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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global impact of monocyclic aromatics on tropospheric composition" *by* David Cabrera-Perez et al.

Anonymous Referee #1

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The manuscript by Cabrera-Perez et al. discusses the impact of aromatic species on the tropospheric composition using the global chemical transport model MESSy/EMAC. A model simulations with aromatics included is compared to a simulation without considering aromatics and the impacts on key species such as OH, Ozone, NOx, NO3, HNO3, HCHO and glyoxal are explored. This manuscript is strongly based on Cabrera-Perez (2016), which describes the aromatic chemistry added to EMAC and evaluates the global distribution of modeled aromatic species. While I acknowledge the importance of studying how aromatics impact the chemistry of the troposphere, I see limited scientific advancement in the presented study. What is it that really can be learned from this work aside from that aromatics play a role in tropospheric chemistry? For this reason and because the manuscript rather represents a model description, I

see it better suited for a journal like GMD. Major Comments: Aromatics come from a number of different source types including both anthropogenic and natural sources. Simply turning off all aromatic species in the model independent of their origin or their reactivity does provide little practical insight. The study would provide more value if it were to look at individual source types and/or individual aromatic species. The study is conducted at a rather coarse horizontal model resolution (1.875 degrees). At this resolution urban chemistry cannot be represented, yet aromatics play an important role specifically in urban regimes. The authors themselves state in Line 260 that the coarse resolution limits the representation of urban chemistry. So why was the study not done at higher model resolution or with a higher resolution regional model? Has the aromatic chemistry in EMAC been compared to a fully explicit chemical scheme? If so, how well does it represent the full chemistry? The authors list a large range of uncertainties that impact the presented results and reduce confidence in the value of the conclusions. I agree that emissions in general can pose, in parts, large uncertainties. Have the authors looked at other inventories (HTAP, RETRO, NEIs, ...)? What was the reason for selecting IPCC emissions? What is the reason for the high ozone bias in ECAM? Could the chemical scheme be a cause? Line 362: Do the authors imply that MCMV3.1 is incorrectly representing ozone chemistry or that the lumped chemical scheme that was developed based on MCMv3.1 and used in the current study has problems? If so, how much can one trust the derived sensitivity of tropospheric chemistry to aromatics? Does including HONO help with this problem? Has this been tested in a box model? Given the large role of SOA in aromatic chemistry, should such a study not have been conducted with a more detailed aerosol scheme that represents SOA formation. There is zero evaluation of the model with observations. Aromatic species themselves have been evaluated in Cabrera-Perez (2016), but what about e.g. O3 or CO?

Minor Comments: Line 55 (and also Line 195): This is a very simplified description of ozone chemistry. High NOx regimes do not equate net ozone formation as at very high NOx concentrations ozone can actually be lost. The actual urban chemistry cannot be resolved at the coarse model resolution applied in here.

Line 125: Given how closely this manuscript is linked to Cabrera-Perez (2016) I suggest referencing some of the major findings of this study as they apply to this work.

Figure 5 and Figure 6: Is this daytime? Daytime and Nighttime? Most Figure Captions require more details on what is shown.

The authors discuss the role of a number of intermediate species. I suggest adding the chemical reactions for the aromatics as included in MESSy in the Supplement or at least include a reference to where the chemical reactions can be looked up.

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