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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The weekend effect within and downwind of Sacramento: Part 1. Observations of ozone, nitrogen oxides, and VOC reactivity" *by* J. G. Murphy et al.

Anonymous Referee #1

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Review of Murphy et al., "The weekend effect within and downtown of Sacramento: Part 1. Observations of ozone, nitrogen oxides, and VOC reactivity", ACPD-2006-0349.

General comments:

This is a very good presentation of data and is one of the best analyses of the California weekend effect in ozone that I have seen. With some corrections as suggested below, this will be a nice contribution to ACP and especially to the improved interpretation and use of ambient data in emissions control policy decisions regarding the weekend effect in CA. Well done.

Specific comments:

Abstract.

p. 11428, line 10: Please define NOx here in the abstract, in addition to the definition in the introduction.

2. Air quality in the Sacramento region and downwind.

p. 11433, line 5: “Ědivided the region into two distinct air basinsĚ” the distinction is not clear from the map in Fig. 2, but is shown well in the map of Fig. 4. Perhaps combine the two into Figures 2a and 2b, calling out more locations (see comment below on Fig. 4) as well as showing topography to identify the two distinct air basins? Flow patterns could be included in fig. 2a as well.

p. 11434, line 29: “Ě yet the region clearly had an existing air quality problem by the time routine monitoring at these sites began.” Undoubtably a true statement, but undemonstrated in this paper (or any other) due to the unfortunate lack of data. Perhaps “clearly” states this too strongly? Rewording this as an inference might be appropriate.

3. Regional meteorology and transport.

p. 11435, lines 26 ff. Place names are unfamiliar to non-residents. Please identify locations of the Carquinez Pass, San Joaquin Valley, and the location of Sacramento on the map in Figure 4.

p. 11436, line 4: Fig. 5 is small and difficult to read, but conveys a lot of relevant information so could benefit from some improvements. I would suggest locating the wind roses on a simple map of the area, showing their measurement locations, and making the individual roses as large as possible. Most figures in this draft could be expanded for legibility.

4. Timing of peak ozone.

p. 11437, line 12: “Ě not available upwind (>15 km south) of that site.” From the data

ACPD

6, S5976–S5980, 2007

presented in the wind rose for Elk Grove in Figure 5, I would expect the predominant upwind direction would be southwest of this site during the day?

p. 11437, line 15: “Even on daysĚ reduce the effects of titration from local NO sources.” This assertion seems to warrant a citation, or more explanation in the text at this point. It is addressed later in the draft, but lacks support at this stage of the development.

p. 11437, line 20: “The timing follows a geographical trend where the sites closer to the city have earlier maxima.” Understandable but a little awkward. Perhaps reword along the lines of “the timing of the maxima are determined by transport time, therefore distance, from the urban source”? That’s not much better though.

p. 11438, line 2: “Days on which peak ozone concentrations [for the MC sites] occur in the early morning hours can be roughly equated with days when the urban plume did not reach the site later that day.” Presumably these peak concentrations would be relatively low, as well. If so, noting this might make the point more clearly.

p. 11438, line 6: “Thus improvements to air quality in the SV and MC are likely to require controls on the emissions of urban precursors to ozone production.” Not sure what point needs to be made about this - goes without saying? Minor comment though.

p. 11438, line 14: s-v agreement - “Ěcan depend on which data are chosenĚ”

p. 11439, line 3: Please include 1-sigma variances shown as error bars for the data shown in Figure 6.

p. 11439, line 9: “Jackson Ě likely receives anthro. emissions from I-5 and possibly Stockton about 50 km to the west.” Not quite clear that this is the case, solely based on the wind roses and assertion in the text. Trajectory calculations should be run to support this conclusion.

6. Day-of-week trends in O3, NOx and VOC

p. 11439, lines 21 ff.: Referring to data shown in Figure 7b, “Ě along with three

ACPD
6, S5976–S5980, 2007

Interactive Comment

EGU

other sites [Big Hill] has lower ozone on Saturday than Friday." and "Ĕ most [Mountain County] sites have lower ozone on Monday than on Saturday." The stated 95% confidence intervals for the Mountain County ozone data range from 2 to 3.5 ppbv, as noted in the Fig. 7 caption. Given the presentation of the MC data in Fig. 7b, I am not convinced that the assertions quoted above are statistically significant for MC sites; for instance, the Friday and Saturday data for Big Hill seem to be well within 3 ppbv of each other. The Cool site appears more urban in nature, as its Saturday value is higher than for Friday. While the other MC sites do have lower weekend values, they should be shown to be significantly lower by statistical tests before generalizing. My impression from Fig. 7b data is that some MC sites will pass, and some will fail, a statistical test for weekday/weekend differences, which appear to be very small in any case. As presented, the conclusion from these data is not fully convincing yet.

p. 11440, line 3: The symbols in Fig. 8 captions have printed incorrectly.

p. 11440, line 12: "NO Ĕ is estimated here using measurements of NO2 and the relationships described in R1-R3." It's not clear how this was done without measurements of jNO2 and assuming photostationary state. Please clarify.

p. 11440, line 22: " ... the inlets on these instruments are not configured to sample HNO3 effectively." More information on the inlets would be appreciated. Even a metal inlet would pass some amount of HNO3, which would then contribute to bias in the NO2 data.

p. 11443, line 6: "Ĕ measurements from the Del Paso site were used, which likely overestimate COĔ" Why would they overestimate CO?

7. Differences in odd oxygen partitioning.

p. 11445, line 6: "This suggests that [sic] monitoring sites located near major transportation routes are potentially underestimating the regional abundance of ozone, and should include NO2 measurements in order to capture the true concentration of odd

ACPD
6, S5976–S5980, 2007

oxygen and guide policy decisions." This an excellent summary of the situation, is the most important point of the paper, and can't be emphasized strongly enough. An alternative presentation of the odd-oxygen data also suggests itself. Titration of O3 by fresh NO emissions, in the absence of subsequent chemistry, leads to a linear anticorrelation of O3 with NO2 with slope of -1. A scatter plot of O3 versus NO2 data, from a monitoring site expected to most clearly show the titration signature, might complement the Ox data presented in Fig. 11 and illustrate this point more clearly. A suggestion only, left to the authors' discretion.

8. The role of aerosols in the ozone weekend effect.

This section seems lost in the rest of the discussion. The authors present no data to either clearly support or undermine the hypothesis stated in the introduction, and acknowledge only that more measurements are needed. I'd suggest deleting this two-paragraph section entirely, as it is a distraction from an otherwise very nice paper.

9. Conclusions.

p. 11447, line 15: suggest changing this to "Ĕhigher on the weekend (negating hypothesis #2)."

p. 11448, final paragraph beginning on line 12: This concluding paragraph reads like an introduction to, or advertisement for, part 2 of this work. As such, it's a bit out of place here, and should be deleted.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 11427, 2006.