Comment: In this analysis the authors evaluate day of week patterns in average O3 and O3 exceedance days for the 2002-2019 timeframe at ~50 different high O3 sites in the U.S. The authors identify several different patterns including disappearing weekend-weekday differences and others. The analysis mixes observations and model results, which I find to be somewhat problematic. In addition I have some statistical concerns that would need to be corrected or clarified before this could be published.

Response: See responses to specific comments below

Comment: The authors mix observed patterns with model patterns in a way that I believe is misleading. I think its incumbent on the authors to first clearly document what the observations show. Then we can ask how well the model reproduces the observations and what we can learn from the model where it is consistent with the observations, or if not consistent, then why. Certainly there are plenty of NOx observations that could have been used for this work (see Jaffe et al 2020), so I am not sure what is gained by showing and using only the modeled NOx. But NOx is not that big of a concern. Its formaldehyde that I find much more problematic. For formaldehyde, we have much poorer understanding of emissions and chemistry, both of which are essential to understanding the concentrations. Without any evaluation of the modeled formaldehyde, these results should be removed. In other places the authors quote both modeled trends and observed trends and appear to put equal weight on these. That is incorrect, in my opinion, for the reasons stated above.

Response: Thank you for this comment. We will address the comment in regards to NO_x and Formaldehyde analyses separate from ozone analysis.

We start by looking at NO_x and formaldehyde day-of-week patterns to confirm whether the ozone precursor concentrations follow the expected decreases on weekends that would be necessary to drive an ozone day-of-week effect. We explain in methods that due to sparsity of monitoring data we chose to use modeled values for this analysis. However, based on the reviewer comment we have added a new analysis to the supplemental information (Figure S-1 through S-26) that compares modeled and observed NO_X DOW patterns and trends in locations where measurements are available. We note that this does not include all nonattainment areas from this analysis and within nonattainment areas it does not include all locations with ozone monitors. However, the new figures in the supplemental information show that the model does a reasonable job of representing the NO_X DOW patterns and trends across these areas. We were unable to add a similar analysis for formaldehyde because until very recently most formaldehyde measurements occurred at NATTS and urban air toxics monitoring networks (https://www.epa.gov/amtic/air-toxics-monitoring-national-program-reports) which have 1-in-6 and 1-in-12 day sampling schedules which does not provide sufficient data frequency for DOW analysis. In recent years PAMS has required HCHO at 1-in-3 day interval during June-Aug but most sites did not start meeting these requirements until between 2017 and2019. We have clarified throughout section 3.1 that results represent modeling rather than observed data and have added the following text to the beginning of section 3.1 to clarify these points:

We first look at modeled NOx and formaldehyde day-of-week patterns to better understand how daily changes in precursor emissions impact modeled day-of-week ozone patterns. We chose to focus on modeled data here because of the ubiquitous spatial and temporal coverage provided in the model for these pollutants allowing us to evaluate these pollutants on the same days and at the same locations as the ozone monitors. We note that some observed NOx data can also be used for this purpose, although the available NOx data are not available for all nonattainment areas and are not available at the

locations of all ozone monitors even within nonattainment areas with some NOx monitoring data. A comparison of monitored and observed trends in NOx day-of-week differences provided in Figures S-1 through S-26 show that the model does reasonably well at capturing the patterns in the limited observational dataset that is available. Due to the sparsity of formaldehyde measurements, both spatially and temporally (formaldehyde is commonly measured at a 1-in-6 day or 1-in-12 day frequency), a similar comparison cannot be made for modeled and measured formaldehyde. However, with more recent requirements for formaldehyde measurements at Photochemical Assessment Monitoring Stations (PAMS) locations starting in the 2017-2019 time-period future assessments may have additional measured formaldehyde data that could be used for this purpose.

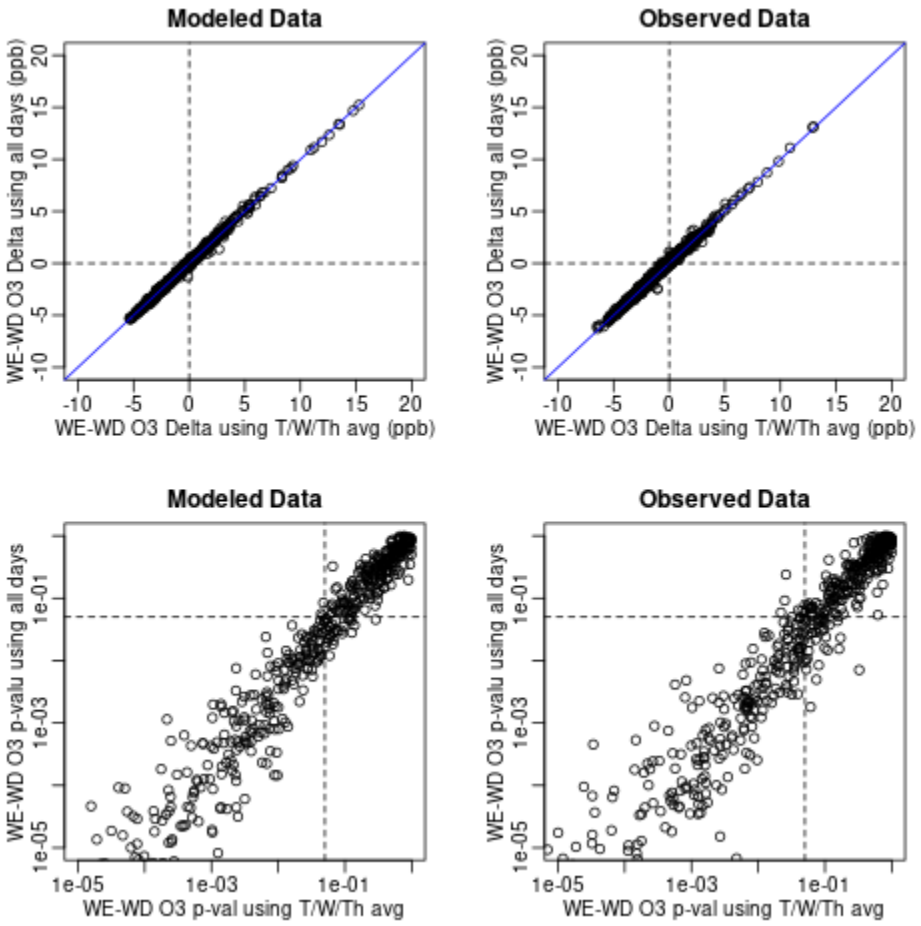
For the ozone analysis, we think it is appropriate to present the observational results side-by-side with modeled results. We are always clear on which results come from the monitoring data versus the model and do not believe this is misleading in any way. Rather we think that the current structure allows for better flow of the manuscript and highlights which aspects of the observed trends the model captures and which it does not.

Comment: I believe the authors may over-state some of the statistical significance due to auto-correlation. This could be true for both the t-tests for individual years and the trends, which use 5-year running means.

Response: Thank you for this comment. We agree that the auto-correlation is an issue for the Theil-Sen trends using the 5-year rolling time periods. This is a challenge because the single-year values are subject to meteorological and other random effects which are minimized by using the five year rolling windows. We do believe that the trends shown by the Theil-Sen method show real changes over time as they are consistent with patterns shown in Figure 10 which compares WE-WD differences in 2002-2005 vs 2015-2019 which do not have any overlapping data points. However, we think it is important to acknowledge this weakness and are thus de-emphasizing any language implying "statistical significance" of trends and rather are simply reporting P-Value ranges which still provides some indication of which areas have stronger trends. We note that reporting P-values while de-emphasizing "statistical significance" is also consistent with the recommendations of Reviewer 1 and the approach taken in TOAR. We have added the following language to the methods section to acknowledge this issue: "*Because we use a 5-year rolling window for each area, the individual data points in the trends analysis are correlated. While this should not systematically bias the calculated slopes, it will lead to lower P-values and narrower 95% confidence intervals than would be calculated if the data points were uncorrelated. However, the P-value is still informative to characterize which areas have the strongest trends. Therefore, while we do report P-values we do not rely on a strict threshold for determining statistical significance.*"

We developed an additional analysis that demonstrates auto-correlation is not an important issue for the individual year t-tests. The t-tests for individual years compare T/W/Th weekday ozone to Sunday weekday ozone. The most important autocorrelation issues would occur on the transition days (i.e. Mon, Fri, Sat) and would degrade the statistical significance, which is why those days are excluded from the WE-WD difference calculation. To show that the T/W/Th correlation does not impact our results, we performed a second analysis in which we used the average of O3 on T/W/Th of each week rather than individual T/W/Th O3 values to compare against Sunday O3. By averaging T/W/Th (instead of pooling), we create weekly data points days that would be more independent. The figure below shows the

individual year t-test results from the original analysis compared to the analysis using a T/W/Th average and demonstrates that results do not meaningfully change. We therefore have chosen to retain the original method which used all T/W/Th O3 values. It is important to note that we cannot perfectly eliminate autocorrelation (e.g., monthly), but longer term autocorrelation would degrade statistical significance because it would manifest as a residual component of both weekday and weekend.



Comment: Finally I note that this has a lot of overlap with our earlier analysis (Jaffe et al 2020). We used data for 1995-2020. This analysis uses data for 2002-2019. There are some modest differences, but overall the results are quite similar. I think its essential that the authors clearly describe what is new and/or whether these results are consistent with the earlier analysis. One area that is different is use of probability of exceedance vs mean concentration. The authors seem to want to discount any differences as being due to random variability, but I am not sure that is true. One focuses on the highest days and the other approach focuses on all days in the O3 Do these days have the same VOC-NOx sensitivity?

Response: Thank you for this response. We think there are multiple important novel aspects of our work compared to the analysis presented by Jaffe et al (2022). First, in our work we endeavor to examine areas individually and highlight nuances in behavior driven by local factors while Jaffe et al (2022) mostly provided national-level results. We agree that the "transitioning chemical regime" trend (formally called the "disappearing weekend effect") is broadly consistent with the national results

reported by Jaffe et al (2022) and have added a statement acknowledging this work in the first paragraph of section 3.2.1. The "disappearing weekday effect" trend that we report in rural/agricultural areas of California was not identified in Jaffe at al (2022) and we are not aware of it being reported anywhere in the literature. Jaffe et al focused their analysis on areas with NO_x measurements and many of the areas displaying this trend type do not have NO_x monitors and were not included in the Jaffe et al analysis. Additionally, we examine local features that have led to no trends in DOW patterns in some nonattainment areas. We believe that the local analysis of O3 DOW patterns in individual areas is a key, unique aspect of our analysis that has allowed us to better understand the varied local factors leading to different trends in different areas. A further unique factor of our work is that we evaluate trends using both a mean metric and the percentage of exceedance days metrics to show that trends are broadly consistent across not only high ozone days but also when looking across the entire ozone season. Finally, Jaffe et al focused on observed data while the inclusion of both modeled and observed data allows us to evaluate the skill of the CMAQ model at capturing these patterns of changing ozone chemical formation regimes which has important policy-relevant implications since many regulators use CMAQ modeling as part of planning for ozone control strategies. Demonstrating the skill of this model builds confidence in our ability to use it as a tool for this purpose. The model additionally allows us to better characterize drivers of observed trends since we are able to probe the model in locations where there are no measurements available. For instance we can confirm that the trends in DOW O3 patterns in the model are occuring coincident with expected trends in modeled NOx and formldehyde ozone precursors.

Comment: Abstract: It is important in abstract to describe the scope: All US O3, all US urban areas or all US non-attainment areas. What years? How many regions considered? In addition, I am unclear what it means if you have a "disappearing weekday" effect. The information here is contained in the relative O3 and NOx behavior between weekday and weekend. So the terms "disappearing weekday" is confusing.

Response: The abstract does include the scope of the analysis "across US nonattainment areas" (we have added "51" before "US nonattainment areas" to clarify the large number of areas analyzed) and also include the years of analysis and the metrics. We do not break out our results by region but rather report results from individual areas so regions are not mentioned in the abstract. We agree that the term "disappearing weekday effect" was confusing and have revised the name of this trend type to "transitioning chemical regime" to better convey that negative slopes in WE-WD ozone represent areas that are transitioning from VOC-limited conditions to NOx-limited conditions.

Comment: Line 25-26: "both datasets" ?

Response: We have updated the language to clarify this is using both observed and modeled data.

Comment: Line 27: The abstract uses area names that are consistent (I think) with EPA designations, but are often rather non-intuitive. For example, Los Angeles – San Bernardino County vs Los Angeles – South Coast. The San Bernardino monitors are in Riverside CBSA, so aren't these two locations essentially same region. It would be more interesting to include a site closer to downtown LA like Azusa, where we might expect a different pattern.

Response: We think that using the official area names and delineations consistent with EPA nonattainment designations provides a consistent framework with which to distinguish areas. Within

each nonattainment area we include data from all available monitoring site locations. The Azusa monitoring data is included in the Los Angeles South Coast nonattainment area.

Comment: Line 33: It is not clear what model evaluation for this work. As near as I can tell, nothing was shown about the models ability to capture year-year variations. The model does seem to capture the trend in weekend-weekday differences.

Response: We include multiple comparisons of the model's prediction of ozone (and now NOx) DOW patterns and trends with observed patterns and trends. These comparisons are provided in every figure within the paper. The term "year-to-year variations" captures both the trends in DOW differences and the areas in the Ohio River Valley region of the country without trends but with multi-year meteorology-driven patterns (i.e. Figures S-28 and S-29). We additionally include plots showing model evaluation for ozone across years, seasons, and regions of the US in Figures S-40 and S-41.

Comment: 132-135: While I understand why you excluded 3 out of 7 days, does this change the results?

Response: Due to concerns with autocorrelation between days that this reviewer has brought up, we did not evaluate how including M, F, and Sat would impact the results.

Comment: 144: Not clear how t-tests were done. I think you took every weekday and weekend day in one year and compared the means and treated each day as an independent observation. If this is right, then I don't think autocorrelation was taken into account. In any case please clarify how the t-tests were performed.

Response: Yes, your understanding of how the t-tests were performed is correct. As shown in our response above, we conducted a sensitivity analysis using the mean of T/W/Th for each week and found results did not change. We have added the following sentence to the methods clarifying the t-test calculation: "Within each nonattainment area, the t-test calculation compared the means of every weekday and every weekend day in a 5-year window, treating each day as an independent observation."

Comment: 169: Given the 5 year running means, these values will have sig autocorrelation. Was this taken into account in the results?

Response: As explained above, due to the autocorrelation issue and comments from Reviewer #1 we have decided to de-emphasize the use of strict P-value thresholds for determining statistical significance and have added the following discussion of limitations to the methods section: "Because we use a 5-year rolling window for each area, the individual data points in the trends analysis are correlated. While this should not systematically bias the calculated slopes, it will lead to lower P-values and narrower 95% confidence intervals than would be calculated if the data points were uncorrelated. However, the P-value is still informative to characterize which areas have the strongest trends. Therefore, while we do report P-values we do not rely on a strict threshold for determining statistical significance."

Comment: 173-174: Unclear meaning.

Response: We have added the following sentence to more fully explain the meteorology analysis: "Meteorological parameters were similarly compared across weekends and weekdays, matching times and locations of the ozone analysis and using the same statistical methods for comparison."

Comment: 193-194: Unclear meaning.

Response: We have modified this sentence for clarity as follows: "While the model does not predict substantial day-of-week formaldehyde differences in most areas, there are small modeled formaldehyde enhancements on weekdays compared to weekends in some areas such as Chicago (Figure S-28)"

Comment: Figure 1: Please clarify meaning of P values in top right plot. I think these are for each individual year, correct? Given that the NOx and CH2O plots are for all years, not sure what is the value in showing these. There are major differences (for NOx) between the early and later part of the data record.

Response: The p-values come from the t-test results for each 5-year window to show whether the WE-WD differences are statistically different from zero. We have added a sentence clarifying this to the caption of each figure.

Comment: 234: As noted above, the terminology "disappearing weekend effect" is very misleading. Its really about the difference between weekend and weekday values.

Response: Thank you for this comment. We have updated the term used for this trend type to "transitioning chemical regime" to better convey that negative slopes in WE-WD ozone represent areas that are transitioning from VOC-limited conditions to NOx-limited conditions.

Comment: 258-259: So how do we interpret these model obs differences? You may say the random variations impact the obs more than the model, but aren't these variations important?

Response: Figures 10 and 11 show that the model generally captures the DOW O3 trends across most areas although the model does not perfectly simulate the patterns in every case.

Comment: 283: I don't think the probability approach is inherently noisier, especially when averaged over several years as you have done. I think this is an interesting spot to do a deeper dive.

Response: We initially based the statement on the observation that there were more areas falling into the "no trend" category for the probability approach. However, we have taken a closer look at the results and agree that the probability approach results do not look inherently noisier in the observations although the model has less skill at replicating this behavior. We have deleted this sentence.

Comment: Conclusions: As noted above it would be good to understand what is new here. Please add some discussion to clarify, perhaps focusing on the differences between the prob of an exceedance approach and mean O3 approach.

Response: Within the results section, we now note where our results are in broad agreement with Jaffe et al (2022). We also note in the section discussing the "disappearing weekday effect" that we are not aware of this being reported anywhere in the literature.

Comment: Finally, I note that the regression information in the right plots of figures 1-9 (not 5) is almost impossible to read.

Response: We have removed this text from the plots. Readers can find now find this information in Tables S-1 and S-2.