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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diurnal tracking of anthropogenic CO_2 emissions in the Los Angeles basin megacity during spring, 2010" *by* S. Newman et al.

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The general comments given in the first paragraph of this referee's review require that we improve our motivation, as we describe in our response to the first referee's first major comment. The recent paper by McKain et al. (2012) is indeed an excellent study of CO2 emission in the Salt Lake City valley. Unfortunately, we could not include the conclusions of this paper in our original ACPD manuscript because our paper was published online before the McKain et al. article was published. We have included references to it now. The different diurnal trend for emissions in Salt Lake City relative to what we show for the LA basin is an average over the entire year, not just for the

May-June time period of our data. Our experience is that the warm months in Pasadena show much less evidence of morning and evening rush hour than do the winter months (Newman and Stolper, 2009, AGU Fall Meeting).

Major comments:

1. The main analysis of the paper uses CO/CO2 and the observed BLH diurnal variations to tease out the anthropogenic contribution to CO2. However, it fails to discuss how fire emissions were excluded from their data sets, since they have very high CO/CO2 ratio. Was there any fire contamination in the data set? How was it excluded? I can clearly see large spikes in CO and CO2 as well as sustained periods where they were increased. What does the CO/CO2 for these episodes? Is it indicative of fires-biomass burning (e.g. Wunch et al filtered fires in their TCCON data)? If fires are present and not excluded with some CO/CO2 filter – they will create a high bias in the anthropogenic contributions. This may be one of the reasons why the anthropogenic CO2 fraction F is greater than 1 at noon. It would be useful to see in Figure 4 a CO/CO2 time series with a higher time resolution to address this question.

There is no effect of fires on our data set. There was one fire during the campaign located ~65 km ESE of our site on May 20-21, 2010, in Home Gardens, CA. However, it is extremely unlikely that any emissions from this fire came to Pasadena, since it was downwind of our site. There was no spike in either the CO/CO2 or CO time series that would be indicative of such contamination.

2. Please explain justification of using a CO time varying background of 90 to 136ppb. I suspect this can make a difference! Figure A2 shows that there is almost always an excess of âĹij50ppb over the background at Pasadena – that is the region never cleans out in CO.

We respectfully point out that we reported (p 5784, ln 22-23) that the CO background varied from ~135 to ~110 ppb (not 90 ppb). Background values were calculated using STILT particle trajectories intersecting the NOAA pacific marine CO background, pro-

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ducing results that agree quite well with NOAA P3 CO observations during the CALNEX campaign. We do not consider it surprising that CO at CIT is often greater than the background values. This is consistent with the combination of high CO emissions into the LA and incomplete atmospheric flushing that results in high CO (> 500 ppb) CO. However, there are periods when air flushes the valley to relatively near background values as compared to these higher values.

3. Figure B2 – CO2ff from CO/CO2 and Vulcan forward model are unclear to me. The CO/CO2 (red) seem to be higher than WRF/STILT/Vulcan (brown). Please elaborate more in the text. I am not clear on the confidence in the claim regarding WRF-STILT being within 5 ppm – there appears to be biases. and this can be 20-50% of the signal (10-25ppm). What is the interpretation of the intercepts in Fig B3? Are they related to assumed background being off?

We appreciate the comment and have clarified the text to identify that Figure B2 compares 1) the CO2ff estimated using the continuous estimate of COxs measurements scaled to CO2ff using the mean ratio, R, estimated from flask measurements and background values, and 2) the CO2ff estimated from WRF-STILT footprints convolved with the VULCAN2 emission maps. We also explicitly comment that the mean difference between the model predictions and measurements is a small and statistically insignificant offset of 2.6 +/- 2.3 ppm as shown in Figure B3. The corresponding RMS difference between predictions and measurements is $\sim$ 5 ppm, which is small compared to the range of measured and predicted CO2ff enhancements from local emissions. Therefore the intercepts in Figure B3 are not significantly different from zero, although residual errors in some combination of background, average footprint strength, and/or average emissions cannot be ruled out.

4. Clearly a high degree of averaging CO/CO2 PBL/dynamics has been performed in the analysis of diurnal profiles (hourly over the month) – it would be valuable to discuss the variability during this period.

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A few sentences have been added to the Results section to discuss variations in COxs/CO2xs and wind speed as well as extend the discussion of the CO2 time series. Time series figures of COxs/CO2xs and wind speed have been added as Figure A4.

5. Why is there a large difference in the two C14 derived F's?

The two values of Δ14C-derived CO2ff are different because they are for the two halves of the campaign, as described on lines 19-27 of p. 5779 of the ACPD paper. The afternoon CO2ff, derived from both the COxs/CO2xs ratios and the WRF-STILT prediction and shown in Figure B2, indicate a higher average CO2ff for the second half of the campaign than for the first, consistent with the Δ14C results.

6. Discussion of the afternoon meteorology when the PBL-height decreases and the wind speed increases (Figure 3, 12-16hrs) is important. Clearly there are tradeoffs between the two effects. How and when does mixing with cleaner air mass north of the valley become important for the reported diurnal profile? I can see a 2-hour delay in the PBL peak relative excess CO.

Typically both PBL height and wind speed reach maxima during the afternoon. We respectfully suggest that both higher PBL and higher wind speed will act to dilute and flush the valley, somewhat reducing surface CO and CO2 but having smaller effect on the total column which is not as strongly affected by dilution into a taller boundary layer. The increase in wind speed is not associated with wind from the north, but is usually a stronger afternoon sea breeze. The afternoon breeze never came from the north during this campaign.

Minor comments

1. Many of the figure legends were incorrect or illegible 1. I could not resolve weekend versus weekday lines in Figure 3 they look all solid

Thank you for pointing out inconsistencies, unfortunately missed from a previous ver-

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sion! We do not show weekday/weekend differences because of the short length of the campaign and, therefore, poor statistics for weekend days. The caption to Figure 3 has been corrected.

2. Make Figure 1 on same scale

Figure 1a has been redone to match the scale of Figure 1b.

3. Figure 4 the diurnal variations are shown in red not purple

The caption of Figure 4 has been corrected to reflect the correct color for the FCO2ff diurnal trend.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5771, 2012.