

# ***Interactive comment on “Seasonal and Spatial Changes in Trace Gases over Megacities from AURA TES Observations” by Karen E. Cady-Pereira et al.***

## **Anonymous Referee #2**

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### Overview:

The manuscript by Cady-Pereira evaluates a time series of remote sensing observations of O<sub>3</sub>, HCOOH, CH<sub>3</sub>OH and NH<sub>3</sub> for several years near Mexico City and Lagos. This unique dataset has the potential to reveal new insights into the trends and distributions of different forms of air pollution in these areas, and to evaluate the extent to which this variability is well represented in models. With these goals in mind, the paper does identify some unique features of the data, such as exploring the impact of biomass burning on an episode of high pollutant concentrations near Mexico City, and evaluating the difference between land-sea gradients in the data vs a model near Lagos. The manuscript is generally clearly written and the topic is suitable for ACP.

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However, this paper has several larger shortcomings which would prohibit publication at this time.

The first of these is related to scope. The title and introduction, and even Table 1, seem to lead up to an analysis of megacities world wide. I'm expecting a comprehensive study of satellite data in megacities, such as has been recently published for NO<sub>2</sub> and SO<sub>2</sub>. However, what I find here is a much more narrow look at just 2 cities, despite presenting a Table of 19 cities studied. The reasons for excluding the other 17 cities are never provided. So, this is a bit of a let down when reading through the paper. While it would be tremendously valuable were the authors to extend their analysis to the other 17 cities, I imagine they will resist this suggestion given the effort involved. However, that does mean though that they need to reconsider the scope and aims of the study, and should more succinctly frame the paper in the context of comparing Mexico City and Lagos, and nothing more. What's more, most of the analysis of the data from MCMA is centered around a few biomass burning episodes, which left the authors without much room to consider further analysis of the time series of O<sub>3</sub> or CH<sub>3</sub>OH, which they then state lies beyond the scope of this work.

The second major issue is that the remote sensing products used here don't necessarily reflect the pollutant concentrations at the surface in the urban areas in question, and the extent to which they might will be different for Mexico City vs Lagos. Given the expertise in remote sensing from the authors, this should have been stated and evaluated right up front; rather, it is hardly mentioned, and this just feels like the data is being misrepresented in a way I would have expected more from a group new at satellite data, rather than from the experts. The paper needs to be revised to address this issue head on, and at all stages of analysis throughout the work.

Lastly, the paper tends to read like a bit of a sales pitch for TES. Comparisons of TES to the value from other types of measurements and models is very one-sided. The authors should be more mindful of this throughout.

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Details on these comments, as they occurred to me while reading the paper, are described below.

Comments:

1.30: In ascribing these pollutant concentration levels to the cities, is there any concern that the satellite observations are possibly seeing concentrations very different from what is occurring at the surface, or being ascribable to that cities air quality?

2.23 This statement is debatable. NO<sub>2</sub> and SO<sub>2</sub> gradients near megacities have been well mapped in several studies. Numerous modeling studies provide insight into the key sources and fates of pollution for megacities. I see what the authors are attempting in terms of framing with this sentence, but the wording goes too far.

2.26: Not sure what is meant here by “big picture”. My hunch though is it is a very specific interpretation of that phrase that just so happens to be addressed by TES observations. My suggestion though would be to stick to more precise language here, such as the well-made point about vertical distributions.

2.34: This is a pretty one-sided view of satellite observations, where none of the downsides are considered (low spatial resolution, low signal to noise, vertical sensitivities far from the surface, etc.). It seemed odd that the issue of sensing in the free-trop vs at the surface wasn't really considered in the introduction. I'm not even sure setting up a dialogue pitting surface and aircraft observations vs satellite observations is needed – can't the authors just present the science questions and the satellite data, and let the results stand for themselves?

3.24 - 32: This discussion struck me as a bit narrow, not really considering the science questions and literature associated with these species as much as it was brief mention of papers the authors have written studying these species with TES.

4.19-32: The use of SOs is a key component driving this study. As such, I think it should be discussed earlier, in the introduction.

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4.29: At this point I'm wondering why the paper is going to focus on just 2 cities rather than these 19.

4.32: I don't know the lat lon of city centers of the top of my head, so it's difficult to evaluate Table 1. Can the authors also include the urban center points, so that we get a sense of the alignment? Or make an array of figures such as that in Fig 1? However, even Fig 1 leaves much to be desired. On what day are these concentration values for? Where is the MCMA region in this picture? More broadly, what is the purpose of considering a true-image color map here? Wouldn't it be more informative to plot the transect over a map that shows the MCMA region and topography (like Fig 2) or to plot over a map of e.g. population? I can't align Fig 2 with Fig 1 since lat / lon aren't specified in the latter, and MCMA isn't shown in the former. Overall, more effective use of maps needs to be considered.

5.31: That is not the correct definition of PM10 (the authors seem to be confusing this with "coarse PM i.e. PM10 - PM2.5). PM10 includes all particles with aerodynamic diameter less than 10. Also, the authors should use the phrase "aerodynamic diameter", not "diameter", in these definitions.

6.30: I'm not sure what is meant by "NH3 emissions are limited". In time? In space? In magnitude or the extent of sectors considered? Please clarify.

7.12: All inventories, or just the EDGAR inventory? For example, this might be very different than inventories constructed specifically for these regions, such as NEI (US) or BRAVO (Mexico).

8.3: Yes, but wouldn't it also be important to say that aircraft studies have linked MCMA pollution to mostly being owing to sources within MCMA? Long-range biomass burning contributions are a small fraction of the air quality problem there. The broader relevance of this episode to Mexico City isn't really made clear.

Fig 4: This isn't a very effective use of space. I think the trajectories and fire locations

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could be shown on the same map. The maps may be zoomed over the regions of interest. Other information like the sub-national political boundaries in Guatemala are distracting and should be removed. Overall, I get the sense the authors are using some automated figures generated by different tools rather than synthesizing the data to make their own most effective figures.

8.20: At some altitudes, yes, but for the lower levels the trajectories appear to run north of much of the burning. This would be clearer though if the trajectories and fires were on the same map.

8.29: Fire maps for April 23 not shown?

Fig 6: It would be useful to indicate the latitude of MCMA center and caldera edges in this figure, since they are referred to in the text concerning Fig 6 but not evident here without cross referencing other tables and figures.

9.17: The phrase “air inside the basin was somewhat isolated from the biomass burning influence” summarizes one of my key issues with the presentation of this analysis if the “MCMA air pollution” sources, as to be more precise the analysis appears to be of concentration that are near and high above MCMA but not necessarily indicative of the air pollution at surface level within MCMA itself, and as such the motivation for learning more about them has not been well stated.

9.28: If a critical analysis of the CH<sub>3</sub>OH trends are beyond the scope of this paper, does inclusion of the data itself warrant being within the scope of this paper? I’m struggling to see the point. At this point in the paper, it seems CH<sub>3</sub>O<sub>3</sub> could be dropped and all of the points made thus far (which are mostly about a biomass burning episode) could be made equally well.

9.27: The authors should know, from the TES averaging kernels, exactly what the TES sensitivity is to near surface O<sub>3</sub> concentrations. Hence, this “possible lack” is something that should be rigorously quantifiable rather than speculated.

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9.30: Why? This seems like a rather random thing to do. Is one of the goals of this paper really to evaluate the MIROC model accuracy? Is the MIROC model to be used for some analysis to help explain the TES data later on? After reading the entire paragraph it seems the only point is to make the claim that TES can see data at finer scales than larger models. This is a rather obvious point, given the spatial dimension of the TES footprint vs the model resolution, and does little to quantify anything useful for the satellite data or modeling community. One could imagine using an aggregate of satellite data to see if the coarse model gets at least a good estimate of what it is built to estimate, namely average concentrations at the 300 km scale, but that goes way beyond the analysis provided here. As such, I strongly suggest just removing this paragraph entirely, and the associated summary of this point in the abstract.

10.10: If the authors want to use human health impacts as a motivating factor, then they need to more critically discuss the relevance of free-tropospheric concentrations of these species to surface concentrations and health.

10.22: Perhaps, horizontally, but the sensitivity of in situ measurements to concentrations at the surface level would be a benefit for the latter. So, again, this doesn't come across as a balanced assessment, rather than a sales pitch for TES.

10.9 - 11.8: Should the authors decide to limit their paper to just a case study of these two cities, then this content should all be in the introduction.

11.15- 17: It is implied that the TES data reflect Mexico City surface-level concentrations (because that is what has a "reputation"), in which case it would be interesting/shocking that these values are smaller than TES measurements in Lagos, but in fact from the previous analysis we learned that the MCMA TES data doesn't necessarily reflect the Mexico City basin concentrations. In other words, from this comparison I'm just not sure if I've learned anything about the difference of pollution levels between these two cities, or the difference in the ability of nearby TES transects to represent the urban pollution.

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Fig 10: What is the date for the NH<sub>3</sub> concentrations here? What is the benefit of showing land cover and ocean bathymetry?

11.29: Or because NH<sub>3</sub> dry deposits quickly. I'm not sure the evidence presented here alone is sufficient to blame the loss on secondary aerosol formation, although more analysis of how SO<sub>2</sub> and NO<sub>2</sub> levels vary (or do not) with season might be used to make such distinctions.

Fig 12: What is shown by the vertical dashed line - land sea interface? Why do the JJA and SON CH<sub>3</sub>OH and HCOOH concentrations plummet to sharply at around 7 degrees?

12.11: TES isn't usually used to evaluate surface-level O<sub>3</sub>. To what extent is the TES O<sub>3</sub> profile at these three lowest levels impacted by the prior compared to the measurement?

Corrections:

1.25: Something is grammatically odd about this sentence, switching from singular "it" to plural "data".

1.25: no comma, or change to "and we show"

3.15: Adjust grammar here: "used used", and "two of TES observing modes"

3.25: subscript 3

3.29: Better worded as "carbon monoxide observed by TES"

4.15: After 2011, SOs...

5.11: In general,

5.23 and elsewhere: use degree symbol rather than "deg" and the multiplication symbol rather than x?

5.30: We "consider" instead of "look at"

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6:30: comma after However

7.8: 20%

7.17: <http://geos-chem.org>

7.25: “if from”?

10.6: double period

11.24, 11.33, other places: O3

11.25: missing space

12.4: per force

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