Atmos. Chem. Phys. Discuss., 8, S8538–S8541, 2008
www.atmos-chem-phys-discuss.net/8/S8538/2008/

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 #1

Received and published: 25 October 2008

The work presented in this article shows how satellite observations can be used to constrain biogenic and pyrogenic VOC emissions. The strengths of this work is to use long term (a decade) and state of the art global scale data sets of tropospheric formaldehyde columns (from GOME and SCIAMACHY) and of biogenic (MEGAN-ECMWF) and pyrogenic emissions (GEFD). A weakness is that conclusions drawn from the comparison between observed and simulated formaldehyde columns on the validity of emission data bases are overstressed, because they do not take into account uncertainties in observations and in simulations. A second weakness is that the modelling approach with the IMAGE model remains somewhat unclear because monthly mean and daily/hourly input data are mixed. All in all, this work is an interesting contribution to the field,

which should be published in ACP after correcting the two weaknesses pointed out.

The authors state that uncertainty in observed formaldehyde columns is about 20 to 40 %, depending on cloudiness conditions. In several parts of the globe, strong differences (several tenths of %) with formaldehyde columns derived from GOME by other groups (using other spectral windows and different set-up of retrieval methods) are also noticed by the authors. Last uncertainties in simulations with the IMAGE model are not assessed, but could be also significant. In the context, biases of a several tenths of % between simulated and observed HCHO columns cannot be related to the accuracy of VOC emission data bases, but are simply within the error bars of observations. This should be made clear, and it should be stated which discrepancies are larger than combined observation and simulation uncertainties. Still the phase (timing) of the seasonal and interannual variability can be analysed from this data-set. .

Implications of mixing monthly and diurnal/ hourly input data on the accuracy of simulations should be made clear. What is the impact of using monthly averaged wind fields (and of diffusion coefficients to mimic the synoptic variability) on the accuracy of simulations (section 3.1)? Have there been comparisons between IMAGE simulations and CTM simulations using wind fields more resolved in time (several per day)? Given the model time step of one day, a special correction (by off-line diurnal modelling) is applied to obtain a diurnal variation in concentration fields (necessary for example to compare to observations during satellite overpasses). What are the details of this method? Is is applied at a given day, for given 0D boxes ? Can the model uncertainty due to these and other issues be estimated (i.e. accuracy of OH fields? This would be important in order to correctly assign uncertainty to emission data.

Specific remarks :

Page 16989: How big is uncertainty and bias (with known sign) in HCHO columns introduced by neglecting the aerosol correction, especially for strong biomass burning events ? This again affects the utility of observations to constrain emissions in section

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Page 16993: It should be stated that biogenic emission inventories are monthly means (but with superimposed average diurnal variation). That is what I understood given the monthly average met. input data. The sentence “In addition to this, the emissions of the MEGAN-based inventory account for the diurnal, daily, seasonal and year-to-year variability” at page 16995 is then misleading.

Page 16998, section 4.3: The text suggests that HCHO production in the current study is calculated form emissions and yields given in Table 2. Please make clear that values in Table 2 are given only for a comparison purpose, and that HCHO formation is explicitly treated within IMAGE.

Page 16999 : Use monthly OH, HO2, NO, NO2 and NO3 fields for sensitivity tests Wouldn’t it be more coherent to include O3 in the list of fixed species ? It is an oxidant as NO3, and governs the NO/NO2 ratio.

Page 17002 : discussion of differences over North America Here conclusions are drawn on the relative validity of the GEIA and MEGAN–ECMWF data base from biases with HCHO columns. But at the same time, strong differences to other satellite data sets of HCHO columns are noted, which tend in my view to make the previous statement invalid.

Does the calculated correlation coefficient refer to temporal or spatio-temporal correlation ? Please make this clear. Later on, the text suggests spatio-temporal correlation (page 17003): “The high correlation coefficient values (0.9) calculated over the extended Amazonian region, Guatemala and Santarem yield strong confidence to the spatiotemporal distribution of the implemented emission inventories. ”

Page 17003 : “Note finally ….. in Palmer et al. (2006).” This paragraph is difficult to understand. Please clarify it.

Page 17005 : Comparison over Africa during biomass burning events Please indicate if

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the fact not to take into account biomass burning aerosols in the retrieval could explain the differences.

Page 17007, line 21 : typo by ouR comparisons

Page 17009, Conclusion : “The high correlation coefficients” Please note that for the extreme case of a seasonal variation given by a function (i.e. sinusoidal), correlation is only sensitive to the phase and not to the amplitude. Some of the seasonal variations (of monthly means) are not far from sinusoidal functions, so the interpretation of a good correlation should not be overstressed.

Page 17010 , line 18 : typo puts

Page 17011 “It is understood that our conclusions depend vitally on the quality of the retrieved columns. However, discrepancies among the retrievals, inherent to differences in the retrieval methods, exist between HCHO datasets. For instance, our GOME slant columns are by 5 about 30–40% lower than the Chance et al. (2000) dataset over North America (Palmer et al., 2006) and desert regions, whereas over central/Southern Africa, HCHO columns used in Meyer-Arnek et al. (2005); Wittrock et al. (2006) are by 40% lower than in the TEMIS dataset. “ The authors should be made clear which of the conclusions still hold given these discrepancies.

Figure 1 : What is the unity of the represented value ?

Figure 8 : What exactly represent the error bars ?

Figure 10 and following : Lines colors like in Fig. 8 (not 10)

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

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Interactive Comment