

Interactive comment on “Using ground-based solar and lunar infrared spectroscopy to study the diurnal trend of carbon monoxide in the Mexico City boundary layer” by W. Stremme et al.

Anonymous Referee #1

Received and published: 25 June 2009

This paper describes the evolution of carbon monoxide in the Mexico City boundary layer using solar infrared spectroscopy and novel lunar infrared spectroscopy. This reviewer cannot evaluate the technical details of the spectroscopy but needs to accept them on trust and focus on the interpretation of results presented in the paper. The measurements seem to be novel and important, and so publication is recommended in ACP subject to the comments below. As a general comment, the paper could benefit from a thorough pass of editing.

Major comments:

The introduction lumps together a string of citations 11503-10. This should be ex-

C2248

panded to mention the insights from these papers relevant for the current paper. In particular, Burton et al., have done lunar spectroscopy before, and their work should be discussed in addition to the reference at the end of section 2.3.

The question of the emission inventory has received considerable attention before please do a more thorough review of past work and conclusions, including current discussion of the MILAGRO campaign (eg. Tie et al., 2009 ACPD). 11524-5 says “opens the possibility”. It would be nice to see a more complete review of this question along with some estimates based on this paper. In connection with this, I think it would be worth expanding the discussion of CO concentration and emission trends. (11503-25)

Fig. 8 shows a big difference between Nov and Feb. This should be discussed in the text. Also please put the background level on the fig, which makes the contrast even more striking.

In relation to the two points above, you might consider looking at horizontal wind speeds to get a dilution factor. This could tell you if winds were stronger in Feb leading to lower columns. It could also be used in a first order calculation of expected columns based on the emission inventory.

The discussion of night-time dispersion should be expanded. There are tethered balloon and Lidar measurements that could be used to expand the discussion. In particular, the high night-time values of MLH in Fig. 10 suggest that you are getting the residual layer aloft, not the surface CO.

The model data of the boundary layer height is from the NARR model, please add a sentence about this. With a horizontal resolution of 32 km, it is not clear that the values are going to be representative with 3 cells across the basin. I would recommend comparing MLH values with readily available radiosonde data. Furthermore, the growth of the MLH in Fig. 10 seems slow and the maximum values low. Comparison with radiosonde data might suggest if this is because of the cold season or for other reasons.

C2249

11518-18: The effect is very slight – if it is evidence of a residual layer, it would suggest that the residual layer does not have much CO. This paragraph should be either expanded based on discussion of known features of basin circulation, or the speculation should be limited.

11519-25: different surface pressure might impact the background column value, but otherwise the resemblance is presumably due a match in the combination of emissions and horizontal ventilation.

Minor comments:

Please review the language, there are numerous errors that a spell checker would correct immediately, eg.:

Challenging, dirunal, informartion, carfully, interference, qunatities, diappeares,

“from an in the laboratory”, “goals of sought”, “an current topic”,

“strongly variable” – use highly variable, ‘if in contrary” – use in contrast

Sec. 4, first sentence is very vague.

Fig. 14 is referred to as fig 13 in the text.

Expand the caption for fig. 8 and 9 to say the location.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11501, 2009.