

Review report of Hoshyaripour et al. 2025

In this manuscript, the authors present a comprehensive description of the latest developments in the ART atmospheric model as coupled to the ICON meteorological model (ICON-ART v2025.04). They have pitched this model as a novel tool which allows for a unified simulation of atmospheric composition and climate across scales without compromising on any key aspects of atmospheric chemistry and physics. That is, it represents key processes such as atmospheric chemistry for trace gases and aerosols, aerosol growth dynamics, and their interaction with meteorology via radiative transfer, aerosol-radiation interactions, and aerosol cloud interactions allowing for atmospheric composition-climate feedback simulations at varying scales within a single global framework. This, as I understand it, eliminates the need for running separate limited-area models (LAMs) that depend on lateral boundary conditions from coarse global models which often introduces abrupt and unrealistic meteorological features within the simulations and also lead to inconsistencies in the treatment of physical and chemical processes.

The authors have done an excellent job of detailing all key modules within the modelling framework, from emission processes and sources, chemistry schemes which vary from simplistic to sophisticated, deposition schemes, aerosol sources ranging from natural to anthropogenic, multiple mixing states, aerosol dynamics, aerosol-radiation interactions (which now incorporate a novel ML-based approach which better treats aerosol mixing states and results in a more accurate radiation perturbation), and aerosol-cloud interactions. They have also described multiple ways in which the aerosol-radiation interactions are called within the model which allows the user to study the direct vs holistic/interactive impacts of aerosols of different sizes on climate variables.

They have also provided a fairly clear description of the code infrastructure and the coupling between ICON and ART models (although such things remain largely opaque to basic model users unless they're willing to do the due diligence of probing into the code themselves, but it's a good starting point). The authors have also discussed the portability of ICON-ART on GPUs for potential speeding-up of simulations, which is a work in progress; it's good to see this forward-thinking. Standard configurations are also discussed which show the model's flexibility for various applications where there's more focus on certain processes and less on others.

Overall, I have to say this is very impressive work - both the actual development of the model and its clear documentation in this manuscript. Therefore my comments are fairly minor. I have mentioned them pointwise below:

L18: *“essential for improving predictions related to weather, renewable energy, climate change, air pollution...”*
consider changing to:

*“essential for improving predictions **and understanding** related to weather, renewable energy, climate change, air pollution...”*

L59-60: *“OEM enables efficient processing of emissions that are constant in time or changing only temporally, but not spatially”*

This is not clear to me (and may also bother other readers): if different sources are varying differently temporally, it means emissions overall are changing spatially too - please clarify this.

While the paper excels at describing what has been implemented, it could be strengthened by briefly showing why some of the new developments matter in a more quantitative sense. For example:

Section 2.4 (Sea Salt): The new Grythe et al. (2014) parameterization is included to better represent SST dependence. A simple zonal-mean plot or a brief statement quantifying the typical change in sea salt emissions or burden in tropical regions compared to the older scheme would be highly illustrative.

Section 4.4 (Subpollen Particles): The parameterization for SPP release is described. It would be beneficial to include a sentence stating the typical order-of-magnitude contribution of SPPs to total aerosol number concentration or CCN in relevant regions during pollen season, even if citing another study (Werchner et al., 2022?).

Section 3.2 (Detailed chemistry mechanisms): a brief comment on the typical computational cost increase when moving from a simplified chemistry scheme (like Linoz) to a full mechanism (like MOZART-T1) would provide valuable context for users planning simulations. A percentage increase in runtime, similar to that provided for LINOZ in Section 3.1.3, would be sufficient.

Figure 6: The inner and outer circles are unclear (visually and also in terms of values). For the winter plot (left), the outer rings mostly match the surrounding areas on the contour map but for the summer plot (right) these outer rings are consistently of a lighter shade than the surrounding values in the map which suggests some issue with sampling - please double check. If I disregard the outer rings and only compare the inner circle values with the surrounding values on the simulated map, I see a better model-obs agreement. However, when comparing the inner circles with outer rings, it looks like the model is underestimating surface ozone in both winter and summer. This underestimation doesn't sit well with the broader context of basically all global and regional models overestimating Northern Hemispheric surface ozone (e.g., Young et al., 2013; 2018; Ansari et al., 2025; Nalam et al., 2025, Gao et al., 2025). I suggest that the authors make this figure simpler by only showing one solid circle representing only observed values, and include the overall mean bias, RMSE, and correlation coefficient r for both seasons somewhere in the figure and the text. Accordingly, the text that *“the model accurately reproduces...”* should be made more nuanced and discussed in the broader context of the

aforementioned papers. The authors must also mention which emission inventory was used for these LAM simulations over Europe. The authors should discuss potential reasons for O₃ underestimation.

L432: *“hats and overbars”*?

L491: *“processes such as removal processes”* to *“processes such as removal mechanisms”*?

L493: Describe the key aspects of this alternative method in a couple of sentences here, especially in relation to its computational efficiency.

L571: *“implemented in other models”*: name those models here along with the citations.

L669 (or thereabouts): Also include a couple of sentences on the best practices of using this dusty cirrus parameterization for different (coarser, finer, or variable) grid resolutions. How does it perform across scales? Has this been tested? This could be discussed a bit.

Figure 11: The average OLR value should be shown in enlarged font or ideally printed over the map, or the reader might miss it. The technical name of the simulation experiment is not needed on the figure.

Figure 12: This schematic could be improved: include additional boxes at the top showing input data (for both ICON and ART). Name some typical variables (winds, moisture, pressure; anthro emissions). Similarly, name some typical output variables from ICON and ART; add additional boxes if necessary. Use appropriate arrows along the lines to indicate the direction of control and sequence of execution and data flow. Aim to better depict the loops and subloops within the model time integration workflow. In the caption, consider changing *“circles”* to *“loops”*.

L698: *“since also diagnostic variables can be defined with it”* to *“since diagnostic variables can also be defined with it”*.

This manuscript is an exemplary model description paper. It is extremely well-written and thoroughly documents a critical tool for the atmospheric modelling community. The suggested revisions are minor and are aimed at making an already excellent paper even better. I strongly recommend its publication in GMD once these comments are addressed.

References:

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Gao, Y., Kou, W., Cheng, W., Guo, X., Qu, B., Wu, Y., et al. (2025). Reducing long-standing surface ozone overestimation in Earth system modeling by high-resolution simulation and dry deposition improvement. *Journal of Advances in Modeling Earth Systems*, 17, e2023MS004192. <https://doi.org/10.1029/2023MS004192>

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