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Interactive Comment

Interactive comment on "Evaluating the performance of pyrogenic and biogenic emission inventories against one decade of space-based formaldehyde columns" *by* T. Stavrakou et al.

T. Stavrakou et al.

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The authors would like to thank Referee#2 for his/her constructive criticism and comments which contributed to substantially improve the manuscript.

- To reply to Referee's#1 comment, a new section has been included (Subsect. 4.2), providing a tentative assessment of model errors.
- The section on the description of the HCHO dataset has now been shortened, as suggested by Referee#4.
- To comply with the Referee#2 and #4 request to shorten the manuscript, the

descriptions of the emission databases have now moved to the Supplemental material (Part A).

- The model results presented in the revised manuscript are obtained with a model time step of 3 hours (instead of one day). This change does not affect much the results and the conclusions.
- The error bars in Fig. 6 and Figs. 8-12 now represent the retrieval error estimated by De Smedt et al.,2008.
- To reply to the Referee's#4 comment, we have now added two tables (Table 5 and 6) with the average biases and the spatiotemporal correlation coefficients over large regions for the burning season and for the rest of the year.
- The abstract, the Section 5 and the conclusions are reformulated to reflect the existence of uncertainties in the HCHO retrieval, especially over fire scene s, as requested by all referees.

A point-to-point reply to the referee's comments (in italics) follows.

General comments

The authors present a 10-year dataset of HCHO measurements from GOME and SCIAMACHY and compare them to output from the IMAGESv2 CTM driven by different emission inventories. The subject matter is suitable to ACP. I think this new satellite dataset is a useful contribution to the community. The paper should primarily be viewed as a qualitative overview of this new dataset. Some of the quantitative interpretations are questionable given the acknowledged shortcomings in the retrieval over fire scenes. In particular, the authors point out that the retrieval is biased by up to 40considered, but this is then ignored in the subsequent evaluations of bottom-up pyrogenic emission inventories. A revised version should reframe the biomass burning comparisons throughout the manuscript, so that such uncertainties are part of the discussion.

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I also have some suggestions for reducing the manuscript length. I recommend publication if these comments are addressed.

We fully agree that caution is necessary when presenting comparisons between observed and simulated formaldehyde columns, especially over fire scenes. Conclusions drawn from these comparisons are now formulated more carefully to reflect the existence of uncertainties, when appropriate. In particular, the role of the (absence of) aerosol correction in the retrieval is taken into account in the discussion.

Specific comments:

I don't see the point of including outdated emission inventories (GEIA, GFEDv1) in the discussion. The paper is overly long for the amount of science presented, and one way to simplify and streamline would be to just focus on MEGAN and GFEDv2. Since both MEGAN and GFEDv2 incorporate improved methodology, and quantity and quality of input data, occasions that GEIA and GFEDv1 happen to do better would seem to be a result of chance and so not really of interest.

The reviewer is right that newer versions of inventories should be better. However, our work clearly shows that this is not always the case. For example, GFEDv1 performed better over the Amazon than GFEDv2. We cannot say whether the earlier version was "right for the wrong reasons" but our work does indicate the the GFEDv2 inventory still has important shortcomings. We believe this is important information as these products are often used without considering their uncertainty; our work clearly shows the limitations of these datasets.

16987, L28. How is the shape factor interpolated? By interpolating concentrations at each altitude? The IMAGES horizontal resolution should also be mentioned here.

Yes, the IMAGES concentrations are interpolated at each altitude, as is now mentioned in 2.1. The IMAGES model is described in the next subsection of the manuscript.

16988, L17-21. See general comment above. Please discuss how this will affect your

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subsequent analysis and interpretation. This effect also needs to be acknowledged at later points in the paper during the satellite-model comparisons for biomass burning. A 40would yield a corresponding underestimate of the vertical column, leading to spurious agreement / disagreement with bottom-up inventories. It seems that given this issue, the comparison of spatio-temporal distributions between satellite and model should be weighted more heavily than comparisons of column amounts (i.e. correlation vs. bias), at least for areas strongly affected by fires.

The omission of the aerosol correction may lead to a significant underestimation of the derived HCHO column over fire scenes, by up to about 40%. This is now mentioned in the manuscript (Subsec. 2.1). The role of this underestimation is better acknowledged and taken into account when interpreting the model/data comparisons.

16989, L18-19. And the above-mentioned aerosol effect.

The description of the HCHO dataset has been shortened, and this sentence has been deleted.

16989, L24-25. But higher than that over fires, if the aerosol effect alone is up to 40%.

Correct. The paragraph has been modified.

16991, Section 3.2. The pyrogenic EFs vary with underlying vegetation, yes? This should be mentioned.

Correct. This is now mentioned in the manuscript (Subsection 2.2)

Figures 1, 2. I suggest combining Figures 1 2 into a single 2-panel figure. Also I recommend adding a 3rd panel showing the difference between the two; otherwise it is difficult to compare. But see my other comment about which emission inventories to include.

The figures are combined into a single two panel figure (see Supplement, Part A).

16993, L1-2. Is this problematic at all for mid-latitude fires?

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The diurnal profile used in the model calculations is an average profile. Although the work of Giglio (2007) clearly showed that diurnal cycles vary between different biome types, all are qualitatively relatively similar with low fire activity at night and a peak in the early afternoon. This is also the case for mid-latitude fires. At present, we have used the information available, which is an important step forward from using no diurnal cycle at all. In the future, merging geostationary data with 4 overpasses of the MODIS satellite and TRMM diurnal information may provide consistent spatially-explicit information. This information, however, is currently unavailable. To more quantitatively answer the reviewer, we performed a sensitivity calculation on the impact of diurnal profile of pyrogenic (run E2) in Subsec. 4.1.

16993, L14. Point out that neglecting monoterpenes is not likely to have an important effect given the references cited earlier and your subsequent discussion in Section 4.2. Consider moving some of Sections 3.2 and 3.3 to a Supplemental Information section in the interest of length. There is a lot of detail provided here which seems like pretty standard stuff.

The emission inventories are now described in the Supplement. The description of the formaldehyde dataset has been shortened (Subsec. 2.1).

Table 1. Since you have modified the IMAGES mechanism to mimic MCM, it seems redundant to include both the IMAGES and MCM HCHO yields. I suggest removing one of them, and simply stating in the text that the computed yields differ by < X% between the two.

For the sake of completeness, and since this does not truly lengthen the manuscript, we prefer to keep these columns in Table 1.

16998, L4. Only slightly? Give approximate amount or bound on the amount.

It is difficult to give precise numbers here, given the significant uncertainties in the laboratory results. The primary HCHO yield might be overestimated by about 10%, but

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this represents only a small fraction of the total HCHO yield from isoprene. More work is needed to quantify the uncertainties in the secondary HCHO yield from isoprene.

16999, L20. Recommend "5.1 Contribution of the different emission sources to the total SIMULATED HCHO column"

Corrected.

17000, L20. "Below the Tropics" is confusing. Within the Tropics? South of the Tropics?

Corrected. Within the Tropics.

17000, L21-22. But also point out that these are for different years so we don't expect them to be identical. This is true, but the systematic offset between GOME and SCIAMACHY at mid-latitudes cannot be due to interannual variability.

17004, L25-26. A bit more info? How big a region was the MEGAN average computed for?

The Tapajos measurements were compared with MEGAN at 0.5 degrees resolution. Details are given in Müller et al. (2008).

Figures 10-14. Captions should say Line colors are as in Fig 8, not Fig 10. Also consider instead restating the color scheme in each caption, as otherwise it is annoying for the reader to have to keep flipping back to the first figure.

The color codes are given in every caption, when applicable.

17005, L13. Suggest "in the dry season" instead of "in January".

Corrected.

17005, L16. Suggest "IN THE WET SEASON for most of the years."

Corrected.

17005, L18. Please indicate whether by "correlation" you mean R or R2 (I assume R)

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to avoid ambiguity.

We meant R. Correlation coefficients and biases are now summarized in Table 5 (for the burning season) and Table 6 (non-burning season).

17007, L17-25. Given the acknowledged retrieval bias over fire scenes, do you really want to make the argument that the findings of Fu et al. "are not supported by our comparisons"? If you do, then you need to demonstrate how this statement holds even given the potential retrieval bias.

The GFEDv2 simulation overestimates the HCHO maxima by 30-80% over Indonesia and by 0-30% over Indochina in comparison with the HCHO retrievals (see our new Fig. 9, also Table 5). Even when accounting for retrieval bias over fire scenes, these results are not compatible with the 5-fold increase of the biomass burning source suggested by Fu et al.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16981, 2008.

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