Anonymous Referee #1

We thank the reviewer for constructive and helpful suggestions. We have provided our responses to the reviewer comments and believe our manuscript is much improved as a result. The reviewer specific comments (shown in **bold**) are addressed below.

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Received and published: 27 September 2012

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 environ-mental 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 pro-posed 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).

Description of the original Kuo-type scheme is elaborated. Limitations are highlighted.

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.

Section 2.1, 3.2, discussion and conclusion were revised and elaborated. The limitations of the approach are presented in discussion.

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?

Equation (7) represent only deep convection accompanied by convective precipitations, however precipitation occurs, only when a cloud water threshold is exceeded and large raindroplets are created, so condensation-driven convection may exist without precipitation like in shallow cumuli. Moreover, convective updraft and fall of convective precipitations may occur in different moment of time or in different grid cell if convective column is moved. Thus, the proposed convection scheme successfully captures most of the large-scale upward convective mass flux that is accompanied by convective precipitation, but does not appear to reproduce upward fluxes at smaller scales.

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.

We agree. Statement revised as follows:

"… consider the wider spectrum of processes that influence vertical transport."

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.

Usually higher resolution improves the description of resolvable winds, so it can be a reason of found strongest penetrative mass flux. Thus, the statement extended with "or higher resolution improves the representation of resolvable winds, especially in the vicinity of the frontal lines"

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.

Statement reworded.

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.

Statement P 20260, L 4-5 "We should note, the apparent change in behavior from poor simulations from 1994–1998 to better ones later." was removed as we found an error in the plot program. Figure is updated.

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

It should be a text processing error, as in the original manuscript file there is no such error. We will correct it during final proof preparation for ACP.

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.

The phrase was revised as follows:

"… successfully reproduce deep cloud convection as shown from comparison with MERRA reanalysis, radon vertical and near surface profiles."

Last sentence in abstract is revised as: "From this comparison, we demonstrate that the proposed convective scheme in general is consistent with observed and modeled results."

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.

The phrase "Simulated seasonal cycles in ^{222}Rn concentrations generally match observed seasonal cycles at continental, oceanic and coastal sites." was revised as "Simulated seasonal cycles in ^{222}Rn concentrations are generally consistent with observed seasonal cycles at oceanic and coastal sites."

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

This statement was reworded.

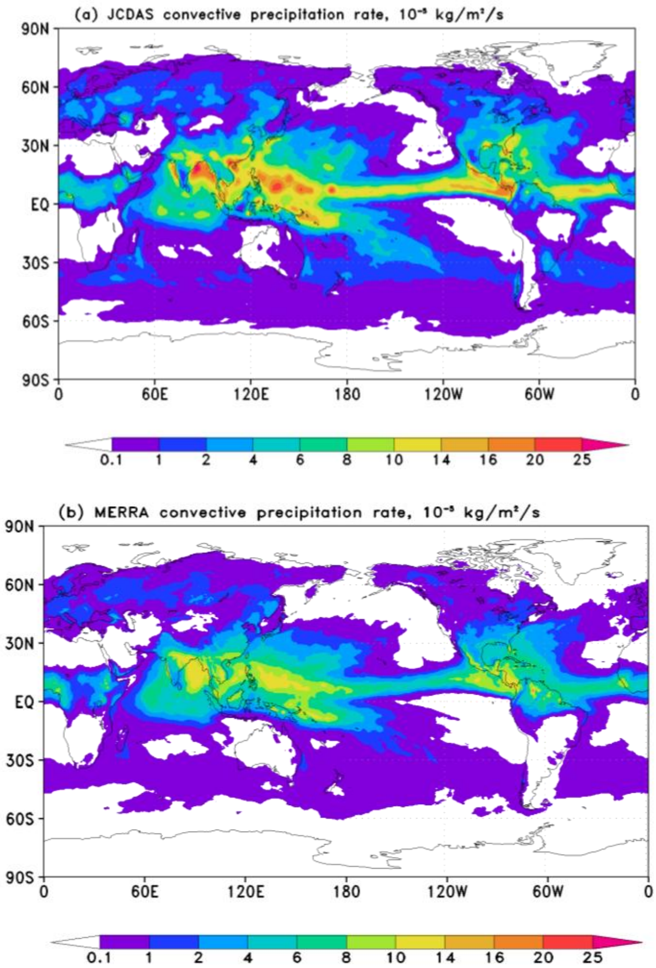
"Results of NIES TM with proposed parameterisation are consistent with the results of considered TransCom-CH_4 on-line and off-line models."

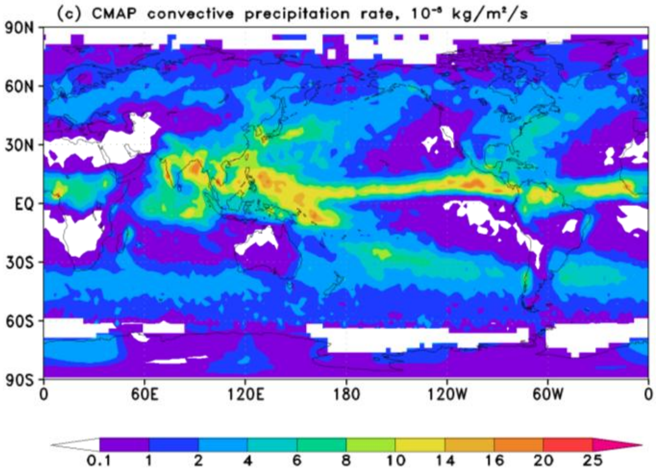
Page 20267, Last Paragraph: Very confusing. Please reword and elaborate.

We agree. This statement was removed.

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

Longitude axis in figure 2 was changed from 180W-180E to 0E-0W.





Section 4.5.2: Figure 1 ! Figure 10

It should be a text processing error, as in the original manuscript file there is no such error. We will correct it during final proof preparation for ACP.