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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #3

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Review of "Evaluating the performance of pyrogenic and biogenic emission inventories against one decade of space-based formaldehyde columns" by Stavrakou et al.

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 vari-

ations 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. The material is appropriate for ACP 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.

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

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

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.

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

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.

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.

8) Page 17003, line 7: It is not clear what the authors mean when they state that "over

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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.

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 (40%). I think the comparison with independent data is important, but I do not think this resolves the issue with the large uncertainty of the satellite product.

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

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

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

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

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

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

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