

## ***Interactive comment on “Global atmospheric budget of simple monocyclic aromatic compounds” by David Cabrera-Perez et al.***

**David Cabrera-Perez et al.**

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Received and published: 18 May 2016

We want to thank the referee#1 for the review and comments. We include the comments of the referee (in bold) followed by our replies.

**Abstract and throughout paper: Emissions, burden and loss rates of total aromatics should be reported in TgC as the individual aromatic species have different molecular weights.**

The authors agree with the referee comment, as it will be helpful (more intuitive) for an easier comparison of fluxes. Therefore, we changed the units to TgC in the manuscript.

**Abstract, I.4: the current wording implies the emissions are a result of the model**

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**simulation, where as the anthropogenic and biomass burning amounts are determined by the emissions inventory used. You might want to re-phrase that sentence.**

We rephrased the sentence in order to clarify this aspect: “Anthropogenic emissions provided by RCP database represent the largest source of aromatics in the model ( $\simeq 23 \text{ TgC/yr}$ ) and biomass burning from the GFAS inventory the second largest ( $\simeq 5 \text{ TgC/yr}$ ). The simulated chemical production of aromatics accounts for  $\simeq 5 \text{ TgC/yr}$ .”

**p.3, l.5: Guenther et al., 2012 should be cited here, and other places throughout the paper, instead of Sindelarova. Whenever you are referring to a fundamental or general aspect of the MEGAN biogenic emissions. Sindelarova presents an application of the MEGAN model, but did not develop the model, or determine which species have biogenic emissions.**

We added the citation Guenther et al., 2012. However, we think that citing Sindelarova et al, is also helpful for the reader, as it mentions the total emissions of toluene.

**p.3, l.10: Replace “Besides, there is” with “In addition, there are”**

We changed it.

**p.3, l.24: Place ‘e.g.’ at the start of the reference list.**

We changed it.

**p.3, l.26-27: re-write - not clear currently if Henze looked at SOA or not.**

The intention of Henze et al, is the quantification of the SOA formation by aromatics. We rephrased the sentence. “This work focuses on the gas phase chemistry of simple

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aromatics, hence neglecting any SOA production. Other global studies as (Henze et al, 2008) include SOA production as they were focused on the aerosol phase.”

**p.3, I.29: give the chemical formulae of each compound.**

We added them in the revised manuscript.

**Table 3: Units are g-species per kg-(dry matter burned) state more explicitly. Would be helpful to also list totals as TgC/yr.**

We clarified this in the revised manuscript. We express now the fluxes in TgC/yr.

**p.6, I.8: rewrite “does not present such amount” Do you mean doesn’t include them, or doesn’t indicate them separately?**

The sentence have been rewritten to clearly state that RCP includes open cooking emissions, although in lesser amount than in Johnson et al. (2013). “In the present study open cooking emissions are included within anthropogenic sources but the *RCP* database does not present such large phenol emissions.”

**p.6, I.15: Guenther et al., 2006 only presents isoprene emissions. Do you mean MEGANv2.1 (Guenther et al., 2012)?**

We want to thank the referee for noticing the wrong reference. We removed it and added the adequate one.

**Table 4: Observations are really the reference for the model, so it would be more appropriate to give the ratio of the models to the observations: MLIT/Mobs and MRCP/Mobs.**

We agree with the reviewer's interpretation and modified the table.

**Figure 2: How are the model results compared to the mountain sites? Do you interpolate the model value to the pressure of the observation sites (correct way), or do you just use the surface model value (probably not correct - as the model probably does not resolve the topography of the mountain site).**

We compared observations with the surface layer of the model, because the model altitude at the surface is similar to the altitude of most of the stations. Nevertheless, for a more accurate comparison, in the revised manuscript we made the comparison including the altitudes (we did for EMEP and EEA, not for LITERATURE as not always this information was available).

**Figure 4: I do not find this figure very informative - the model results in Asia are not visible. Small points could be used to indicate the obs. locations, then plot model vs obs in a scatter plot.**

We added an scatter plot next to the original figure in order to better compare observations and model mixing ratios. Nevertheless, as we mention in the manuscript, the purpose of this plot is merely qualitative due to the limitations in the data (observations).

**p.14, l.15: What do you mean by a “bad representation of the sinks”? That OH is too high? Explain further.**

As we mention in the conclusion “The uncertainty of the rate constant for the reaction of toluene + OH is about  $5.6 \pm 0.9 \times 10^{-12} \text{ cm}^3 \text{ molecule}^{-1} \text{ s}^{-1}$  at 298K, which implies a 30% error on the chemical sink estimate.”. We substitute “bad” to “to strong model sinks in those regions” in order to correct this in the manuscript.

**p.14, I.16-18: How do you treat the observations that are below the detection limit? Rewrite these sentences.**

We did not include observations below the detection limit in the comparison with observations. However, we are aware of the consequent overestimation in our calculations. The sentence has been modified to make this clear. “In this case, 46% of the CARIBIC observations for toluene are below detection limit, which partially explains the bias. We only use the other 54% of the data for the calculations in table 4.”

**Fig. 6: The large difference at Hohenpeissenberg could be due elevation differences between model and obs. (see comment about Fig. 4).**

Model height for the grid box where Hohenpeissenberg station is contained, has only few meters difference from the observed altitude. Thus, we exclude this reason as a source of the large underestimation found.

**p.15, I.7: HCs are not removed by wet deposition.**

In the model wet deposition process is included for aromatic HCs. However the contribution by this process is negligible. We have now stated it clearly in the manuscript.

**p.17, I.9: ‘Coherent’ → ‘Consistent’. I don’t understand what the location of the emissions (NH) has to do with the seasonal cycle.**

The global atmospheric burden is dominated by the northern hemisphere emissions, this explains the large concentrations observed in the NH in comparison with the SH. Therefore, increases in the atmospheric burden in winter on the NH due to the decrease on OH concentrations explain the changes in the global burden.

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**p.17, l.13: 'totals' instead of 'sums up [to]'**

We changed it.

**p.17, l.15: rewrite to: 'on the order of a day or less'**

We changed it.

**p.18, l3: 'asses' should be 'assess'**

We changed it.

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Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-996, 2016.

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