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Interactive comment on "Global Simulation of Semivolatile Organic Compounds – Development and Evaluation of the MESSy Submodel SVOC (v1.0)" *by* Mega Octaviani et al.

Anonymous Referee #2

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The paper describes modeling of the physical and chemical processes of PAHs in a global chemical transport model. Correctly modeling PAHs is important in addressing their adverse health impacts on both human being and the ecosystems. The topic is certainly within the scope of GMD and is of interest to the modeling community. The paper is well-written. I would recommend minor revision before accepted for publication.

First of all, the title of the paper is "Global simulation of semivolatile organic compounds . . .". However, it appears that it was particularly developed for modeling PAHs so a more specific title should be used to reflect this. If the model is intended to be

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applied for other SVOCs, this should be clearly stated in the manuscript. Parameters for all other SVOCs treated by the model should be described and model evaluations should be performed. Regarding the “global” in the title of the paper, the main text does not even include a figure to show modeled global distributions. Land surface concentrations of PAHs in different regions (Asia, North American, etc.) have been published before so a global distribution plot would allow the reviewer to compare these with previous studies.

The authors did an excellent job of investigating the factors that affect the modeled concentrations. However, in my opinion, this is somewhat an overkill because many parameters in the model detailed treatments carry large uncertainties. For example, the ppLFER scheme requires partitioning coefficients for more aerosol components. Uncertainties in these parameters may lead to a different judge on their impact on the predicted concentrations comparing to the base model.

The authors appear to imply that the model runs with the most sophisticated treatment of the processes gave the best results as they only presented these results in the model evaluation section. I have a few comments here: (1) models with more detailed processes might not provide the best results due to compensating errors in the model. The authors should compare the model performance with the simpler treatment of the processes (e.g. using the Lohmann−Lammel scheme vs. with the ppLFER scheme; using annual emissions vs. seasonal varying emissions). (2) the model was configured at 2.8 degrees horizontal resolutions thus cannot resolve local gradients when the monitors are not in the remote areas that can represent the average concentrations represented by the grid cells. Are the monitors used in the analyses selected to filter out the non-remote sites? (3) What’s the model performance of BC, total PM and size-resolved PM? What about gaseous pollutants (O3?) The authors didn’t mention these in the manuscript. Without these, it is hard to further understand the bias in the model predictions. (4) the authors have included some discussions on comparing with results with GEOS-Chem. As the resolution, emission inventories, model time spans

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are all different, this appears to be of less value and can be considered to move to SI. There is a tendency these days to write overly long papers with I am not a big fan of.

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