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Geoscientific Model Development Discussions
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Interactive comment on "A new module for trace gas emissions in ICON-ART 2.0: A sensitivity study focusing on acetone emissions and concentrations" *by* Michael Weimer et al.

Anonymous Referee #2

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In this study, the authors have newly introduced an emission module and a simplified OH chemistry module into ICON-ART aiming at simulations of VOCs. The VOC targeted in this study is acetone. The emission module has two options: the one is offline, in which external emission data are prepared in advance and are read during simulation, and the other one is online, in which emission values are calculated during simulation. In this study, the online emission module of MEGAN2.1 is implemented to simulate biogenic acetone emissions at a diurnal cycle scale. The OH chemistry module includes reactions related to CO, CH4, and acetone. In the model evaluation, the authors have compared simulated acetone VMR values with those observed by IAGOS-CARIBC in the UT/LS region and they have claimed that the developed model

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performs reliably.

I understand that the previous study of Rieger et al. (2015) first developed ICON-ART specifically for aerosols and this study has extended that for trace gases. The ICON itself is a relatively new model and its application to the atmospheric chemistry is interesting. Therefore, this study is suitable for Geoscientific Model Development. However, I recommend major revision on this manuscript before publication.

Major comments:

As the title says, the emission module is claimed as the new topic, but readers cannot agree with this. Both the offline and online emission modules employ commonly-used techniques and are nothing new. Furthermore, descriptions of the off-line emission module are too technical and not suitable in the main text. I recommend to move most of the descriptions in Section 3.1 to a supplementary document as a sort of manual. Only descriptions of emission inventories used and Fig. 5 may be left in the main text.

Comparing only with IAGOS-CARIBIC is not sufficient and more evaluation analyses are required. The evaluation only with the UT/LS data might be misleading, if the model vertical transport, which is often very uncertain, is wrongly simulated. Surface station data may be available and they should be compared with the simulated values in addition. Furthermore, I cannot understand why the authors limited the IAGOS-CARIBIC data to the mid-latitude UT/LS region. I think tropical data and vertical profiles (if available) are also useful to evaluate the overall performance of the model. Furthermore, one more result with MEGAN-Online LAIsun, which is newly introduced in this study, is needed to be shown in the sensitivity test.

Minor comments:

Title: As stated above, “a new module for trace gas emissions” seems inappropriate.

P.1, L.12: Insert a space between “dominated” and “concentrations”

Introduction: What is the benefit of using ICON for atmospheric chemistry studies?

Please discuss about that. Also, other previous studies in which similar icosahedral models (other than ICON) are used for atmospheric chemistry should be cited; for example,

Suzuki, K., T. Nakajima, M. Satoh, H. Tomita, T. Takemura, T. Y. Nakajima, and G. L. Stephens (2008), Global cloud-system-resolving simulation of aerosol effect on warm clouds. Geophys. Res. Lett., 35, L19817, doi:10.1029/2008GL035449.

Elbern, H., J. Schwinger, and R. Botchorishvili (2010), Chemical state estimation for the middle atmosphere by fourâĂŘdimensional variational data assimilation: System configuration, J. Geophys. Res., 115, D06302, doi:10.1029/2009JD011953.

Niwa, Y., H. Tomita, M. Satoh, and R. Imasu (2011), A three-dimensional icosahedral grid advection scheme preserving monotonicity and consistency with continuity for atmospheric tracer transport. J. Meteor. Soc. Japan, 89, 3, 255–268.

Goto, D., et al. (2015), Application of a global nonhydrostatic model with a stretched-grid system to regional aerosol simulations around Japan, Geosci. Model Dev., 8, 235-259, doi:10.5194/gmd-8-235-2015.

P.2, L.28: “to to” => “to”

P.4, L.9: What is the overbar of rho?

P.8, L.14: I cannot understand the summation in Eq. (2).

P.9, L.1-2: These sentences are not clear to me.

P.9, L.9: The biomass burning emission seems duplicated. The MACCity inventory includes biomass burning, while another explicit biomass burning data of GFED is also added.

P.11, L.7: “leaf area index” => “leaf area index (LAI)” P.11, L.8: Delete “(LAI)” P.11, L.11: “leaf area index” => “LAI”

P.13, L.5: Why is the online emission so much higher than the offline one, although they are made by the same MEGAN?

P.14, L.4: What of Sander at al. (2011) is used?

Section 4.2: Is this reaction method for the stratosphere similar to those of other models?

P.16, L11: "(IFS)" Please cite a paper and list it in Reference, not describing the URL in the footnote.

P.16, L.20-P.17,L.1: "The air pressure corresponding. . .in the CH4 VMR." This reason is not enough for the validity of using 1ppmv CH4 as the threshold.

P.16, L.14: "110 to 261 and 373 to 528" Are they flight numbers? And where did the aircraft fly to? Please clarify.

P.17, L.20-21: "All the simulations . . . in the tracer concentrations" is not clear to me.

Appendix A: Description of tau is needed somewhere.

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