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Atmospheric Chemistry and Physics Discussions

Interactive comment on "How consistent are top-down hydrocarbon emissions based on formaldehyde observations from GOME-2 and OMI?" *by* T. Stavrakou et al.

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The authors would like to thank the reviewer for the positive evaluation of the manuscript, the careful reading and the useful comments and suggestions. Below we address the raised concerns. The reviewer's comments are *italicized*.

Stavrakou et al. utilize GOME-2 and OMI HCHO columns in an inversion with the adjoint of the IMAGESv2 CTM to evaluate the consistency of a posteriori emissions derived from the two different satellite sensors for anthropogenic, biogenic, and biomass burning VOCs. There was a good degree of consistency between the results obtained

for biogenic and biomass burning emissions, with some interesting regional differences. Impacts of specific events such as Russian fires and Amazonian drought are discussed. This work is useful in the context of several recent studies that have used one or more satellite HCHO products to estimate biogenic and/or biomass burning emissions of VOCs. It is generally well-written (though a bit cumbersome to read at times, see comments below) and the scope is certainly appropriate for ACP. I recommend publication after the following comments are addressed.

General comments:

1. *Since monthly averages are used in the inversion, it would be good to know how the variation in frequency of retrievals between the two sensors (due to cloud cover, etc.) is handled in the cost function. Differences in coverage are touched on a bit in the results sections (specifically with regards to solar zenith angle and cloud cover), which is helpful information that could be expanded upon. Are there any systematic differences between the two sensors that could be introduced in Section 4?*

   We have now clarified the handling of retrieval frequency (in Section 4): "The simulated monthly averaged columns are calculated from daily values weighted by the number of satellite (OMI or GOME-2) measurements for each day at each model grid cell." It follows that the variation in frequency of retrievals between the two sensors is taken into account.

2. *What are the implications of including isoprene as the only biogenic VOC in the a priori emissions? It seems like the seasonality of satellite-retrieved HCHO in some regions (particularly in the U.S.) is such that it peaks sooner in the summer than the model HCHO. Could this be due to the impact of VOCs that have earlier springtime emissions, such as methanol?*

Isoprene is actually not the only biogenic VOC included in the model and we apologize for not providing information on non-isoprene BVOCs. The model description has been expanded with the following text: “Monthly averaged biogenic methanol emissions (100 Tg/year globally) are taken from a previous inverse modelling study (Stavrakou et al., 2011) using IMAGESv2 and methanol total columns from IASI. Biogenic emissions of acetaldehyde (22 Tg/year) and ethanol (22 Tg/year) are calculated following Millet et al. (2010). The model also includes the biogenic emissions of ethene, propene, formaldehyde, acetone and monoterpenes from MEGANv2 (http://eccad.sedoo.fr/). Note that the non-isoprene biogenic VOC emissions are not varied in the source inversions."

Varying methanol emissions along with isoprene would not seem appropriate given the direct constraints provided by IASI methanol observations.

3. *It is difficult to navigate the discussion in Sections 6 and 7 with so many figures to flip back and forth through, particularly in regards to the discussions on biomass burning. You mention biomass burning results for the North China Plain for June, a month which is not included in Fig. 9, so you have also included the timelines of fluxes for different regions in Fig. 12. Is there any way to combine or condense this a bit? Also, some of the navigation would be helped if the text more explicitly referred to what aspect of a particular figure illustrates the point being made (i.e. rather than making a statement that ends with a reference to Fig. X in parentheses, I would start more statements like “The data for region Z in Fig. X show...").*

   Although we agree with the referee that the discussion might appear long and sometimes difficult to follow, we do not believe practical (nor desirable) to combine or condense the different figures presenting the emission updates. Whereas Figs. 9–11 present the geographical distributions of the updates for the standard (OMI and GOME-2) inversions, the Figs. 12–13 present seasonal variations of the emissions, not only for the standard simulations but also for the different

sensitivity inversions. However, to avoid confusion, the text has been amended where appropriate to make more precise references to the figures, as suggested by the referee.

Specific comments:

1. *Page 12024, Line 12: Is there any justification to be provided for the a priori uncertainty values used?*

   The error values are chosen so as to reflect the higher uncertainty associated with the fire source (factor of 3) and biogenic emissions (factor of 2). The error in anthropogenic emissions for OECD countries is assumed to be lower (1.5) than for other countries. We acknowledge that these values are somewhat arbitrary, and this is the reason why we present sensitivity inversions assuming either doubled or halved a priori errors (Table 2).

2. *Page 12026, Line 2: What numbers are being referred to here? The global ones? Looking at Table 3, OMI-HE corresponds to a 8% decrease from the a priori isoprene emissions, whereas OMI-DE corresponds to a 16% decrease. Correct? It would be odd if both produced larger isoprene reductions than the standard OMI inversion (∼13%).*

   It is now mentioned that the numbers are total estimates. As the reviewer points out, the standard OMI inversion leads to a decrease of 13% with respect to the a priori, whereas OMI-HE and OMI-DE produce a decrease of 8% and 16%, respectively.

3. *Page 12027, Line 19: Here it mentions that Fig. 14 contains a posteriori results for both the OMI and GOME-2 inversion, but only the OMI results are shown.*

The reviewer is right. The resulting modelled GOME-2 and OMI HCHO mixing ratio are very similar. We show only OMI-derived results in Figure 14 for the sake of simplicity.

4. *Page 12028, Line 12: How do we know this is not just a conflation of the isoprene and biomass burning emissions? It seems the isoprene increase in the OMI inversion is highly correlated spatially with the biomass burning emission increase (though different months are shown), whereas a posteriori isoprene over the rest of Europe is reduced relative to the a priori (in the GOME-2 inversion at least).*

   The referee raises a valid point. The discussion has been expanded as follows: "Note that, although the isoprene enhancement over Russia peaks earlier (July) and at slightly higher latitudes (ca. 61° N) than the biomass burning emission enhancement (55–57° N in August), the significant overlap of the two distributions makes impossible to rule out that pyrogenic emissions are the only cause for the observed strong formaldehyde columns. The very widespread extent of the observed formaldehyde plume cannot be easily explained by the comparatively much more localized emissions of the GFED3 inventory, and an additional, more widespread formaldehyde source (such as isoprene) could help to explain the observations. However, as discussed below, the GFED3 total emissions over Russia are likely largely underestimated, and their geographical distribution might also be in error. It is therefore possible that these fires were more widespread than in GFED3 and that strong isoprene emission enhancements are not needed to explain the observations."

5. *Page 12030, Line 1: I find it hard to see that the ratio of 13h30 to 9h30 columns is lower in the model than the satellite for Northern China in Fig. 7, given that the lines are all on top of each other. Can you include values of this ratio somewhere?*

   Agreed. The text in Section 7.3 is changed as follows: "This discrepancy is primarily due to the lower modelled ratios of 13h30 to 9h30 columns (average

   ratio of 1.0 in the model in North China between March and November) compared to the satellite datasets (average ratio of 1.16)."

6. *Page 12031, Line 27: The fluxes attributed to GOME-2 and OMI here are reversed compared to Table 3.*

   Corrected.

7. *Section 7.1 and Fig. 15: As the discussion here centers on August 2010, I am assuming that is the month shown in Fig. 15, but the figure caption says March 2010.*

   Corrected.

8. *Elimination of passive voice (phrases such as "is found to be" and "is estimated at") throughout the manuscript would improve flow and more concisely communicate the main points.*

   Done.