

***Interactive comment on* “Temporal variations in CO₂ and CO at Ahmedabad in western India” by N. Chandra et al.**

Anonymous Referee #1

Received and published: 27 January 2016

This paper presents a year of data on CO and CO₂ concentrations from a site in Ahmedabad. High quality concentration data from urban areas in general are sparse, and such data from the large urban areas in rapidly developing regions are especially limited. These observations can contribute to understanding emission patterns in a poorly studied region that is critically important to the global carbon budget. The experimental methods are excellent and include decent calibration scheme. The text provides a good summary of the methods and defines precision and accuracy. However, the discussion needs to be more focused and strive to present a consistent set of key findings. As noted in detailed comments, some of the observed variations in concentrations may not contribute to interpreting emissions patterns. The results will be more convincing by focusing on the key aspects of the data. It is important to distin-

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guish between patterns with information about atmospheric dynamics (vertical mixing and transport) and patterns that have information about emission sources.

Comments and suggestions for revised analysis.

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With respect to the evolution of CO₂ during night time. Even in cold regions there soils approach 0C respiration continues throughout the night. At this site I don't think you can attribute lack of increasing CO₂ during night in some seasons to respiration being dormant. There is certainly no evidence included in the text for this. In this site I would only expect respiration to be suppressed by very dry soils, so it could be a reason in the spring, but temperatures are probably not cold enough to suppress respiration. You don't show any data for nighttime winds. Differences in depth and strength of the nocturnal inversion and whether or not winds persist at night are factors that would impact whether or not trace gases accumulate at the surface during night. In subsequent section you show that nighttime concentrations of CO decline continuously in the winter and spring season, which indicates that there is enough vertical mixing of low CO air from above that once the CO source is turned off its concentration drops. Thus the constant CO₂ at night is evidence of a continued source in order to offset dilution by mixing of low CO₂ air from aloft. The dynamics of CO₂ is not just the depth of mixing. You can note that because there is active CO₂ uptake during seasons when vegetation is active the entire mixed layer is depleted during daytime and when residual layer mixes to the surface in morning, low-CO₂ air is mixed down.

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This paragraph about comparison to a model ought to come later as discussion and not be in the results. Also, keep in mind that the magnitude of concentration variation is not directly proportional to the magnitude of a flux. In a simple sense the amplitude of concentration changes are proportional to flux divided by mixed layer depth and strength of vertical mixing. In order to use the observed concentrations to evaluate the

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validity of modeled CO₂ fluxes you need to consider what the influence region for the concentration is and convert the observations and model to comparable units. Either combine concentration data and typical mixing depth evolution to estimate a change in column density, or merge the CASA fluxes with a transport model to predict concentrations. The claim that model and observations are inconsistent is not convincing. The greatest magnitude of net daytime uptake and difference between CASA fluxes in day and night is in September through November, consistent with the peaks in amplitude of mixing ratio diel cycle (day/night difference of CO₂ concentration increases from 20 ppm in August to 50 ppm in October). So I don't see where the observations suggest productivity is higher in August than Sept-October

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The statement here on pg32302, line 26 about respiration contributing to CO₂ is inconsistent with the previous section suggesting that respiration was dormant.

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The regression slopes for CO:CO₂ are not credible estimates of the emission ratio. The difference between actual background CO₂ and the assumed constant value that is used to compute excess is correlated with time of day and thus with CO, so the slope of CO:CO₂ will be corrupted I do notice that the upper edge in all the figures appears to have a similar slope. That edge represents the air that is most strongly influenced by CO emission sources. Although I think it would be better to split up the data into groups that actually show a decent correlation, if you want to stick with the overall regression those lines should be shown on the figure and for comparison include some lines that show the slopes for a few representative emission sources. Note that in previous section examining diel cycles you made a convincing argument that CO emissions were shut down at night so concentrations declined but CO₂ from respiration continued. Thus nighttime data should not be useful for finding an emission ratio. I would suggest trying something similar to the analysis of Potosnak et al 1999

that seeks to extract the influence from biosphere and mean diel cycle. (J. Geophys. Res., 104(D8), 9561–9569, doi:10.1029/1999JD900102.)

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In the end the CO:CO₂ ratios have such a wide range as to not be very useful at all. Unless you can reanalyze them to bring a narrower estimate it is not worthwhile to show this section. It is curious that the nighttime data have such a good correlation when the diel cycle analysis suggested that combustion emissions of both CO and CO₂ together were shut down. It would help to illustrate the relationship between CO and CO₂ in night by coloring the symbols for nighttime data differently for time of day in Figure 8a I suspect the daytime values, with low correlation coefficients are not reliable, as you suggest by indicating the importance of CO₂ uptake. When biospheric influence influences the CO₂ mixing ratio you shouldn't bother to try and analyze the CO:CO₂ ratio.

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The previous section about CO:CO₂ slopes is rather muddled. It would be more convincing to focus on demonstrating the validity of just the nighttime and rush-hour periods that you are using here. Showing the data for entire day just confuses things Assuming the discussion of ratios just for the relevant periods is more convincing you can also include some calculation of the uncertainty, which then feeds into providing estimates of uncertainty in the emissions you compute from those ratios and the CO₂ inventory. Uncertainty estimates are critical to include here.

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Consider plotting actual CO and CO₂ mixing ratios to see if the intercepts match the values chosen for background. In the active growing season the biospheric influence will impart a wide range of CO₂ for given values of CO, which is what shows for most seasons. A meaningful slope is difficult to extract in this case. A better estimate of

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CO₂:CO could possibly be derived by using information from the mean diel cycle analysis to subtract a variable background, or restrict the analysis to just a fixed time of day, or analyze night and daytime separately

Minor editing

32197 line 25

There must be a missing word in the sentence; 'resulting in concentrations at the surface in the summer compared to the winter...'

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 32185, 2015.

ACPD

15, C12020–C12024,
2016

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