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Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation and Analysis of MOPITT CO Observations of the Amazon Basin" *by* M. N. Deeter et al.

Anonymous Referee #1

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In this paper the MOPITT v6 multi-spectral product is validated with aircarft profiles flown over the Amazon basin. This is an important subject and the results indicate a significant negative bias in retrieved lower-tropospheric CO concentrations. This is important for the use of MOPITT data in an inverse modelling framework, e.g. to infer biomass burning emissions and interannual variability therein.

De paper is well written and consice, and I have only a few major remarks, which I list below.

1. Role of the prior profile

As the authors clearly outline, the MOPITT product comes at 10 vertical layers, and depends on the averaging kernel and the prior profile (a multi-year model-based climatology). First, I was confused by the fact that no mention is made that profiles are

used as log(VMR). I think this should be added. In the comparison with the aircraft profiles, the authors mention the role of the prior, especially in the TIR-only product. In fact they mention that extra noise is added by including NIR observations. A valid question to be asked is: "What is now exactly the added value of MOPITT in observing CO biomass burning plumes?". I think this question can at least partly be answered by presenting the skill of the prior data to explain the aircraft data. How would figure 4/5 look for the prior climatology? The prior data are already presented in figure 7, so it would be relatively easy to construct a comparison between aircraft CO and the prior. Since the role of the prior is important, it would be good to give a bit more explanation about how the prior has been evaluated and its time resolution (monthly?). Finally, at specific locations (e.g. line 131) it would be good to mention the value of the prior.

2. Comparison of the interannual varibility to bottom-up variability

The paper starts with a brief explanation of "bottom-up" emission inventories, like GFED and FINN. Also, the conclusion section starts with these datasets. In that respect, the paper could provide slightly more context by presenting a comparison with these datasets. One option would be to compare the (scaled) seasonal cycles and interannual varabilty in the MOPITT data (figures 7 and 8).

Minor points:

Line 26: "To account for the chemistry and dynamics that affect trace gas concentrations after their production, inverse modeling methods are exploited". I think the main issue of inverse modelling is to relate emissions to observations. This relation indeed involves transport and chemistry. I suggest rewording this.

Line 85: Here it would be good to provide more information about the prior (time resolution, climatology, which model?)

Line 126: This is confusing. Why not present the second overpass after the first overpass?

Line 131: include values of the prior (e.g. refer to figure 7).

Line 154: Maybe some explanation what causes this would be in place?

Line 210: Is this the same climatology as the prior? Yearly, monthly?

Line 257: The correlation coefficients for the three retrieval levels decrease with increasing altitude, from 0.94 at the surface to 0.82 at 600 hPa. In the plot I see values of 0.98 and 0.86. Please check.

Line 260: Here you could add that the dynamic range (max-min, see figure 7) largely explains the high r-values. However, the noise in MOPITT data lowers the r-values I guess. Would a time series provide additional information?

Line 278: “Therefore, the larger standard deviation associated with the TIR-NIR product does not by itself necessarily indicate lower retrieval quality”. Indeed, but here I wonder about the performance of the “prior”. I have the impression that the NIR-data induce quite some noise in the MOPITT product.

Line 314: I missed in the description where these T-profiles used in the retrieval come from.

Line 361: “analyzing Level 3 monthly-mean products is much more efficient than for Level 2”. If this would introduce errors in the analysis, I still would prefer the level-2 analysis. Efficiency is not a good argument.

Figure 4/5 caption: Are the statitics based on the log-log fit, or on the lineair concentration scale?

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