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Geoscientific Model Development Discussions

Interactive comment on "On the impact of recent developments of an atmospheric general circulation model on the simulation of CO_2 transport" *by* Marine Remaud et al.

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Review of "On the impact of recent developments of an atmospheric general circulation model on the simulation of CO2 transport", by M. Remaud et al.

As ESMs are more frequently used for climate projection and many of them include explicit simulation of CO2 transport and its 3D structure in the atmosphere, it is clearly becoming more important to adequately evaluate whether the dynamics and transport within these models is fit for purpose, and to understand the implications of any model errors or biases.

Over recent years, there has not been widespread interaction between the carbon cycle/GCM/ESM community (such as C4MIP) and the trace-gas transport community such as Transcom. This paper is therefore timely and attempts to address this lack with a detailed examination of the performance of tracer transport in the LMDz GCM. The model has been developed for use in CMIP6 and although not explicitly mentioned, presumably aims to perform the C4MIP CO2-emissions-driven simulations. Its behaviour is compared against an older version, and the impacts of several developments are assessed in some detail.

I am not an expert in the details of transport schemes or their evaluation, but review this in the context of its potential usefulness for CMIP6/C4MIP climate simulations. I found the manuscript well written and clearly structured. The experiments performed are logical, well described and analysed logically and clearly.

Overall I find the manuscript well suited to publication in GMD and recommend mainly minor amendments.

I have one specific query though. I am concerned by the apparent circularity of deriving surface CO2 fluxes using LMDx in an inversion system and then using these fluxes as input in a forward transport experiment to test against observations. Surely this is more than the passing mention in section 2.3, "... the surface fluxes carry some imprint of a ... [LMDz]...". You mention that other inversion products exist and could have been used - so why did you choose one so closely related to the model you were evaluating? At the very least I would like to see some defence of this choice - can you show and quantify the different sources available? How different are they? Some of your intro text describes how surface CO2 fluxes are still very ill constrained in many places, so I imagine the different sources available to you may well be quite different. I could envisage some value in using multiple (or at least 2) sources and showing an additional panel to figure 3 - how does the choice of surface fluxes compare with changes in resolution and surface physics?

A few minor points are listed below, but if you can address this main concern then I would consider this a very sound paper.

Chris Jones

1. section 2.2. finishes by mentioning a mass correction - can you elaborate slightly? Is this simply a diagnoses of the loss on each tilmestep which is then added back evenly distributed through space?

2. in a couple of places you mention CMIP4. I am assuming this is meant to say CMIP5? (but could also mean CMIP3 which accompanied AR4, or it could mean C4MIP - the confusion of MIPs is endless!). Please check and clarify.

3. section 4.3 uses independent measurements of vertical profiles - this seems like a more reasonable (i.e. not circular on the choice of transport model used for the inversion that produced the surface fluxes) test. If this is really the case then I'd expect the results here to be (almost) independent of your choice of surface fluxes, so that would be a good check if you performed a run with different sources - do the results in this section still come out the same?

4. related to my main point, I'm quite happy with conclusions about the impact of processes such as "the new physics tends to weaken the vertical mixing" - this seems a reasonable conclusion from your analysis. Where I am less comfortable are the comments around quality such as "the higher variance does not lead to an improved correlation". Unless you can show otherwise I don't quite believe that you can judge absolute quality with your experimental design.

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