

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 the referee for his/her constructive criticism and comments which contributed to substantially improve the manuscript.

Please note that the main changes of the revised manuscript can be summarized in the following points.

- 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.

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- 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.

The authors present 10 years of satellite observations of HCHO columns with the goal of evaluating the HCHO simulation in the IMAGES model using different inventories for the biogenic and pyrogenic emissions in the model. They showed that the inventories provide a good description of the temporal variations of the observed HCHO columns, but that they exhibited regional biases. In particular, they showed that although the GFEDv2 pyrogenic inventory is supposed to be an improvement over the earlier GFEDv1 inventory, neither inventory reliably captures the observed HCHO variations in all regions. We now have available similar long-term satellite datasets for other important tropospheric trace gases and I believe this manuscript is an important demonstration of how these data can be exploited to identify deficiencies in our models.

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The material is appropriate for ACPD and I recommend publication with minor revisions to address my comments below.

Specific Comments :

1) Abstract: The abstract should be modified to acknowledge that the results of the manuscript must be interpreted in the context of the large uncertainty of the HCHO retrievals. As it is currently written, there is no mention of the 20-40% uncertainty of the HCHO data.

Done.

2) Page 16986, line 11: I would suggest changing the title of this section to "GOME and SCIAMACHY HCHO Columns".

Changed as suggested.

3) Page 16990, lines 20-24: How often are the diurnal coefficients (gamma) updated?

The diurnal coefficients are updated every month, now mentioned in Subsect. 2.2.

4) Page 16992, lines 26-28: What about during the 2006 El Nino? We know that there were record high abundances of CO in the southern tropics in 2006, but Figures 1 and 2 do not show unusually high HCHO. The authors should comment on this.

Figures 1 and 2 display the NMVOC emissions estimated from the bottom-up inventories GFEDv1 and GFEDv2. The Indonesian fires of 2006 are apparent in Fig. 2 (latitudes 0-5 S) and also in the HCHO column time series (Fig. 12). However, biomass burning was not especially high in 2006 in Southern Africa and South America according to both the inventories and the HCHO retrieval.

5) Page 16996, line 15: Is there a reference for the box model? If not, the authors should give more information about it.

References added.

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6) Page 17001, lines 3-5: *What about the Indonesian and Australian regions? The model also does not reproduce the observation well in these regions.*

Corrected.

7) Page 17003, lines 5-10: *The GEOS-Chem model is not shown in Figure 9 and it is therefore not clear what is the point of this comparison between IMAGES and GEOS-Chem. The authors should either remove the discussion about GEOS-Chem or explain more fully the context for this comparison.*

Agreed. We removed the references to the GEOS-Chem results, since they don't bring much to the discussion.

8) Page 17003, line 7: *It is not clear what the authors mean when they state that "over the ocean, the agreement is very good." I assume they are referring to the panel in Figure 9 for the Atlantic. But the model is biased high with respect to the URI data and low with respect to the NCAR data.*

Understood. We deleted the sentence, as it does not bring much to the discussion.

9) Page 17003, lines 10-12: *It is not clear to me how this comparison between IMAGES and GEOS-Chem and IMAGES and the aircraft data is a "validation" of the satellite data? In fact, although it is difficult to tell by inspection, it seems that the bias between the two sets of aircraft observations is comparable to the uncertainties in the satellite retrievals (40independent data is important, but I do not think this resolves the issue with the large uncertainty of the satellite product.*

Agreed. Sentence deleted.

10) Page 17007, line 22: *Typo "out" should be "our".*

Corrected.

11) Page 17010, line 18: *Typo "put" should be "puts".*

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Corrected.

12) Table 1: Remove the "a" after "Note" in the table description.

Corrected.

13) Figures 1 and 2: Please add the units for the emissions.

Corrected. The units are $\mu\text{g}/\text{m}^2/\text{hour}$.

14) Figure 7 caption: The regions are used in Figure 8 as well as in Figures 10-13.

Corrected.

15) Figure 10 caption: The line colors are defined in Figure 8.

The color codes are now given in all captions, when applicable.

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

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