EGUsphere, referee comment RC2
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Comment on egusphere-2022-226

Anonymous Referee #2

Referee comment on "Implementation and evaluation of the GEOS-Chem chemistry module version 13.1.2 within the Community Earth System Model v2.1" by Thibaud M. Fritz et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-226-RC2, 2022

Fritz et al present a landmark overview of a well-designed and implemented configuration of the Community Earth System Model incorporating the GEOS-Chem module. This constitutes a significant technical achievement and represents a very impressive step forward in model capability. This paper fits well within the scope of GMD and I believe is suitable for publication after consideration of the manuscript structure and some further thought is given to the level of detail of the discussion.

As the authors note on L377, the use of the same host ESM allows the differences in results to be attributed to the two chemistry modules. The new configuration offers the possibility to perform interesting chemistry module intercomparisons and offers the possibility to work towards a better understanding of the role of the chemistry scheme, and other processes connected to chemistry (aerosol processes, wet and dry deposition), in determining model performance and intermodel differences.

At this stage, that goal is still someway off, which is understandable given that this is the first paper from this project. There remains a number of differences in the implementation of key processes that inevitably lead to intermodule differences, and it will require further work to unpick the role of, say, the different dry deposition or aerosol schemes in driving differences between the two modules. The CESM-GEOS-Chem framework does allow this work to begin, but the authors might wish to say more about what possibility exists to harmonise further these key processes between modules and to further increase the modularity of the chemistry schemes. This would better facilitate being able to swap between chemistry module process-level treatments to improve attribution which is an important goal, and I would say is the most important potential outcome of this work.

This is a big paper that is doing the work of two or three: it is a description of the technical changes required, a description of the model configurations, a model/module intercomparison paper and a model evaluation paper. This is not to criticize, but it does

serve to illustrate the rather huge task of the essential role of model description and evaluation. However, I do wonder if the paper has become rather overlong. The evaluation itself is often rather cursory and little is gone into in detail. This puts the success of the paper in some jeopardy - the scope is impressive but the level of detail occasionally leaves the reader hanging, and for specialists it does run the risk of being rather unsatisfactory.

The paper performs a comparison between model configurations using zonal mean O3, surface O3, aerosol mass concentration, NOy, Bry and Cly, as well as an evaluation agains observations of surface NOx,O3, ozone profiles (2016 model year vs climatology), satellite O3 (2004-2010 period for troposphere, stratosphere and total column), total CO column (2016 vs 2003-2013 climatology) and wet/dry deposition (fluxes at various stations 2005-2007). The use of different observational periods for the intercomparison could presumably be addressed with a longer transient, but the text is reasonably caveated on this point.

The intermodule/model comparison is really interesting. The paper describes a whole atmosphere chemistry scheme, and so some whole-atmosphere evaluation is performed, particularly for O3/NOy/Bry/Cly. I think the impact of the structural differences in the model is probably the main result in this paper - wet deposition and Cly/Bry sources are frequently mentioned - so breaking discussion down into C-GC vs C-CC for most of the evaluation and considering in a separate section the offline S-GC runs might make things a bit simpler to follow, not least as there are huge differences arising from the different meteorologies that frequently dominate the S-GC runs, making the comparison not one between modules but more between models at a high level, i.e. between CTM-style offline meteorology and GCM-style free-running experiments, which is interesting but perhaps muddies the waters.

Better understanding the drivers of inter-module differences would be welcome. I think the manuscript would be improved significantly by examining not just the levels of key species but also the factors controlling the level of their reservoirs in more detail. The manuscript would be improved significantly if this would go further and address the species' budgets, quantifying the inputs and outputs between the modules. While biogenic emissions are compared, it would help - from an ozone evaluation point of view - to add data on other ozone precursors such anthropogenic, soil and LNOx to this table. Similarly, sink terms in the ozone budget would also be beneficial. A table similar to Table 1 in Tilmes et al. Geosci. Model Dev., 9, 1853–1890, 2016 would be ideal for the purposes of comparison. Putting more results into such tables would be helpful for the specialist reader.

Similarly, ideally, where key parameters or processes are identified, it may be useful to add references that indicate how the model configurations/chemistry was tuned/optimised when that model configuration was produced (e.g. is it possible to say how the sulfate dry deposition was evaluated originally in CESM2 and GEOS-Chem that means the deposition rates are so different?). This would give some traceability of the model configuration to the evaluation paper.

Specific points

The level of detail is rather variable in section 2.1

Figure 1 and L161-165 - I am not sure of the timing of the various calls to dynamics, physics and chemistry - can the authors expand on why dynamics does not modify the atmospheric state in the diagram? What order are the routines (physics/chemistry) called in?

Section 2 would benefit from a summary table that lists configurations side by side, e.g. aerosol scheme, dry deposition, as in e.g. the supplementary to Turnock et al., Atmos. Chem. Phys., 20, 14547–14579, 2020.

Section 2.3.4 repeats some of the detail in L328 and L185.

L366 missing words after to ensure

L470-481 if the authors prefer to keep the three-panel structure (see comment above) it would be helpful to describe the figures in the same order that they are presented (L-R)

L587 'emission' regions?

L617 what understanding of the ozone and aerosol do the subsequent analyses aim to improve? What beyond assessment of model skill does the comparison with observation aim to do?

L648 are the aerosol reactive tendencies stored? Can this be further assessed?

L657 reads strangely