

Geosci. Model Dev. Discuss., referee comment RC1  
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## Comment on gmd-2022-80

Anonymous Referee #1

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Referee comment on "OpenIFS/AC: atmospheric chemistry and aerosol in OpenIFS 43r3"  
by Vincent Huijnen et al., Geosci. Model Dev. Discuss.,  
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Summary: The paper describes in some detail the chemistry and aerosol module coupled to the OpenIFS model. The paper is well written, I don't have any major comments that would require any major revision of the paper. One comment is on the separate treatment of tropospheric and stratospheric chemistry in OpenIFS/AC in different modules, which I consider unusual and possibly worth revisiting in future releases. These two domains share a lot of important reactions (e.g. methane oxidation, NO<sub>x</sub>/HO<sub>x</sub>/O<sub>x</sub> chemistry, CO) that would be duplicate and possibly differently represented in the two modules. Also there is a trend in tropospheric chemistry to consider halogens in the troposphere. Again there would be a lot of overlap of the tropospheric chemistry package with stratospheric halogen chemistry. So I wonder whether a single unified chemistry scheme (with perhaps options to vary aspects with this scheme, such as represent or not represent individual source gases or processes) might be the way to go. This is standard in other models. It would also eliminate the need to define an interface between the two schemes. I imagine the present approach is partly pursued because it allows for some simplification of the two separate chemistry schemes, and hence some savings in computational cost.

How does the model perform at lower resolution? Many (academic) users whom this release is targeting will struggle to afford to run a model with full chemistry at TL255L91. If there are any insights around that, it would be good to comment on these qualitatively.

A little more mention of the CMIP6 model EC-Earth3-AerChem, and how the package presented here relates to this model, might be helpful. EC-Earth4 is mentioned on p3, but I'm unclear whether EC-Earth3-AerChem (which has only recently appeared in the CMIP6 archive as a late addition) is the same as the model described here, or else what the relationship is.

The model supports two solvers: EBI and the predictor-corrector Rosenbrock solver.

Ideally the choice of solver would have no discernible impact on the results. Is that indeed the case?

On the whole, subject to the minor revisions that I suggest here, I recommend publication of the paper in GMD.

Minor comments:

P1L21: Please correct citation (Naik et al., 2021?)

P2L34: There are examples out there where the composition model operates at a lower resolution than the meteorological model, e.g. MRI-ESM2-1. There are upsides and downsides to this approach (some cost saving while retaining small-scale dynamical features, but inconsistent transport on the two grids).

P2L48ff: Here's where a mention of EC-Earth3-AerChem would be good to have.

P6L160: Offline photolysis is probably fine for the stratosphere but also likely creates inconsistencies with the approach used in the troposphere. A unified approach to photolysis would be desirable in the future, e.g. the Cloud-J scheme by UCI/ Michael Prather.

P10L246: Indeed online dry deposition is preferred, e.g. using the Wesely scheme,

P11L291 ff: I admit that the filenames mean nothing to me. This level of technical detail would be best left to an operator's manual or an appendix.

P12L328: Insert "and" before "reactions".

P14L360: Lower-case "this".

P14L381: ERA5 has superseded ERAI. Is there a particular reason for you to use ERAI? Also, when nudging typically there would be a cut-off wavelength whereby nudging is only applied to longer wavelengths than the cutoff. Is this used here? What's the cutoff

wavelength?

P27L594: Replace "as" with "different from".