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Atmospheric Chemistry and Physics Discussions

Interactive comment on "FTIR time-series of biomass burning products (HCN, C_2H_6, C_2H_2, CH_3OH, and HCOOH) at Reunion Island (21 S, 55 E) and comparisons with model data" *by* C. Vigouroux et al.

Anonymous Referee #2

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This paper presents retrieval of total column amounts for a series of chemical species (HCN, C2H6, C2H2, CH3OH, HCOOH) from measurements by a FTIR spectrometer based at La Reunion Island. The authors show that biomass burning is the main driver for the observed variability and discuss comparison to model simulations in terms of emission inventory evaluation. The manuscript is well written and most of the methods used and analyses performed are clearly explained. The paper provides a good reference for the retrievals from FTIR at La Reunion, which have already been used in several studies. Therefore, I recommend publication with minor corrections

and clarifications, following the specific comments listed below.

Specific comments:

My main comment is that the data analysis regarding the seasonal and interannual variability remains somewhat too general. It could be improved by including a rapid analysis with respect to the emission inventories used for the model simulations for the same time periods (GFEDv2 for GEOS-Chem and GFEDv3 for IMAGES). These data are freely available and in an easily accessible format so that their analysis should not require significant work. It would help understanding the variability and the relative contributions from different ecosystem burning (available for GFED3 at least), an information that is critical for emission factors analysis. My second major comment is that although correlations to CO are shown, the comparison of the model simulations to that species are not shown or discussed. In my opinion, the analysis of the enhancement ratios would be more efficient if the reference (i.e. CO) was also evaluated with respect to observations.

Detailed comments:

Section 2.2.1: p. 13741: The treatment of water vapor and the use of "a priori" is not clear to me in the part (l. 17-18) and the following of the section. Water isotopologues are 1st retrieved and then these retrievals are used in the retrieval of the other targeted species. But since the authors mention that the water retrievals are used as "a priori", does it mean that they are fitted in parallel to the other species in the 2nd step of the retrieval?

Section 2.2.2, p. 13743: define WACCMv5, at least mention that it is a model. At the end of the section it would be interesting to discuss the possible biases due to the lack of sensitivity to the surface layer.

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Section 2.3, p. 13746: The authors introduce the Svar matrix but it is not clear why this can not be used as a priori variability Sa (since authors mention that there is no valuable information on this in the previous section). Also, on what bases was the 4km correlation length chosen?

P. 13747: For the interferences, the authors mention specifically the error associated to temperature, and also to the interfering species. What about water vapor? Can the specific error associated to this major interference be estimated precisely?

Section 3: Although the authors give a reminder of the results of another study for CO, it would be important for the understanding of the enhancement ratios to provide timeseries and comparisons to models for CO as well in Figure 4. The discussion of the seasonal and interannual variability could be more specific if related to variability in the GFED (v2 and v3 used for the model simulations) inventory in the main source regions for the considered time periods (with another figure). This would also help discussion in Section 4 (p. 13751 for instance).

p. 13753: using GFED could also allow the identification of vegetation types for the analysis of enhancement ratios in this section (specific ecosystem could be more easily related to EF numbers in the literature). For C2H2, could a shorter lifetime explain the discrepancies (with different chemical evolution)?

Section 5: GEOS-Chem and IMAGES are two different models and different emission inventories are used for the simulations: a discussion of the possible implications is necessary. What meteorology is used to drive the IMAGES CTM? Why not use GEOS-Chem for the organic compounds as well? For both models, enhancements

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compared to CO are discussed but this assumes that CO in the models is correct? Are both models consistent in terms of CO? Some discrepancies between model and observations depend on the season: a link to the origin of the air masses, their age, and the vegetation type burned may help understanding what can be learned about the emissions.

For methanol comparisons: any idea why the IASI constrain is not enough to match the FTIR measurements? Do both datasets agree for this time period? Is it due to the inversion procedure (errors, etc.)?

Technical comments:

p. 13745: avoid "..."

Figures:

Figure 1: it would be interesting to plot the variability of the a priori profiles.

Figure 2: The corresponding DOFs could be provided in the title.

Figures 2 and 3: Why is there a sharp gradient in the C2H2 AKs around 20km? (This needs to be explained in the text).

Figure 4: should add CO in the list of species – for reference.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 13733, 2012.