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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.

Anonymous Referee #2

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This is an interesting paper that describes the inversion of HCHO columns of GOME-2 (morning orbit) and OMI (afternoon orbit) in the IMAGES model, with the aim to optimise emissions of isoprene, from biomass burning and anthropogenic HCHO sources. Two consistent source of satellite data provide the interesting possibility to study the diurnal behavior, which is done in this paper. The results are interesting, but unfortunately, the paper is rather lengthy with many figures, which does not stimulate (full) reading. The paper provides an interesting contribution, after the following issues have been addressed.

Major issues

The structure of the paper is somewhat messy. An example is section 2.2. Here the reader gets very detailed information about anthropogenic VOC emissions and their chemistry without knowing the HCHO budget. This budget should certainly be included in the introduction. I also suggest to move section 2.2 to an Appendix, because it distracts from the main aim of the paper. In the introduction, the authors partly describe their method (e.g. page 12012, l24 and further). Also the paper is a bit short in referencing work of others, and how this study fits in existing knowledge. So, the introduction should be improved in this respect. Further, in section 3.1 reference is made to sensitivity simulations before they have been introduced. It would therefore be good to first do a complete method section, before the discussion of the results. Also, on page 12024, section 5, part of the method is introduced in a section entitled: "Overview of the results". A proper method section would certainly improve the paper. This also gives the opportunity to introduce terms like "cost function", terms that now pop up without any reference.

The description of the model is slightly misleading. As far as I know, IMAGES uses monthly mean meteorological fields to transport and mix the tracers. This important issue is not clearly mentioned. It would be good to add this, and also add a discussion of its potential impact on the inversion. I expect some impact on the inversion, because of difficulties of co-sampling the model with the observations and potential clear sky biases. Also, in comparing with aircraft observations on page 12027, line 11, there might be issues with monthly-averaged winds, and some words of caution are required.

In the discussion I also would expect some reflection of the separation of biomass burning sources, anthropogenic sources, and isoprene sources. In general, the inversion should give error reductions, and also the posterior co-variance terms that would reflect the ability to separate the different sources. I understand that an error estimate is ore difficult for a non-linear system, but the sensitivity experiments give some room for error discussion. But statements on page 12030, line 15: "Chinese isoprene emission are decreased from 7 Tg year-1 to 6.5 Tg (OMI) and 5.9 Tg (GOME-2)" need to be accompanied by error estimates. I cannot imagine that you can properly separate isoprene HCHO sources from other sources.

Units: please check all the units in the paper. They are often missing or incorrect (e.g. TG instead of Tg/year). Also check and add legends to figures. E.g. figure 12: does this show TG/month?

Minor issues

12009, l 23: units are missing

12009, l 25: add per year in the unit

12011, l 1: CO and H2 (add H2)

12012, l23: The inversion framework is assumed known to the reader. I think it would be could to describe this a bit better in the introduction, i.e. also by referring to earlier studies in this field by other groups.

12013, l2: Here method and introduction are mixed. I would prefer in the introduction references to studies that show the need for these sensitivity studies (e.g. associated with diurnal cycle of emissions). Referring to "inversion design" is a bit too short and methodological.

12014: l7: add unit kg/kmol (or g/mol)

12014: l14: "The African. . ..worldwide". Maybe good to add some cautious remarks here. Over peat fires (e.g. Russia in section 6.2) this assumption is certainly not valid, and maybe also not for boreal fires.

12015, l9: I miss somehow some recent references, e.g. Fuchs, H. et al. Experimental evidence for efficient hydroxyl radical regeneration in isoprene oxidation. Nature Geosci. 6, 1023–1026 (2013).

12017, l3: I would use something like (g CHCO/g OAHC) as unit here.

12018, l10 (and further on): on Fig. xx → in Fig. xx

12020, l14: please repeat that you evaluate the diurnal cycle in the column, and not in the near surface concentration.

12022, l3: here I wonder why the modeled HCHO concentrations in the boundary layer are not compared to observations. I agree that boundary layer mixing complicates issues here, but the authors should al least argue why they did not evaluate the model with other HCHO measurements. Also, by comparing only diurnal profiles they might hide deficiencies in the model.

12023, l29: on this figure → in this figure

12024, l16: Table 2 lists other sensitivity studies than described earlier in the discussion of the diurnal profiles. I suggest to include one table with all simulations performed.

12031, l8, acronym IASI is introduced, but was used before

12033, l3: contrasted → contrasting

12033, l14: Tropical Asia emissions have been studies using IASI: (Basu, S. et al. The seasonal variation of the CO2 flux over Tropical Asia estimated from GOSAT, CONTRAIL, and IASI. Geophys Res Lett 41, 1809–1815 (2014).)

12035, l27: and (to a lesser extent) meteorological parameters. It is unclear what is meant with this statement.

Figure 2: Please use a common y-ax metric.

Figure 9: the order of the panels does not make sense. Jan-mar-aug-oct? why not Jan, Apr, Jul, Oct?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 12007, 2015.