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Interactive comment on "Summer ozone in the Northern Front Range Metropolitan Area: Weekend-weekday effects, temperature dependences and the impact of drought" *by* Andrew A. Abeleira and Delphine K. Farmer

Anonymous Referee #2

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The authors investigate O_3 trends in the Northern Front Range Metropolitan Area of Colorado, a region which has exhibited ongoing issues with O_3 exceedances in spite of significant reductions in NO_x emissions. In addition to examining overall trends over time, the authors use weekday/weekend comparisons of NO_x and O_3 to help explain features of local chemistry, and also compare O_3 vs. temperature over time. Overall this paper is clear, well-organized, and represents a solid, if incremental addition to the existing air-pollution literature. I recommend publication, following improvements in a few areas.

1. First, and most importantly, I have concerns over the authors' use of binned temperatures as a preliminary step to linear regression. While I understand that this methodology has been utilized for similar purposes in the past, there are clear statistical flaws related to the practice that should be addressed before these results can be considered robust. Specific issues in the context of this paper include the following:

    - At relatively small sample sizes (n = 64-92 per summer), terms such as "95th percentile" become somewhat problematic. Dividing this already thin sample size into even smaller 3°C temperature bins must have, I assume, resulted in some bins with observations in the single digits. What methodology was used to determine percentiles from such small sample sizes?
    - By choosing uniformly spaced bin widths (years, in the case of this paper's temporal analysis, and uniform 3°C temperature widths in the case of the O_3/T comparisons) information regarding sample sizes within each bin is lost completely. A bin containing more observations clearly should be weighted more heavily than a bin with fewer, but as written I see no indication that this kind of weighting was performed. This issue will be especially consequential for the temperature bins, since the relatively sparse temperature extremes will be incorrectly given weights equal to those of the middle bins, most likely exaggerating the resulting slopes. See Wasco and Sharma, 2014 for a description of how evenly spaced bins can produce exaggerated slopes as a result of this bias. Two methods that could correct this bias are equal number bins (with variable temperature widths based on the frequency distribution) and quantile regression (Koenker and Bassett, 1978). I think either of these would be superior to the current "equal distance bin" approach, with quantile regression also having the benefit of simultaneously addressing the small sample size issue.

    Wasko C, Sharma A. Quantile regression for investigating scaling of extreme

precipitation with temperature. Water Resour Res 2014;50:3608–14.

Koenker R, Bassett Jr G. Regression Quantiles. Econometrica 1978;46:33–50.

Further examples of this technique applied specifically to similar air-quality questions may be found elsewhere in the literature.

2. Figure 6: While I appreciate the attempt to use many symbols to distinguish years, I think the end result just doesn't work. The dense area around 10 ppb NO_2 in particular is nearly impossible to interpret easily. I suggest either abandoning the symbols entirely, and using shaded dots to represent different years, or else zooming in on the data to create more whitespace in this concentrated region.

3. The usage of "standard deviation" in several figure captions seems unclear. For example, on Figure 9 it seems to suggest that this is a standard deviation of many regression slopes. Is this the standard error of a single regression? Was bootstrapping performed, leading to many regression coefficients?

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