Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC2
https://doi.org/10.5194/gmd-2022-97-RC2, 2022

Comment on gmd-2022-97

Anonymous Referee #2

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-RC2, 2022

This manuscript is an interesting contribution to the technology of atmospheric transport model, extending the series of works made with CLaMS. The main conclusion is that parametrized convective fluxes are necessary to perform realistic transport from the boundary layer to the UTLS, especially in the tropical region where no conveyor belt is operating, and I ready to accept that. The paper is on the overall well written and I have only a few minor comments to forward to the authors.

1 - My only significant reservation is about the usage of the e90 tracer. Prather et al. (2011) say that the 90 ppbv value fits the tropopause for their given CTM and meteorology but there is no reason to use it as a general reference since there is by definition no observational constrain for such an artificial tracer. It is OK to use e90 in order to characterize the overall structure and time-scale of the tropospheric overturning circulation but I do not think that the comparison of the 90 ppbv surface with the WMO tropopause in Fig.3 can be used to score the versions of the model. In all panels of Fig.2, there are other surfaces with large gradients that fit as well the tropopause than the I1C1 surfaces in Fig.3.

2 - The paper is written in an incremental mode and could be somewhat difficult to follow for someone who is not already familiar with CLaMS and the terminology that is proper to this model. Perhaps this is the intended audience but I would find useful to have a few general definition of notions like tropospheric mixing, pure advective mode and so on. The new epsilon parameter is defined is the appendix but this is again very cryptic if you do not know anything about CLaMS.

3 - It takes some time to understand and follow the awkward naming convention of the runs. At first, the table 2 does not make sense. Perhaps a more detailed description would help. It is not necessary to name the experiments in a publication in the same way as the directories on the computer.

4 - The parametrized convection is based on a fairly crude scheme, of the type that has been rejected long ago in the numerical weather forecast to the benefit of more sophisticated schemes which are found necessary to better represent the convective timing and organisation, and the convective fluxes. I understand that the present scheme used in CLaMS is better than nothing but why not using the convective fluxes of the ERA5 which are archived and available along the meteorological data used in this study. This will not be perfect as there are still many biases in such scheme but is would be more in pace with the state of the art in convective parametrization with the sole cost of additional storage. Off course, this suggestion is aimed only at future work.

5 – I would have liked to see a more detailed discussion of the effect of convective parametrization on p. 7, for instance about the meridional gradients associated with the Hadley circulation.

6 – I am puzzled by the discrepancies of CO_2 in the boundary layer where it is forced by Carbon Tracker. Is it that Carbon Tracker misses the biological carbon cycle and in which way is it local ?

7 – I do not understand why the black reference dots are different in the various panels of Fig. 6 and I do not understand either why the modelled cloud is closer to the reference in panel b that in panel a with apparently the same values of d. Perhaps this is an effect of the linear color scale that does not display the difference among small values.

8- There are two modes in Fig. A0 that should be distinguished, a UT tropical mode on the left which is strongly affected by the convection and a LS extra-tropical mode on the right which is only affected by mixing.

9- The authors should check the DOI numbers in the reference list. At least one (Konopka et al., 2019), the one I tried, points to another paper.