewer # 1 of whether the anomalies were with respect to the seasonal or daily mean. We made it clearer that these anomalies are with respect to the daily mean, and the seasonal cycle is thus removed.

*We added lines 144-152 in the last round of revision: "*While many previous studies applied a low-pass filter or/and EOF dimension reduction prior to K-means clustering analysis (e.g., Grams et al., 2020; Lee et al., 2023; Lee and Messori, 2024; Robertson et al., 2020), Falkena et al., (2020) cautioned against the use of either EOFs or time filtering on top of K-means clustering. Our analysis shows that the application of the 5-day running mean or EOF dimension reduction prior to K-means does not qualitatively affect the regime patterns or the regime frequencies (Figs. S2-S4). We thus chose to use the simplest procedures for regime classification. Additionally, unlike Tippett et al., (2024), 500H anomalies are not normalized as we focus on one season, so seasonality is not as much of a concern."

The prior text was added to the manuscript to better explain and defend our weather regime methodology. It is true that Lee et al. (2023) and subsequent studies applied a low-pass filter and/or EOF dimension reduction to their 500H anomalies prior to K-means clustering analysis, but Falkena et al. (2020) made a point that this is not necessary on top of K-means clustering. In our previous response, we made WR spatial structures which applied both a 5-day low pass filter and an EOF filter prior to K-means clustering, and it showed very little change. Since our work focuses on one season, the seasonality effect is not as a big issue as in a year-round analysis. This text has since been removed in the latest version to be consistent with our updated methodology.

*We added lines 163-166 from the first revision state: "*A Monte Carlo simulation test with 10000 resamples was used to test for significance of the anomalies. The number of WR-i days was multiplied by the climatological mean TD probability to get an expected number of tornado days. The p-value was calculated based on the proportion of simulations that were more extreme than the observations."

The prior text was added to provide a more detailed description of the significance testing done in figures 3 and 4.

3) *More information was added in the results section on WR comparison.*

*We added lines 203-213 in the last round of revision: "*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)."

The prior text was added to provide more detailed descriptions on the 5 WRs and relate them to work done by Lee et al. (2023) and Tippett et al. (2024). We discuss spatial similarities between our work and theirs. This text has since been modified to accommodate qualitative comparisons from multiple studies.

4) *Additional discussion was added on the possibility that WRs and tornado activity have sources of predictability from large-scale, low-frequency climate modes.*

*We added lines 400-406 in the last round of revision: "*Furthermore, although not explored in this study, WRs and tornado activity may both be modulated by large-scale, low-frequency climate modes (Cook and Schaefer 2008; Lee et al. 2023; Niloufar et al. 2021; Tippett et al. 2024; Vigaud et al. 2018). Given the potential predictability of WRs (Straus et al. 2007), they may 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 prior text was added to acknowledge that the WRs and tornado activity may be modulated by large-scale, low-frequency climate modes, but it went beyond the focus of this studies to investigate the sources of predictability. This is something that we are presently exploring.

***__Below we summarize the major changes made to the new manuscript__** in response to the reviewer 1's comments on the revised manuscript. More information is provided in our replies to the specific comments.*

1) *Daily mean 500H anomalies with 5-day low-pass filtering and EOF dimension reduction are used.*
2) *The Davies-Bouldin Index* (Davies and Bouldin 1979) *is included as an additional cluster identification method to support the choice of the optimal cluster number. This is one of the four methods described in Lee et al. (2023).*

3) *More qualitative WR comparisons with previous studies are added.*

__Below we address reviewer # 1's specific points.__

Author responses are in quotes.

1. Lack of Variance Normalization. I noted that previous work showed that the variance of 500 hPa height anomalies varies significantly across months (April variance is higher than July), and this is a reason for normalization.

"Whether the data should be normalized is an interesting question for debate." A more responsive response would be to check whether variance normalization impacts the results.

"the variance would be expected to be more uniform and less spread out around the mean." In original review I noted that Lee et al. (2023) shows this is not the case. Also, I don't know what it means for the variance to be spread around the mean.

One potential issue with the methodology in Lee et al. (2023) is that the anomalies are normalized by an area-averaged standard deviation with a cosine-latitude weighting. Since the standard deviation of geopotential height increases with latitude and the cosine-latitude weighting decreases with latitude, such normalization inflates the anomalies at higher latitudes, where the actual standard deviation is higher than the areal-averaged standard deviation. It contributes to the strong high-latitude loading of the WRs in Lee et al. (2023). We thus choose not to do the normalization.

We also want to point out that many studies on WRs for a specific season do not normalize the anomalies (Miller et al. 2020; Robertson and Ghil 1999; Vigaud et al. 2018a; Zhang et al. 2024). In particular, Zhang et al. (2024) used WRs to study west coast wildfires during June-October, and no variance normalization was done. The 5 CONUS WRs that we have identified are similar to WRs identified in Zhang et al. (2024).

*We justified our methodology in lines 133-137 in the new revision: "*Although geopotential height anomalies were normalized prior to the K-means clustering in the year-round WR analysis by Tippett et al. (2024) and Lee et al. (2023), 500H anomalies are not normalized in this study because we focus on one season, which is consistent with many previous studies (Miller et al., 2020; Robertson and Ghil, 1999; Vigaud et al., 2018a; Zhang et al., 2024)."

2. My first review: The new regime classification is not compared with previous ones from the same authors for April and May and with year-round regime classifications from Lee et al., (2023) [data is in Zenodo for download]. Making connections to previous work would increase the value of the current work. The classification data (data needed to classify independent data and classification of the days in the study) should be provided. I also asked: with what frequency are the classifications the same. No response.

A responsive response would have been to make a quantitative comparison of new weather regime classification with the previous ones. The data is available for that, and authors chose not to. They also have failed to provide the classification data. Code and a link to ERA5 data is not really very helpful.

Qualitative comparisons were made to Lee et al. (2023) and Miller et al (2020) in the last revision, but we don't think a quantitative comparison is necessary because i) our study and those studies focus on different seasons and/or geographic regions; ii) there are many previous studies on weather regimes, and a quantitative comparison with just two of them does not provide much added value.

On the other hand, additional comparisons are made in the revised manuscript to a recently published paper, Zhang et al. (2024). This study used seasonal WRs from June-October to study wildfires on the west coast, using a total of 5 clusters. Our WR spatial structures are very similar to theirs with only minor differences in long-term frequency.

*Lines 184-191 now state: "*The WR spatial structures closely resemble the WRs in Zhang et al. (2024) and Miller et al. (2020). WR-A is also similar to the Alaskan ridge pattern in Lee et al., (2023) and Tippett et al., (2024), and WR-D is similar to their Pacific ridge pattern. However, since our study focuses on a different region and a specific season, and is based on a different number of clusters, there are noticeable differences. In particular, the WRs in Lee et al. (2023) have a stronger loading in higher latitudes, probably partly because they normalized geopotential height anomalies by the area-averaged standard deviations with a cosine-latitude weighting, a procedure we choose to exclude."

Also, we respectfully disagree with the reviewer's comment that "Code and a link to ERA5 data is not really very helpful." We believe that sharing code ensures transparency in how the results are produced and enables users to easily reproduce our findings using the publicly available ERA5 data. Nevertheless, the WR label and spatial pattern data have been also made available in the GitHub site now.

3. "The 'once-per-day snapshot' approach was pursued because the chosen time (2100 UTC) of

500H represents a typical time of day when U.S. tornado outbreaks are ongoing." This makes sense for tornadoes but not for weather regimes which are supposed to be persistent features.

We have made this change and used daily anomalies in the revised manuscript, although using either a once-per-day snapshot or daily means does not qualitatively change any of the results or the physical interpretation. As shown in the two figures below (Figs. R1-R2), the analyses using the 21z vs daily mean show minor changes in long-term frequency of each WR, but the spatial structures remain the same and the modeling results are comparable.

*Lines 112-114 now read: "*Data from the ERA-5 reanalysis (Hersbach et al., 2020) were analyzed over the CONUS $[24-55^o \text{ N}, 130-60^o \text{ W}]$ at the native 0.25^o latitude $\times$ 0.25^o longitude resolution. This includes daily mean 500 hPa geopotential heights (500H)."

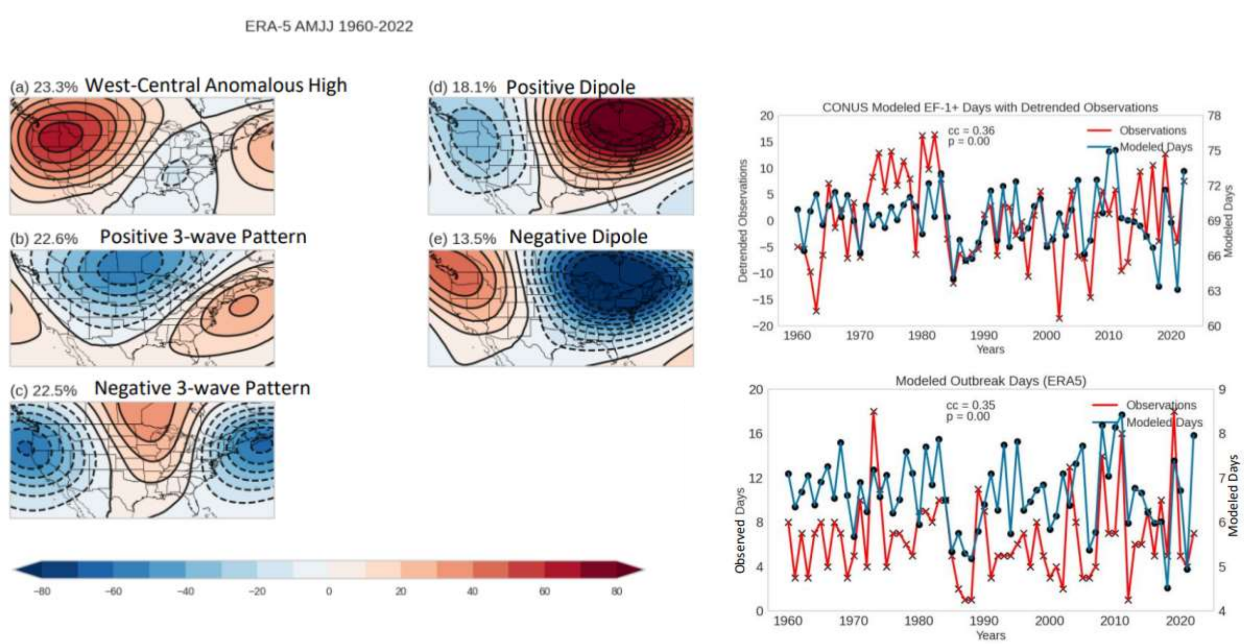

Figure R1: WRs and empirical modeling using 21z snapshot anomalies (from the original manuscript) and 5-day low-pass filtering.

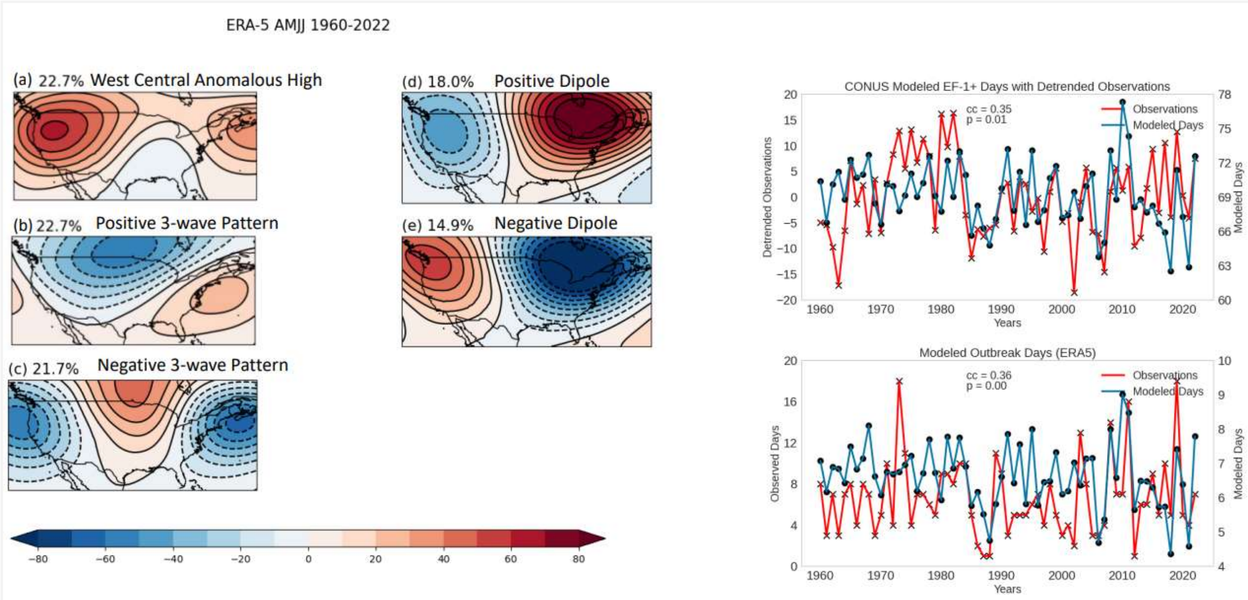

Figure R2: WRs and empirical modeling using daily mean anomalies and 5-day low-pass filtering.

4. Given that previous work with weather regimens has reported relations with ENSO and other modes of variability, the refusal to do so here is hard to understand. This added text is not particularly helpful in that regard.

“Furthermore, although not explored in this study, WRs and tornado activity may both be modulated by large-scale, low-frequency climate modes, with WRs potentially serving as the intermediate piece between large-scale climate modes and tornado activity, and the low-frequency modes may be important sources of predictability for the interannual variability of tornado activity.” This is speculation, and fails to acknowledge previous work that has already considered these issues.

Since our study focuses on the link between WRs and tornado activity, exploring the relations between WRs and various climate modes (including ENSO), or the sources of predictability, goes beyond the scope of this work. Our statement that the reviewer cited above is a reasonable hypothesis based on previous studies and a focus of our other ongoing study, which we plan to report in an upcoming manuscript. We believe that the current results, on their own, are worthy of publication.

Additionally, previous studies were cited about the impacts of various climate modes on tornado activity in the introduction of the manuscript, and more citations are now added in the revised manuscript:

Lines 400-406 now state: “Furthermore, although not explored in this study, WRs and tornado activity may both be modulated by large-scale, low-frequency climate modes (Cook and Schaefer 2008; Lee et al. 2023; Niloufar et al. 2021; Tippett et al. 2024; Vigaud et al. 2018). Given the potential predictability of WRs (Straus et al. 2007), they may 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."

5. In the first review I questioned whether the manuscript supported the abstract statement "Our study highlights the potential application of WRs for better seasonal prediction of tornado activity." In particular, I asked: Is there evidence that these regimes are predictable on seasonal time scales? The statement remains in the abstract, and the question of seasonal prediction was left unanswered by their response which talks about weekly timescales.

"First, Miller et al. (2020) showed that the hybrid model has skill better than climatology out to Week 3. Second, with increasing forecast lead times, the information of the predictand will be less specific. Miller et al. focused on weekly mean tornado activity, but one may focus on seasonal mean tornado indices for seasonal prediction. Applying these regimes to seasonal prediction is our ongoing research, which shows promising results and we hope to publish in due time."

The AGCM simulations carried out by Straus et al. (2007)*, forced by observed SST and sea ice, implies the predictability of weather regimes, which is also supported by our ongoing research. We prefer to keep this statement in the abstract as it reflects what motivates this study and where we anticipate our future study to go from here, but we have added more discussion in the last section of the manuscript, along with a new reference:*

*Lines 390-395 now state: "*Since the empirical model used WR frequency derived from the ERA reanalysis, its predictive skill can be considered an upper bound for this empirical prediction framework, assuming the perfect knowledge of WR frequencies. The atmospheric general circulation model simulations by Straus et al. (2007), which were forced by observed SST and sea ice, suggested the predictability of WRs, thus indicating the potential value of this approach."

*lines 403-406 now state: "*Given the potential predictability of WRs (Straus et al. 2007), they may 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 hybrid model results shown in Miller et al. (2020) showed promising results on the subseasonal timescale, and now our study is attempting to expand to the seasonal timescale. A hybrid model for seasonal tornado prediction has not yet been investigated, hence why the potential we refer to in the manuscript remains an open question. Our study provides evidence that this is a project worth to continue exploring, and we will present more results in due time.

6. In the first review I said: The weather regime classification method here lacks standard diagnostics and assessments of robustness. There is no mention in their response of diagnostics or tests for robustness. There are multiple common diagnostics for weather regime calculations that have been used previously.

"As explained above, what accounts for the "standard" regime classification is controversial. In particular, Falkena et al. (2020) argued against the use of either EOFs or time filtering on top of K-means clustering because K-means clustering reduces the dimensions and is a form of data filtration. We have made our weather regime methodology code available"

We have added the Davies-Bouldin Index, discussed in detail under point 7, as evidence that we have chosen the correct # of WRs.

We now apply a 5-day low-pass filter and use the first 8 eofs for EOF dimension reduction as requested by the reviewer. All figures and in-text numbers have been changed to address the new WRs, but the physical interpretation of our results remains largely the same.

*Lines 137-140 state: "*A 5-day low-pass filter was applied to 500H anomalies, and the leading 8 EOFs, accounting for ~90% of the variance, were retained in the EOF dimension reduction; such pre-processing does not qualitatively affect the regime patterns or the regime frequencies (Figs. S2-S4) but does facilitate comparison with previous studies."

Regarding robustness, we tested various analysis approaches for data pre-processing as requested by the reviewer, and they all yield very similar results, which supports the robustness of our analysis. In addition, the relationship between WRs and environmental parameters, such as CAPE, VWS, and precipitation, corresponds well with the WR-tornado link. Such process-level analysis helps further confirm the robustness of our results.

Figs R1-R5 show that the WRs and modeling results undergo little change with whatever methodology is chosen. Fig. R5 shows the WRs that will be used in the new version of the manuscript. All statistics in the manuscript text have been changed to go with the results of the Fig. R5 WRs.

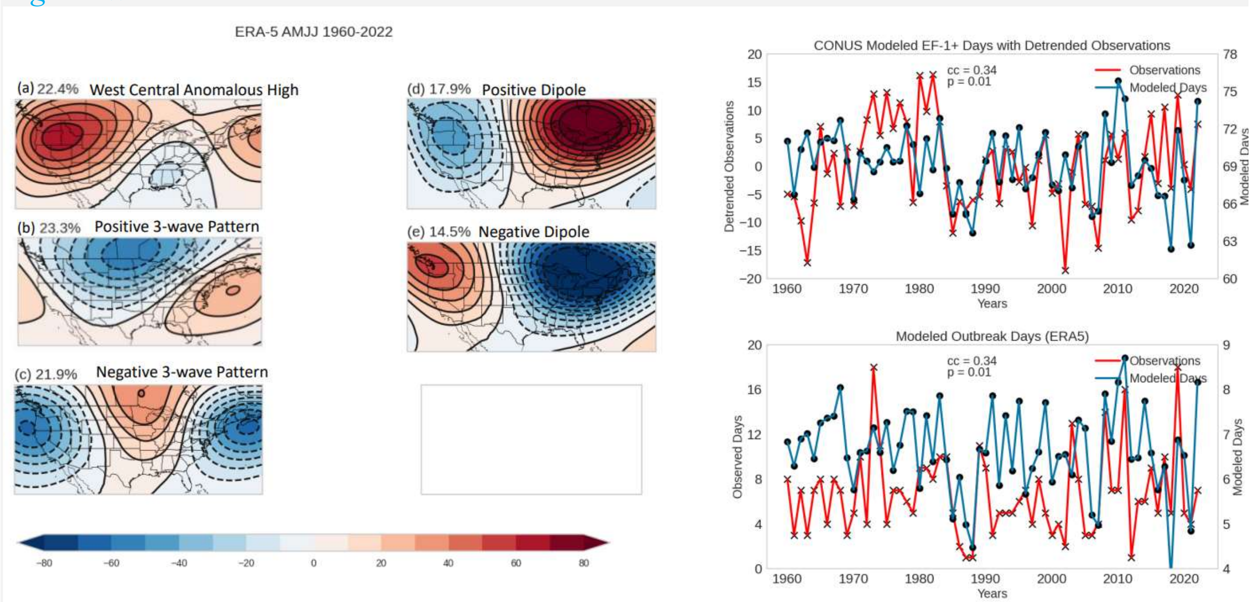

Fig. R3: 21Z WRs without any filtering.

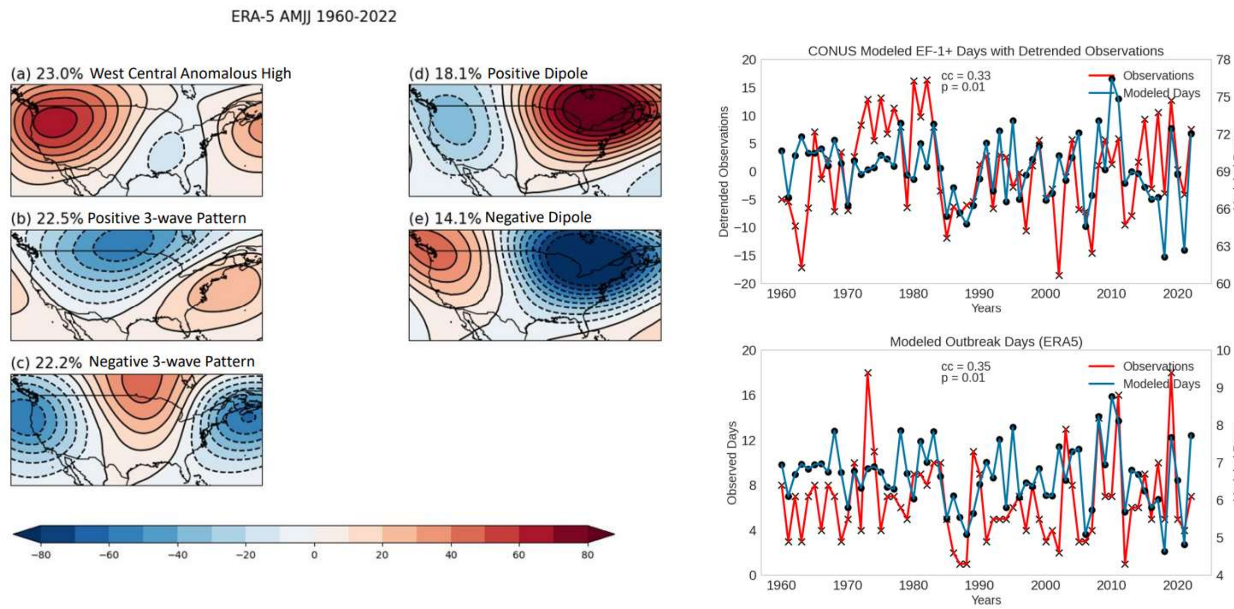

Fig. R4: 21z WRs using a 5-day low-pass filter and the first 8 eofs which account for ~90% of the variance.

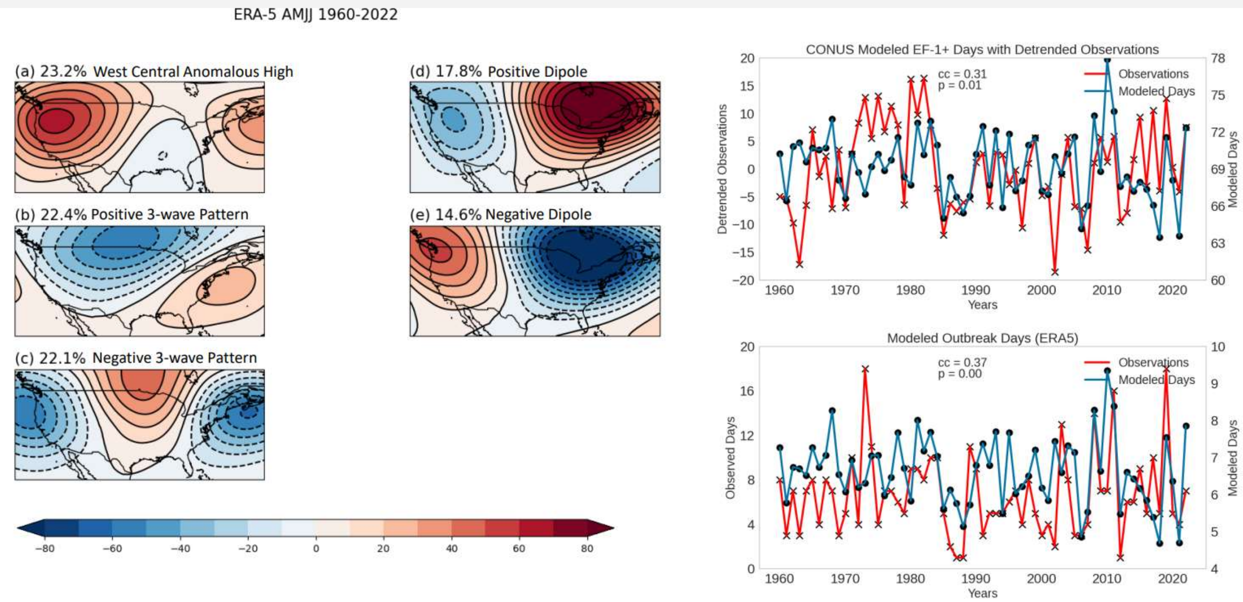

Fig. R5: Daily Mean WRs using a 5-day low-pass filter and the first 8 eofs which account for ~90% of the variance.

7. The "elbow method" is not an objective method for choosing the truncation. I noted that: Lee et al., (2023) apply four objective, data-driven methods for determining the best number of clusters, including the classifiability and reproducibility indices of Michelangeli et al. (1995). Again the authors have chosen to ignore that comment and to apply no such objective diagnostics to their classification.

We have added the Davies-Bouldin Index (Fig. R6), which is one of the classification methods in Lee et al. (2023), and this shows that 5 clusters are indeed the optimal number of clusters. As stated in Lee et al. (2023) and explained fully in Davies and Bouldin, (1979), this method measures the average similarity between clusters, and when the index value reaches a minimum point, adding more clusters makes it harder to distinguish the patterns in each WR. For the purposes of our study, 5 clusters are an appropriate number and goes with previous work (Miller et al. 2020, Zhang et al. 2024). The plot shown is for WRs using daily mean 500H anomalies, as now done in the manuscript.

Lines 142-143 now mention use of both the elbow method and the Davies-Bouldin Index.

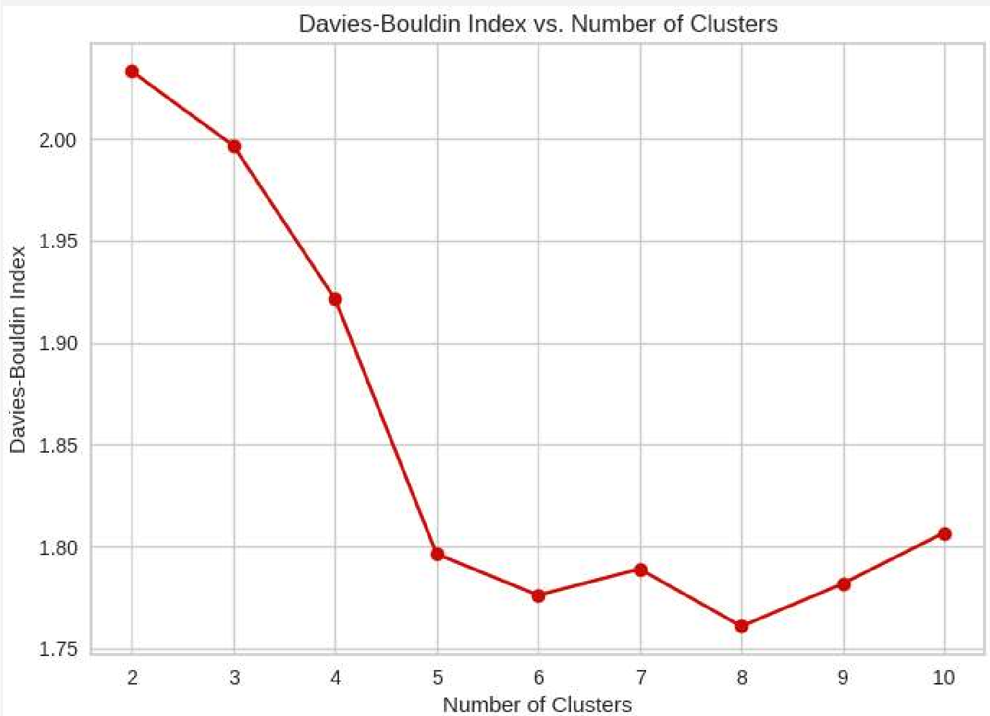

Fig. R6: Davies-Bouldin Index for each number of clusters

8. In my first review I said: Overall there are essentially no diagnostics of the WRs such as variance explained. There is no indication in the response that the authors have added any diagnostics including variance explained.

In our original analysis, since total anomaly field is used, the WR explains 100% of the variance. We have now employed the EOF dimension reduction in the revised manuscript, and the first 8 eofs account for ~90% of the total variance.

9. In my first review I questioned whether their statistical tests accounted for seasonality. Again I see no response to this question. I assume that seasonality was not accounted for, despite the seasonality of the data. Removal of the mean is inadequate in this regard because sampling variability depends on variability.

One potential issue with the methodology in Lee et al. (2023) is that the anomalies are normalized by an area-averaged standard deviation with a cosine-latitude weighting. Since the standard deviation of geopotential height increases with latitude and the cosine-latitude weighting decreases with latitude, such normalization inflates the anomalies at higher latitudes, where the actual standard deviation is higher than the areal-averaged standard deviation. It contributes to the high-latitude strong loading of the WRs in Lee et al. (2023). We thus choose not to do the normalization.

We also want to point out that many studies on WRs for a specific season do not normalize the anomalies (Miller et al. 2020; Robertson and Ghil 1999; Vigaud et al. 2018a; Zhang et al. 2024). In particular, Zhang et al. (2024) used WRs to study west coast wildfires during June-October, and no variance normalization was done. The 5 CONUS WRs that we have identified are similar to WRs identified in Zhang et al. (2024).

10. I asked whether the correlation reported in the original S4 figure was misleading because there are fewer tornado days in a particular weather regime in precisely those years when that weather regime itself is less frequent. The response seems to be yes, it is misleading but "we feel this figure is useful as it demonstrates the strong variability of weather regimes and its influence on tornado activity." This statement is patently false because the correlation mainly reflects the regime frequency, not the association of the regime with tornado activity, a fact that even the authors acknowledge when they say in the response (but not in the manuscript) "the correlation is expectedly high."

The main purpose of Figure S4, now Figure S8, was to show the strong interannual and decadal variability of WR frequencies. We added in the tornado day observations curves as a supplement. The TD observation curve and subsequent correlations have now been removed from this figure to address the reviewer's concern. The plot now includes a trend line of the seasonal frequency.

Lines 314-319 now state: "In this section, we further quantify the link between WRs and tornado activity. WR frequencies demonstrate strong interannual and decadal variability (Fig. S8a-e). In particular, WR-A exhibits a frequency increase during the 1980s coinciding with the steepest decrease in TDs (Brooks et al., 2014; Graber et al., 2024). The increase in seasonal frequency in WR-A is consistent with the spatially similar ridge-trough-ridge WR in Zhang et al., (2024). The frequencies of persistent WRs also show changes across different multidecadal time periods (Fig. S8f)."

Additional minor edits were made to the manuscript that were not in response to the reviewer:

Figure captions have been modified to fully reflect what each figure is showing including statements on use of t-tests for significance testing, 2º (latitude) x 2º (longitude) uniform filtering for the convective anomalies, and a gaussian filter for the TD probability anomalies in Figure 2.

This includes an added statement to the methodology on the uniform filtering of convective parameter anomalies.

*Lines 121-123 now state: "*A 2º (latitude) x 2º (longitude) uniform filter was applied to MUCAPE, CP, and S06 anomalies to coarsen the data and were tested for significance using a one-sample, two-sided t-test."

*Lines 225-227 now state: "*Here TD probability anomalies are evaluated following Eq. 2 with respect to P_c at each grid point and then smoothed using a scipy gaussian filter with sigma 6. Such smoothing has removed some small-scale anomalies but retained the large-scale patterns."

Figures 1 and 2 have been modified to make them easier to read.

Figure 6, change in MUCAPE and CIN was removed from the main manuscript and was placed in the SI. The new figure includes smaller changes in S06 superimposed on the CAPE plots. A brief description of the figure is still included in the manuscript to explain a possible limitation of the model.

*Lines 351-355 now state: "*Additionally, convective inhibition (CIN) increases in the Southeast and Midwest for WR-B (Fig. S11h) and in the central-CONUS for WR-C (Fig. S11i) from P1 to P3. Further analysis reveals a lower TD probability for all WRs in P3 than in P1 (not shown), consistent with the negative trend of TDs (Graber et al. 2024)."

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