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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of transported background ozone inflow on summertime air quality in a California ozone exceedance area" *by* D. D. Parrish et al.

Anonymous Referee #1

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General comments

This analysis is an important step towards addressing the role of background in contributing to air pollution events in the Northern Sacramento Valley (NSV) region. I'm not convinced that the interpretation of background contribution to exceedances is well supported by the analysis, which requires extrapolating from relationships that only capture 25-50% of the observed variability. However, additional event-based analysis could provide this support (see specific comments). I don't see where the paper addresses what is causing the observed variability in the sonde data but this seems key to the interpretation and conclusions. The conceptual model in Section 3.3 is confus-

ing and I question if it is "required" as stated in the text (see below). The paper clearly addresses a need from the policy community as discussed in Dr. Lashgari's short comment. I suggest either adding more analysis to support the conclusions or limiting the discussion to only those points clearly supported by the analysis, with discussion of what additional work is needed to provide conclusive evidence as to the implications for the role of background on NSV exceedances; the latter is already done to some extent in Section 7.

Specific comments

This correlation analysis is based on several assumptions: the sondes are measuring background levels; background is not produced over the continent or subsiding to the NSV in other air masses not sampled by the TH sondes; background is not lost during transport or mixing to the surface. This could be more clearly stated.

The intercept is likely very sensitive to the fit used but the derived number is critical to the conclusions drawn regarding the contribution of local vs. background, so some discussion of this uncertainty is needed.

More evidence is needed to support the idea that the sondes don't include a "regional" component, particularly since some of the trajectories in Figure A1 show an occasional continental influence at the sonde location.

The contribution of background to exceedance events is limited to a discussion of averages. The correlations, though statistically significant, are only explaining at best 20-50% of the variance at the NSV sites (Table 3). In Figure 12, the highest events at Tuscan Butte and Redding are not correlated with the sondes; similarly the highest sonde values fall off the best-fit line. What are the trajectories on these days? Some analysis for specific exceedance events could provide more compelling evidence for the conclusions drawn here.

The trajectory analysis should be included in the main paper.

The temporal offset for the highest correlations between the sondes and surface sites is first interpreted as suggesting a direct transport pathway but this is not strongly supported by the trajectory analysis. P16247 L5-11 discuss that direct transport is not required to explain the observed correlations but then how does one explain the time offset? This could be reconciled if there are times when direct transport is occurring and other situations where the correlation represents a large-scale influence - this could probably be addressed by examining specific events.

Given typical wind speeds at 1-2 km altitude in summer, what is the time scale for transport from TH to the NSV sites? Showing a map of summer wind climatology at 2km and possibly also at the surface would be very helpful; e.g. from the North American Regional Reanalysis (http://www.emc.ncep.noaa.gov/mmb/rreanl/). NARR data may also address the need discussed on L2-5 of P16251.

P 16242 L4-5, how do the intercepts compare with the more straightforward averages of the datasets?

P16242 L6-8. What about large-scale conditions conducive to ozone production or destruction at both TH and the NSV sites? Same question for P16243, end of Section 4.1; in addition, is the 0.8 h offset meaningful given the time resolution of the datasets? P16251 suggests this is speculative without a driving mechanism yet the regional nature of synoptic weather influencing pollution has been demonstrated for the eastern USA: Eder, B. K., J. M. Davis, and P. Bloomfield, A characterization of the spatiotemporal variability of non-urban ozone concentrations over the eastern United States, Atmos. Environ., 27A(16), 2645–2668, 1993. Vukovich, F. M., Regional-scale boundary layer ozone variations in the eastern United States and their association with meteorological variations, Atmos. Environ., 29, 2259–2273, 1995. Jeff Lehman, Kristen Swinton, Steve Bortnick, Cody Hamilton, Ellen Baldridge, Brian Eder, Bill Cox, Spatio-temporal characterization of tropospheric ozone across the eastern United States, Atmospheric Environment, Volume 38, Issue 26, August 2004, Pages 4357-4369

Abstract L10-12 seems inconsistent with the discussion on the bottom of P16250 that indicates these approaches are not comparable. Where is the attribution to hemispheric scale transport in the next sentence supported by this analysis?

P16237 L20-22. This statement should be supported by a quantitative assessment such as a correlation coefficient for the seasonal cycles. The seasonal cycle in the sonde data (Fig 4) has a springtime peak which seems closer to the MBL sites than the inland sites.

Section 3.2. Could ship emissions also be contributing to the low values measured at the MBL sites?

Section 3.3. P16239. I see the value of separating an ozone measurement into contributions from processes operating on different scales, but in practice the attribution is ambiguous, as noted later in the paper. For example, although diurnal cycles occur locally (L11 p. 16239) and in part reflect local emissions contributing to ozone production, these chemical processes are operating in tandem with the growth and decay of the boundary layer (and the mountain valley flows illustrated in Figure 11), which mix "background" and "regional" ozone into surface air at the same time as local chemical production is likely highest. L10-11. Are these terms "responsible for synoptic scale variability" or rather varying consistently across sites on synoptic scales? L12-14. I don't follow the distinction in regional versus local processes; isn't "regional" the aggregation of "local" processes, or synoptic meteorology creating conditions that influence chemistry and deposition similarly? L24-25 and P16240 L8-9 further confuse the argument since any measurement includes the sum of background+regional+local as shown in Eq (1), and it is argued later in the paper that MDA8 does in fact contain a local component (P16241 L24-25).

P16245 L21-26 The correlation coefficients are small (r<0.3), so is this discussion necessary? Similarly on P16248 L20-27, why is it NSV character rather than general continental outflow? I'm not convinced this discussion is necessary.

P16246 L2 The offset looks like up to 10-20 ppb; is this “modest”?

P16246 L14-15. It would better support this argument to show differences in, for example, mixing depths in winter versus summer for this region (perhaps from the NARR product mentioned above).

P16249 L17 What is the weighting used in the averaging and why? I’m not convinced that the correlations support the interpretation of the slope as a measure of air mass origin. How does this explain a slope above 1 at Redding?

P 16250 Would the faster removal processes at the NSV sites relative to Tuscan Butte also be accompanied by a larger chemical production? Would there be larger chemical loss of both locally produced ozone and transported background at these sites?

Modeling work addressing the second “implication” on P16252 was recently published in ACP, with the first author on this manuscript as a co-author, yet it is not cited anywhere in the text.

Table 3. Do theMDA8 values occur at the same time of day at all the sites? Why is the intercept for the slope of unity better than the intercept of the bivariate slope?

Figures 4 and 5. Why not use the MDA8 here for consistency? These figures can be more effective by combining them, using the same scale on 2 panels side by side with all the surface data in one panel, and the sonde data in the other.

Is Figure 7 necessary in addition to Table 2?

Section A2. Could fires also explain the CO enhancements? Wouldn’t the CO background of 100-130 ppb include some influence from U.S. emissions?

Technical corrections

The term “marine air” is confusing since it can be interpreted as implying a certain composition of the air mass, such as the marine boundary layer air discussed. Consider rephrasing to clarify, for example, “inflow of air from the marine troposphere” on

P16233 and elsewhere.

The citation to Fiore et al. 2002 for PRB O3 concentrations considered by EPA in the NAAQS process should instead be: Fiore, A., D. J. Jacob, H. Liu, R. M. Yantosca, T. D. Fairlie, and Q. Li (2003), Variability in surface ozone background over the United States: Implications for air quality policy, J. Geophys. Res., 108, 4787, doi:10.1029/2003JD003855.

The discussion on the top of P16236 should refer to Table 2.

P16236 and elsewhere, clarify that “impacted” refers to local pollution sources or drop the phrase.

P16237. Typo: tow -> two

P16240 L5 MBL -> PBL or continental BL?

Table 2. State which years are included in this analysis.

Figure 1 should include a color scale for topography and some indication of the horizontal distance between sites (e.g., adding lat/lon boundaries).

Figure 6. What is the dotted line?

Figures A1 and A2 should include the TH and NSV site locations.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 16231, 2010.