Atmos. Chem. Phys. Discuss., 12, C9523–C9530, 2012
www.atmos-chem-phys-discuss.net/12/C9523/2012/

Atmospheric Chemistry and Physics Discussions

ACPD

12, C9523–C9530, 2012

Interactive Comment

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 #2

Received and published: 19 November 2012

General comments:

The manuscript presents an approach to calculate vertical tracer transport by deep convection in off-line atmospheric chemistry transport models. The scheme computes convective vertical mass transport from information on convective precipitation assuming moisture conservation. In the presented implementation of the scheme a detailed distribution of convective precipitation is provided by reanalysis data and resulting vertical mass fluxes are evaluated against other reanalysis datasets. The scheme was implemented in the NIES off-line transport model and evaluated by comparison of simulated 222Radon distributions with observations and model results from Transcom

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

models.

Convection is still one of the major uncertainties in off-line tracer transport modeling and any attempts to reduce and/or quantify this uncertainty are most welcome. The authors present a scheme, which essentially tries to reconstruct in the off-line transport model the vertical transport in convective clouds from a given amount of convective precipitation and cloud extend available from reanalysis data. This is an interesting approach, which had been implemented in one of the first off-line transport models (Feichter and Crutzen, 1990) based on climatological transport fields, and is modified here to include assimilated precipitation and moisture fields. The authors state that a main advantage of this scheme, in comparison to conventional convection schemes, is that fewer errors are introduced because less interpolation between model and reanalysis grids is needed. This statement needs clarification. Furthermore, the uncertainties introduced by the use of convective precipitation, which is an entirely simulated quantity, should be discussed in more detail. While total precipitation can be measured or derived from satellite data, the distinction between convective and large scale precipitation is largely artificial and depends on the type of parameterization and resolution, i.e. ability to resolve processes leading to precipitation formation, of the model used in the reanalysis.

In order to show any improvement by the introduction of the new scheme for calculating convective mass fluxes, the comparison with results using the old version is essential and should be added to the paper.

Although the authors evaluate the performance of the new scheme in several ways their conclusions remain vague. They present a detailed comparison of Radon simulation results with observations, which is interesting in itself, but they do not sufficiently discuss conclusions regarding the contribution by deep convection and the improvements due to their newly implemented scheme. The author should be more moderate in describing model performance. The agreement with observations is not worse (but also not better) for the NIES transport model than for the other models. This is reassuring but still not a prove of quality because all model could be wrong.

In general, the paper is quite well written and well structured. However, the discussion and conclusion sections should be revised to present a systematic evaluation of the Radon comparison with respect to convective processes and a systematic discussion of advantages and limitations of the new scheme. The paper would be suitable for publication in ACP after careful revision.

Specific comments:

P 20242, L 21-24: Another advantage is that this is done at every time step.

P 20243, L 29ff: This is not a 'problem' but something that needs to be done and was done by Feng et al. (2011). Please revise this sentence.

P 20246, Eq. 4: This is not exactly the equation from Feichter and Crutzen – they might have had a typo in it – but an adaption. The first line is the original equation from Austin and Houze. Please adjust citation.

P 20247, Eq. 7: As this is the central part of the parameterization more information on the individual variables in this equation is required. It should be described how they are derived, e.g. from which dataset, and how they influence the results. Presumably qu, qe, ztop, zbase are from the same dataset as the precipitation but this needs to be stated clearly or otherwise the implications of possible inconsistencies have to be discussed. Which particular value was chosen for x1?

P 20247, L 4: Was this scheme used in the previous version of the NIES transport model? Please state more clearly.

P 20247, L 17: What is the difference between reanalysis model grid and reanalysis data grid in this case?

P 20247, L 14-18: Whether the Kuo-type scheme introduces more errors in the estimates of convective mass flux remains to be seen in a direct comparison between the two parameterizations. This comparison should be shown and discussed in this paper. Is there any reason why an interpolation of the variables precipitation, water vapor mixing ration in the updraft and in the environment should be more precise than the interpolation of pressure, wind, temperature and moisture needed for the Tiedtke scheme? Please elaborate on this difference.

P 20248, L 21-23: From eq. 7 it seems that also water mixing ratio in the updraft and in the environment is needed. Where is this information coming from? Please explain in more detail.

P 20250, L 8-10: Is this really all you need? See comment above.

P 20250-20251: Does CMAP really provide convective precipitation? The original dataset has only total precipitation. Figures 1 and 2 show CMAP total precipitation. This is obvious from the high precipitation in the mid- and high-latitudes. Please take this into account in your interpretation of the comparison and revise text, table and figures.

P 20250: To what extend does the separation of large scale and convective precipitation in MERRA rely on the GCM used in the reanalysis? Please comment.

P 20252, L 6-7: The MERRA reanalysis are also depending on a model that includes a parameterization of convective processes. Although meteorological fields in the reanalysis are optimized against observations they are still not independent of the specific model (parameterization). This is therefore more a comparison between the results of different parameterization schemes. Please state this limitation more clearly.

P 20252, L 21-25: This hints to a systematic difference between the convective mass flux according to eq. 7 and the parameterization used in the reanalysis underlying MERRA. Please state more clearly that the new parameterization limits the occurrence of convection to cases when precipitation is produced and hence will systematically underestimate convective mass flux. Please explain why the new parameterization

ACPD
12, C9523–C9530, 2012

captures only most of the upward convective mass flux that is accompanied by convective parameterization and not all as would be expected from eq. 7.

P 20252, L 25-26: Please specify how you come to the conclusion that the new scheme does not work on small scales.

P 20253, L 1-3: To ‘consider the full spectrum’ will hardly be achievable. Please modify this statement.

P 20253, L 15-18: Please specify here whether the deep cloud parameterization is the only difference or what else is different. This would allow a comparison with the results in Patra et al. (2011).

P20254, L 27ff.: Are these filaments visible in Fig.4? Please indicate where.

P20255, L 22-23: Please try to explain the difference to Tost et al., which is most probably due to differences in the prescribed Radon source.

P 20256, L 16-18: Why is the strong vertical transport ‘throughout the year’ not symmetric around the equator? Why should it extend more to the south? Fig. 6 shows NH winter and still high Rn concentrations in the upper troposphere extend further north than 4N in most models.

P 20257, L 1-5: Nice that you ‘found no problems’. Please rephrase. You should clarify here the limitation of a comparison to other models, e.g. all model could have systematic deficiencies in this region.

P 20259, L 12-13: This is not visible in Fig. 7 or 8. Do you mean ‘higher altitude’?

P 20259, L 17-20: Why are you adding references to Fig. 9 and 10? Are the same figures found in Feng et al. and Zhang et al., respectively? If you want to compare to the results of Feng et al or Zhang et al please do so explicitly and comment the comparison.

P 20259, L 20-23: To meet this statement please adjust Fig. 9, where Radon is given in Bq/m-3.

P 20259, L 25ff.: From Fig. 9 it is not at all obvious that radon emission are realistic. The systematic underestimation of Radon by most models at Hohenpeissenberg and the underestimation of the seasonal cycle amplitude at Amsterdam Island could well be caused by deficiencies in the Radon flux distribution. You even discuss possible deficiencies of the Radon flux distribution in 4.5.2. Please clarify your statement.

P 20260, L 1-2: From Fig. 9a it does not seem that the models show 'good results'. Not even the phase is correctly represented – this could however be simply an error in the plot program, as the seasonal cycle in the observations does not fit to the time-axis (cf. Fig. 12 in Feng et al.). In any case please moderate your statement.

P 20261, L 4-5: Why should the performance of all models change with time? This could well be a misinterpretation of Fig. 9a due to the plotting error (see previous comment). If this is not the case then this change needs to be discussed.

P 20260, L 13: Please use consistently either mBq or Bq.

P 20261, L 11-13: Why do you make use of observations made inside the forest canopy? It cannot be expected that (coarse grid) models can represent this kind of local circulation. Hence, a comparison is meaningless and a high correlation would just be by chance. Better use measurements above the canopy only.

P 20262, L 9-11: Only the amplitude of the seasonal cycle is similar, the phase is shifted by 6 month.

P 20263, L 23-24: But the use of convective precipitation data from a reanalysis that is different from the reanalysis used for advection etc. bears the risk of introducing inconsistencies in the transport. Please comment.

P 20263, L 25-26: Whether the scheme is successful or not can only be judged from a comparison with observations. The results were only compared to reanalysis data and this should be specified here.

ACPD

12, C9523–C9530, 2012

P 20264, L 13-15: In Fig. 6 it is hardly visible that the Radon concentration in the Northern Hemisphere above 200 hPa is higher in NIES-TM than in the other models. Whether there is an underestimation above 100hPa cannot be seen in Fig.7. Please revise statements.

P 20265, L 27-28: This statement is overly optimistic and not true as was shown in 4.5. Please revise.

P 20266, L 4-5: This would imply that parameterization of deep convection is the most important difference between the models. This is not shown in this paper. Please revise the statement.

P 20266, L 13: This statement is only comprehensible if restricted to the implementation of convective parameterization schemes in off-line transport models.

P 20267, L 3-11: The conclusions from the comparison to observations are vague. It is not specified (in the paper) which features in the comparison of simulated and measured Radon in particular indicate that the parameterization of convection is responsible for the agreement (or disagreement). This needs to be substantiated. In particular the last sentence is pure speculation. Furthermore, a comparison to other models is not a 'validation'.

Conclusions: The whole section needs a clearer structure. At the present state it presents rather a summary than conclusions. Statements should be fully supported by the comparisons presented in the paper. Limitation of the study should be addressed more critically.

Technical corrections:

Page 20254, L 8: . . . configurations. The . . .

Page 20254, L 11: . . . (CAM, MOZART. . .

Page 20256, L 12: . . . oceanic regions off the western coastline. . .

ACPD

12, C9523–C9530, 2012

Page 20256, L 15: ‘where land occurs’ might not be the right wording.

Page 20257, L 20: remove one ‘obtained’

Page 20258, L 9: . . . structure. Cold. . .

Section 4.5.2: Should be Figure 10 instead of Figure 1.

Page 20262, L 28: Gosan, Hong Kong, and Bombay. . .

Page 20262, L 28: . . . typical for. . .

Page 20263, L 8: . . .cycle. By. . .

Page 20264, L 13: Specify: Radon concentrations

Page 20266, L 1: Please rephrase ‘coarse models grids’

Page 20267, L 3: Specify: Radon concentrations

Table 3: Several decimal points are missing. Highlight NIES model.

Figures 2-5: Please adjust maps so that the longitude axis is the same.

Figures 9-10: Please use same units for Radon.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 20239, 2012.

ACPD

12, C9523–C9530, 2012

Interactive Comment