

Interactive comment on “Methanol-CO correlations in Mexico City pollution outflow from aircraft and satellite during MILAGRO” by Y. Xiao et al.

Anonymous Referee #3

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This paper uses methanol (CH₃OH)-carbon monoxide (CO) correlations from aircraft and satellite data from the MILAGRO campaign to infer biogenic and anthropogenic sources for emissions out of Mexico City. The main goal of this paper is to demonstrate the potential for TES CH₃OH and CO data to gain insight into global sources through their relationship. Although the ideas presented are interesting and satellite observations of these species could be of value to the community for source attribution, the lack of information in the TES retrievals over this elevated surface region and the substantial differences with the aircraft measurements do not support the conclusions presented by the authors. I do not recommend this paper for publication currently and suggest resubmission after improvements are made to the TES CO retrievals and/or current

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versions of the retrievals are presented.

My specific comments are:

In the last paragraph of Section 1: (1) In the last sentence of the second paragraph, uncertainties are mentioned (ie. magnitude, seasonality, and spatial distribution). How will these results improve these? (2) A comment is made that satellite / aircraft comparisons “proved challenging” because of the “limited sampling coincidence within reasonable spatial and temporal criteria.” There is no mention of what these “reasonable” criteria are here and should be discussed in at least one sentence and/or reference. (3) In the second to last sentence, “in order” is not needed. (4) The last sentence starting with “Descriptions” should not be a new paragraph.

In Section 2: (1) Cady-Pereira et al. (2012) and Wells et al. (2012) are good references for the TES CH₃OH retrievals, but there should be a few more points from their papers presented in this discussion. Cady-Pereira et al. (2012) states that CH₃OH retrievals are most sensitive between 900-700 hpa (which appears to be case here too) with “good” profiles of 0.5 -1 DOF. What was the average DOF for the profiles used in this study? The statement on line 9-10 on pg. 5713 is not specific enough, especially when information content is of concern. (2) In the TES CH₃OH retrievals, there are a possible four different a priori profiles used. The a priori for this region should have been discussed in at least one sentence (since the lower the DOF the more the profile is based off that constraint.) (3) Why is it not mentioned that Wells et al. (2012) presented comparisons of DC-8, C-130, and TES CH₃OH retrievals during MILAGRO? Their results (good or bad) would have provided additional validation that is missing in this discussion. (4) TES version 4 CO retrievals are used here despite knowing the probable issues TES will have in the MCMA. Is there a reason, version 5 retrievals were not selected? If there is, that should have been made clear. Otherwise, I revert back to my original suggestion to using later versions of CO retrievals for this analysis, particularly after reading in Section 3.2 the trouble encountered with the CO retrievals over Mexico City as visible in Figure 4c.

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In Section 3: (1) The statement, “TES does not clearly resolve the CO pollution over the Mexico City Basin,” concerns me. Despite simulations trying to rule out vertical resolution as a problem for the CO retrieval, TES still does not show the trend observed by the aircrafts. At this point, the analysis should have been suspended until further validation could be made on the CO retrievals over elevated regions, or another polluted region should have been used as example for this method of source attribution. (2) In Section 3.4, the authors recognize the difficulty to use this aircraft data for evaluation of TES (although Wells et al. (2012) used a similar dataset for their comparisons). Once this was realized, perhaps “coincidence” with aircraft data during this time period should not have been a priority and separate statistics should have been completed with an increased number of TES profiles for this region. The limited number of points in Figure 6 is alarming. (3) Line 25 on pg. 5719 should have the word “to” not “too”. (4) The differences in the CH₃OH and CO relationship between MCMA and the Amazon Basin show promise for global applications of this method with satellite data. Until critical retrieval errors are removed, the quantitative aspect of this ratio is not reliable in my opinion.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5705, 2012.

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