

Interactive comment on “Diurnal tracking of anthropogenic CO₂ emissions in the Los Angeles basin megacity during spring, 2010” by S. Newman et al.

Anonymous Referee #2

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Newman et al present an idealized semi-empirical analysis of continuous in situ observations of CO₂, CO and boundary layer height (BLH) for a month (15 May to 15 June, 2010, Calnex) at Caltech. The paper is reasonably well structured and written. However, the overarching claims are far too broad, general (and not new) and based on limited data and approximations. These claims need to be toned down significantly. For example Wunch et al 2008 have already shown afternoon increases of CO₂ of 2-8 ppm in LA in 2008 that are healthy and resolvable from space (and damped here due to averaging of 3 minute data over hour) – and of the order expected from LA megacity emissions. Furthermore, a recent analysis by McKain et al PNAS 2012 of 2006 CO₂ data in Salt Lake city shows a different diurnal pattern (morning and night peak) due to

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different meteorological and uses more comprehensive models. Clearly the urban verification problem is much more complex and variable than is this conclusions here lead us to believe. This paper does provide some semi-quantitative and qualitative insights on attributing urban CO₂ sources using CO and PBL observations and a simplified model.

I have several major technical issues that must be addressed and discussed in this paper to be acceptable for publication:

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.
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 ~50ppb over the background at Pasadena – that is the region never cleans out in CO.
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?
4. Clearly a high degree of averaging CO/CO₂ PBL/dynamics has been performed in the analysis of diurnal profiles (hourly over the

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month) – It would be valuable to discuss the variability during this period. 5. Why is there a large difference in the two C14 derived F's? 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

Minor points – 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 2. Make Figure 1 on same scale 3. Figure 4 the diurnal variations are shown in red not purple

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