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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.

J. G. Murphy et al.

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Response to Referee 1

We thank the referee for his/her kind words and constructive suggestions for improving the manuscript. Our author comments (AC) in response to the most significant referee's comments (RC) follow below.

Specific comments:

RC: Define NOx in the abstract.

AC: Done

2. Air quality in the Sacramento region and downwind.

RC: p. 11433, line 5: " divided the region into two distinct air basins " 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.

AC: In the revised manuscript we will revise the maps and figures as suggested.

RC p. 11434, line 29: the referee suggested the word "clearly" referring to an inference about the air quality in the years before measurements were available was too strong.

AC: We agree and have replaced "clearly" with "likely" in the revised manuscript.

3. Regional meteorology and transport.

RC 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.

AC: Done as suggested.

RC 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.

AC: Done as suggested.

4. Timing of peak ozone.

RC: p. 11437, line 12: The reviewer points out the wind direction was incorrectly denoted south.

AC: We have changed the text to reflect the southwesterly origin of the winds.

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RC: p. 11437, line 15: “Even on days Ĕ.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.

AC: We have added reference to other locations where these effects have been discussed and indicated that they are described in more detail later in the paper.

RC: p. 11437, line 20: The reviewer found the sentence awkward.

AC: In the revised manuscript we have changed it to: ”The timing of the ozone peak at these sites is governed by transport and depends on the site’s proximity to the urban source of precursors.”

RC: 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.

AC Peak concentrations are indeed lower on these days, and text has been added to that effect.

RC: 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.” Ĕ”goes without saying”

AC: The reviewer was unsure what point we are trying to make with this seeming statement of the obvious. The point we were attempting to emphasize is that emissions from the Sacramento Valley Air basin lead to local air quality problems AND to problems in the downwind Mountain Counties region. The Mountain Counties region will not be able to significantly improve its own air quality by enacting local emission controls. We have added a sentence to clarify this point.

RC p. 11438, line 14: s-v agreement

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AC: Done

RC p. 11439, line 3: The referee asked for 1-sigma error bars for the data shown in Figure 6.

AC: The data in Figure 6 are computed using the regulatory formula for the fraction of days on weekends vs weekdays where the air quality sites reported exceedances of the air quality standard. We are not aware of any useful way to represent the one sigma variance for data that represents the extreme of a population.

RC p. 11439, line 9: The reviewer asks for more details including trajectory calculations to support the qualified assertion in the text that the town of Jackson is influenced by a major highway and the city of Stockton, CA and not the city of Sacramento.

AC: The exact origins of the air masses measured over Jackson is less certain than Mountain Counties sites further to the north, which are clearly impacted by the Sacramento urban plume. Trajectory analyses are unreliable in this region because they don't often resolve the orographically-driven features of the circulation and are often not consistent with local observations. The purpose of our comment is more to suggest that Jackson should not be viewed as consistently 'downwind' of Sacramento, as the other MC sites are than to assert that we know the details of the air mass origin at Jackson. We have added a sentence to clarify this point.

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

RC: p. 11439, lines 21 The reviewer points out that the differences between ozone on Saturday and Monday at most of the MC sites is smaller than the 95% CI.

AC: The text has been edited to acknowledge that no statistically significant conclusions can be drawn.

RC: p. 11440, line 3: Symbols in Figure 8 have not printed correctly

AC: done.

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RC: p. 11440, line 12: “NO 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.

AC: We have added text indicating the calculations of JNO2 and the assumption that the two species are in photostationary state. In clear skies (which are present through most of the summer) these are calculations are expected to be reliable to 25% of the NO. Since NO is usually less than 20% of NO2 at the sites in question, the uncertainty on NOx is small–of order 6%. Text has been added to describe the calculations.

RC: 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.

AC: Unfortunately detailed information on the inlets used is not readily available. We agree with the reviewer that there is undoubtedly a bias in the NO2 data as the text that follows the sentence in question indicates.

RC: p. 11443, line 6: The reviewer questions our assertion that CO at Del Paso is an overestimate of CO at Granite Bay.

AC: Assuming the CO/NOx emission ratio is similar near the two sites, which is reasonable given the common vehicular source, and noting that NOx measured at Del Paso is approximately twice as high as NOx at Granite Bay, it is likely that CO observations at Del Paso are higher than what would have been measured at Granite Bay. Text has been added using the reviewers suggestion.

7. Differences in odd oxygen partitioning.

RC: 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

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odd 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.

AC: The figure suggested by the reviewer is an interesting idea and we have generated one using data from the T Street site, which is heavily affected by traffic emissions. However, because of the large variability in ozone, the figure is not very compelling. Although it does look more or less as the reviewer suggests, we are choosing not to include it in the final manuscript.

RC: The reviewer suggests deleting a section on aerosol effects.

AC: We agree that we have little to add to the discussion of these effects–except to reinforce the point that existing data has little to say about it. However, since aerosol effects are routinely included in lists of factors contributing to the weekend effect and since we do address all of the other factors in typical lists in either this paper or its companion. We believe a short section is worthwhile highlighting the need for both aerosol data and measurements of actinic flux to assess the true importance of this factor in ozone reduction strategies.

9. Conclusions.

RC: p. 11447, line 15: suggested clarification

AC: Done

RC: p. 11448, final paragraph beginning on line 12: This concluding paragraph reads like an introduction to, or advertisement for, part 2 of this work. The referee suggested

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deleting the paragraph.

AC: Done. We instead added a sentence early in the manuscript pointing to the second paper and explaining which of the hypotheses described on pg 11430 are addressed in this manuscript and which in the companion paper.

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

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Interactive Comment