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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.

**T. Stavrakou et al.**

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The authors would like to thank Referee#4 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 Referee4.

- 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 scenes, as requested by all referees.

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

*Stavrakou et al. use the recently established long-time global dataset of tropospheric formaldehyde retrieved from GOME and SCIAMACHY (published in De Smedt et al.2008, ACP) to indirectly evaluate the performance of different global inventories of biogenic and biomass burning emissions. The evaluation is done by comparing the temporal pattern of observed HCHO columns over selected regions with HCHO columns that have been simulated with the IMAGES global chemical transport model using three different combinations of emission inventories, namely the GFEDv1 and the GFEDv2 biomass burning emissions inventories and the GEIA and the MEGAN-ECMWF inventory of biogenic emissions. This work is a first step forward to an improvement of available HCHO emission inventories, which is of great potential use for*



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*future atmo-spheric chemistry modelling studies. As consequence, I consider the paper a valuable contribution to ACP, provided that several modifications are made:*

General comments:

*1) The paper is too long. There are several sections that could be shortened without narrowing the scientific value of the paper. In particular, large sections of chapter 2 are redundant as they use almost the same wording as the description provided in De Smedt et al. 2008. It would be sufficient to refer to the detailed description of technical details/error estimation in De Smedt et al. (2008) and provide only a short summary here.*

The description of the HCHO dataset has been shortened. The description of the emission inventories is now included in the Supplement.

*2) The choice of the emission inventories, the statistics applied and the conclusions drawn out of the comparisons with the observed time series of HCHO columns do not convince me. a) The GFEDv2 biomass burning inventory uses improved datasets and methods compared to the earlier version GFEDv1. The GFEDv1 inventory in the simulation S2 (with MEGAN-ECMWF), however, appears to better reflect the observed seasonal patterns in some regions than the simulation with the GFEDv2 inventory (with MEGAN-ECMWF). The differences in the correlation coefficients (S2 and S3) are, however, generally small. To provide a convincing conclusion, the differences need to be tested for statistical significance taking into account the 'true' number of degrees of freedom (sample size adjusted for autocorrelation). Simple linear correlation of the time series of modelled columns with observed columns is, to my opinion, not sufficient to prove that one inventory is better than the other. The correlation analysis also needs to cover the anomaly HCHO columns time series (deviation from the average seasonal cycle in HCHO columns). If the outdated GFEDv1 performs significantly better than the GFEDv2 (in terms of statistical significance) in some regions, then a discussion of possible reasons (e.g. unrealistic fuel loads in GFEDv2) will be helpful. The conclusion*



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*should then contain recommendations in which regions the GFEDv2 inventory needs further improvements.*

We agree that the correlation coefficient alone cannot provide a convincing conclusion regarding the respective performance of different inventories, especially when the differences in the correlation coefficients are small. We have now summarized the calculated correlation coefficients and the average biases in Tables 5 and 6. These biases and coefficients are estimated separately for the burning season and for the rest of the year. The burning season is defined based on the precipitation rates and the pyrogenic emission estimates. The differences between the correlation coefficients are tested for statistical significance using Fisher's z-transformation, with the sample size being adjusted for (first-order) autocorrelation. The correlation coefficients are shown in bold in the Tables when the differences are found to be significant at the 95% confidence level. This analysis confirms that GFEDv1 performs better over Amazonia, whereas GFEDv2 provides a better match over Indonesia, and Southern Africa. Note that a positive bias (between 0 and 60%) is generally found between the model and the observations, except over Southern Africa (Table 5). This overestimation might be largely due to the omission of aerosols in the retrievals.

A detailed comparison between both GFED versions falls beyond the scope of our work, but is part of work being done for a new GFED version (van der Werf et al., in preparation) for which our work provides an important benchmark. Since none of the variables (burned area, fuel loads, combustion completeness) is validated we cannot point to the reason behind the discrepancies. We presented our work to show that inventories still contain large uncertainty and that a new version does not necessarily provide better results in all regions.

The third paragraph of the conclusions focuses mostly on the regional differences between the modelled columns on the one hand, and the two GFED version on the other hand. It includes a detailed summary over which areas GFEDv2 needs improvement.



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*b) The comments made in a) also apply to the difference between the GEIA and the MEGAN-ECMWF inventory (simulations S1 and S2).*

The significance test confirms that the MEGAN-ECMWF simulation provides a better match over many regions (Table 6).

*c) Given the large uncertainty of the observed HCHO columns as described in De Smedt et al. (2008) and as visible from the large error bars in Fig. 10-14, all three simulations (S1-S3) generally lie within the range of observed values. Consequently, all combinations of the emission inventories can be considered as a realistic estimate. What would be of potential interest to the atmospheric chemistry modelling community is to use the approach used in this paper to obtain realistic lower and upper bounds of HCHO emission estimates.*

All three simulations do not always lie within the error bars, as can be seen on Fig. 6 and Figs. 8-12. The derivation of improved emission estimates based on HCHO retrievals will be the subject of a forthcoming study.

*d) The title of your paper announces the evaluation of the performance of pyrogenic and biogenic emission inventories. Besides the GFED biomass burning inventories (one of which is outdated), there are other biomass burning emission inventories commonly used in atmospheric chemistry modelling studies that could have been included into this evaluation work (e.g. the regional inventory 3BEM (Longo et al. 2007, ACPD) and other inventories listed at http://www.aero.jussieu.fr/. Please discuss why your study focussed on the GFED inventories, only. To my opinion, the paper would gain substantial value if it contained a more comprehensive evaluation by including one or more 'independent' biomass burning emission inventories. (Independent in terms of the different methodology used by different authors. The fire satellite datasets are commonly the same, e.g. ATSR or MODIS fire counts).*

Our study focused on the GFED inventories because they are global and they cover the entire period of our model/data comparisons (1997-2006). We agree with the referee that the paper would gain additional value from comparisons with other biomass burning inventories. This will be addressed in future work.

Specific comments:

*1. Page 16982 Lines 1-3: Please mention the period your study is covering (i.e. 1997-2006).*

Done.

*2. Page 16982 Lines 23-27: Please provide a concluding remark at the end of the abstract (so far, you only present results).*

Done.

*3. Page 16995 Lines 1-8: Please provide a quantitative description of the differences between the GEIA and the MEGAN-ECMWF emission inventory used (e.g. in a table).*

Done (see Table 1 in the Supplement).

*4. Pages 16999 - 17000: You choose selected years for studying the contribution of different emission sources and to compare modelled HCHO columns with observed HCHO columns (Figures 4-6). How do you justify the selection of these specific years? Are the results different for other years?*

*5. Pages 16999 - 17000: Please restructure the section 5.1. The first sentence of Chapter 5.1 describes a model study focusing on the year 2006, only. In the subsequent, you present results of a model study covering the years 1997, 2000, 2005, which has not been introduced previously. This is confusing.*

*6. Page 17000 Lines 16-27: Please restructure this section. The first sentence of Chapter 5.1 describes a model study focusing on the year 2006, only. In the subsequent, you present results of a model study covering the years 1997, 2000, 2005, which has not been introduced previously. This is confusing.*



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Interannual variability is not a focus in this section and this is why we show results for specific years only. The exact choice of the years is unimportant. Interannual variations are presented in the following section. The respective contributions of the different sources to the HCHO columns are essentially the same in all years, except for biomass burning which exhibits marked interannual variations. The interannual variability of pyrogenic emissions is presented in Fig. 1 in the Supplement. A sentence has been added in the first paragraph of the subsection.

*7. Page 17001 Lines 23-26: Please mention data the observed HCHO columns are displayed as black error bars.*

Done.

*8. Page 17001 Lines 23-26: Please describe what regression model you applied to calculate the correlation coefficient between monthly averaged modelled and monthly averaged observed HCHO columns. I assume you applied a linear regression model with r is the Pearson's correlation coefficient. Please also mention whether your data are sufficiently normal distributed.*

A linear regression model has been applied. We now mention that r is the Pearson's correlation coefficient. A basic assumption of such analysis is usually that the data are normally distributed. The fact that this might not be always the case here should not alter the conclusions of this analysis.

*9. Page 17003 Lines 11-13: I cannot follow this conclusion. Please explain more. In Fig. 9 airborne measurements of HCHO profiles (NCAR/URI) are compared with two modelled profiles (S2 and S2(0.5 MEGAN-ECMWF).). How does this relate to an indirect validation of the 12-year GOME/SCIA dataset described in de Smedt et al. (2008)?*

We removed the assertion that the comparison with airborne measurements constitutes a validation of the GOME/SCIAMACHY dataset, especially because of the large



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differences between the NCAR and URI data.

*10. Page 17009 Lines 14-16: A high temporal correlation coefficient is not a sufficient indication that the model and the emission inventories are in good agreement with observations. Here, you discuss the agreement in the seasonal cycle, what about differences in the absolute values?*

The average biases as well as the Pearson correlation coefficients for the burning and non-burning season separately are now provided in Tables 5 and 6. Based on these tables the conclusion section is now reformulated.

*11. Page 17009 Lines 22-24: The difference in the correlation coefficients of S2 and S3 are generally too small to be statistically significant and thus counting such small (not significant) changes is not meaningful.*

We agree with the referee. In the revised manuscript the differences between the correlation coefficients have been tested for statistical significance using Fischer's r to z transformation and the regions where the differences are significant at the 95% confidence level are now clearly mentioned in Table 5 and 6. In view of these results, the conclusion section has been reframed.

Technical comments:

*1. Please standardise the way you write units throughout the entire manuscript. For example, you use Tg C/yr (e.g. Page 16982 Line 2), molec./cm$^2$ (e.g. Page 16994 Line 14), m$^3$m$^{-3}$ (e.g. Page 16994 Line 14), mg species/m$^2$/hr (e.g. Page 16994 Line 22) and molec.cm$^{-2}$ (e.g. Page 17000 Line 25).*

Done.

*2. Page 16983 Line 28: Please introduce the abbreviation of C$^{-1}$ (per unit carbon).*

The yields are now given as mol/mol.

*3. Page 16991 Lines 19-22: Please cross-reference to Table 1.*



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Done.

*4. Page 16993 Line 16: Please define k.*

As indicated in the text, the sum is considered over the layers of the canopy model.

*5. Page 16997 Line 726: Please use the same units as in Table 1. In Table 1, the unit is mol/mol. In the text, the unit is mol/C.*

Corrected. mol/mol is used throughout the paper.

*6. Page 16998 Line 25: In Table 1, the value is 66 Tg/yr, not 63 Tg/yr.*

Corrected.

*7. Page 17025 Fig.3. Caption: Mention that the time axis refers to local time.*

Done.

*8. Page 17030 Fig.8. Caption: Mention how the observations are displayed.*

Done.

*9. Pages 17032-17036 Figures 10-14 Caption: Line colors are as in Fig. 8 (not Fig. 10).*

Corrected. The color codes are given in every caption, whenever applicable.

*10. Pages 17032-17036 Figures 10-14: The minor tick marks along the ordinate are frequently not visible (see for example Fig 14 Sumatra). Please rescale and/or redraw the figures and make the tick marks readable.*

Done.

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



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