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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Off-line algorithm for calculation of vertical tracer transport in the troposphere due to deep convection" *by* D. A. Belikov et al.

Anonymous Referee #1

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General Comments

The manuscript "Off-line algorithm for calculation of vertical tracer transport in the troposphere due to deep convection" written by D. A. Belikov et al. presents a unique approach for transport by deep cumulus convection in offline chemistry transport models. The scheme uses conservation of moisture, estimates of convective precipitation from reanalysis products, and a description of mixing between in-cloud and environmental air in an attempt to achieve more realistic simulations of vertical mixing and tracer transport. The scheme is implemented in the NIES transport model and evaluated against measurements of 222Rn and output from TransCom models.

The authors make a valiant attempt to evaluate performance of the proposed convection scheme, but it remains to be seen whether it represents an improvement over conventional approaches. This doesn't concern me though. The fact is this study represents a rare and commendable attempt to address a key issue faced by the tracer transport community. It is well documented that sub-grid vertical mixing remains a major source of uncertainty in offline simulations of tracer transport, but there have been relatively few attempts to improve the representation of offline vertical transport. As the authors mention, it is one thing to improve the calculation of cumulus convection inside GCMs, but quite another to propagate those improvements to offline models. The proposed scheme is an interesting and innovative attempt to bridge this gap. There are a few scientific questions that need to be addressed (see Specific Comments) and if possible I would like to see a comparison of the old and new approaches in NIES, but overall the paper is mostly well written and after a few revisions should be suitable for publication in ACP.

Specific Comments

It is not clear why the approach represents an improvement over the original Kuo-type scheme used in NIES (see Page 20247, Line 14-18 and Page 20263 Line 19-23). The authors argue that interpolation of moisture terms introduces errors into estimates of cloud transport, but doesn't the use of convective precipitation rates from reanalysis datasets also introduce significant errors? I suspect that the moisture terms needed for (9) are based on assimilated met fields; in this case, I would tend to trust these terms more than convective precipitation, which is based on cumulus parameterization. I think this argument needs explaining. If possible a test of the new approach (7) against the old approach (9), like demonstrated by Bian et al. [2006] in comparisons of CONV1 and CONV2 in PCTM, would help.

Some terms in (7) should be discussed in more detail, as they affect interpretation of the results. For example, how are qE and qU derived? Are they parameterized somehow, or prescribed from a reanalysis product? I expect they should be prescribed

from JRA-25/JCDAS to be consistent with convective precipitation. If not, there may be a violation of conservation of mass. In either case, this needs to be discussed. Also, what is x1 set to, and do you have an idea of sensitivity of updraft mass flux and tracer transport to x1?

I would hesitate to conclude that the simulations perform well (second to last paragraph in Conclusions). You should certainly highlight that the convection scheme is consistent with models and measurements at a variety of locations, which is a major challenge overcome! Instead of concluding that the simulations perform well, try to be honest and discuss some of the limitations of the approach, and how these might be overcome in future work.

Technical Corrections

Page 20252 Line 21-23: You mention that convective ppt does not always accompany upward convective mass flux, but according to (7), these should be directly proportional. Can you speculate on the cause of the mismatch?

Page 20253, Line 2: It is not possible to "consider the full spectrum of processes that influence vertical transport" in an offline tracer model. Please revise statement.

Page 20256, Line 4-7: Higher resolution in this sense (4x6 compared to 1x1) does not equate to more detailed description of convective processes, which must be parameterized down to about 2 km. At these scales, it may be said that higher resolution improves the description of resolvable winds, but even this is debatable.

Page 20257, Line 1-5: Be careful with wording in evaluations against other models. The statement "insufficient reproduction of small-scale convective fluxes" is misleading because you are comparing models to models.

Page 20260, Line 4-5: What is the difference in simulations prior to 1994? If they are systematically different you need to discuss or remove them from the analysis.

Page 20263, Line 8: "cycle By" should be "cycle. By"

Page 20263, Line 26: "successfully reproduce deep cloud convection" should be followed by "from MERRA". There is another instance of this at the end of the Abstract. The statement is ok, but I would hesitate to describe this as a success, since ultimately the goal should be to reproduce and explain observations, not another model.

Page 20265, Line 27-28: Consider removing the first line of this paragraph. Simulations do not generally match observations. As you describe in the paragraph, they are consistent with measurements at oceanic and coastal sites, but struggle at continental sites.

Page 20266, Line 5-7: Statement is confusing.

Page 20267, Last Paragraph: Very confusing. Please reword and elaborate.

Figure 2: Please change longitude axis from 180W-180E to 0E-0W to conform with Figures 3, 4, & 5.

Section 4.5.2: Figure 1 → Figure 10

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