Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
https://doi.org/10.5194/gmd-2022-97-RC1, 2022

Comment on gmd-2022-97

Anonymous Referee #1

Referee comment on "Tropospheric transport and unresolved convection: numerical experiments with CLaMS 2.0/MESSy" by Paul Konopka et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2022-97-RC1, 2022

This paper describes a new version 2.0 of the CLAMS-MESSY chemistry-transport model. The model is Lagrangian and follows isolated air parcels (APs) around the atmosphere, being required every so often to remap the parcels because there will be regions with a glut, and regions deserted by the APs. The real art here is remapping onto a std grid or figuring out how to remove and add parcels. The new model presented here includes inter-Parcel exchange via "parameterization of tropospheric mixing and unresolved convection." What is worrisome here is that there is no clear atmospheric physics to determine the rate of mixing, but rather it is just tuned and is defined to represent a type of mixing. It appears that CLaMS-2.0 has already been published and this paper is an application of it. If so, is this a GMD paper or an ACP one? I am not too worried about which, but the editor may wish to weigh in.

Overall, this is a reasonably nice paper, written clearly and deserves to be published after some thought and revision. I include comments by line number below.

L15 "The second most important transport process considered is unresolved convection." I remain very confused by this term: if the convection is not resolved in the EC fields (which it is resolved explicitly in the fields I use from the IFS system) then how can CLAMS use it? Just make up a covnective rate?

PLEASE use continuous number, discerning page number as well as line numbers is not nice. I cannot see page number when reading your paper on a screen. Thus I may not get the page numbers correct in my comments.

P2L5 "reduced numerical diffusion compared 5 to the Eulerian-based transport models" I am tired of Lagrangrian models mantra that Eulerian are more diffusive. Some tracer transport Eulerian schemes have negligible diffusion – please see the Lauritzen papers (Geosci. Model Dev., 7, 105–145, doi:10.5194/gmd-7-105-2014 and Geosci. Model Dev., 5, 887–901, doi:10.5194/gmd-5-887-2012. Both methods have their advantages, but you need to retract the old arguments.

L13: " perpetuum runs are performed (14-times 2017) as" This is a very bad approach if you are serious looking at the strat-trop region, because at Jan 1 there is a huge discontinuity in the tropopause and jet stream every annual cycle. This creates havoc with lots of instant strat-2-trop placement of ozone in the troposphere (and vice versa).

L18: " three parameterized components of transport: isentropic mixing (I), unresolved convection (C) and tropospheric mixing (T) schematically" If these are all parameterized and not based on atmospheric physics then I do nto see how you are running ERA fields. There is only a certain amount of such mixing that is consistent with the wind fields. I fear that your model is inconsistent with the ERA model result.

P4. The table shows a worrying feature. You have only the instant winds every 6 hours. You really need 3-hour fields if you are going to resolve any diurnal cycles, such as BL mixing. With 6h, you alias all these cycles at different points as you cross longitudes. It seems like your mixing parameters are arbitrarily selected and not related to the local meteorology. I cannot understand it.

P5: The tracer results are interesting, nice job on the mix of real and synthetic tracers. For example, based on the e90 work, the new mixing in CLAMS-2 is essential in maintaining a clean tropopause. Figure 3, is a very nice representation of the consequences of the mixing.

P18 Conclusions. These are reasonable and rational and accurately describe the model results shown here. The AoA spectrum at 350K is interesting and shows a burst of young air with the new parameterizations. What is unclear is whether this carries on to 380-400K. The Hoffman 2022 results sound very interesting, is it limited to the lower stratosphere with very stable layering? Unfortunately, the paper is still being written (and should probably not be used as a reference here).

Overall, what I am worried about is that the mixing is set by the modelers based on a type of mixing, but it does not respond to atmospheric physics (did I miss something here). Can the authors get the statistics (e.g., like Tiedtke convective fluxes or BL heights and mixing) from the ERA fields?