Anonymous Referee #2

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.

Anonymous Referee #2
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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 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.

We added "… to calculate cumulus convection parameters 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.

Revised.

"Feng et al., (2011) investigated the performance of cloud convection and tracer transport in a global off-line 3-D chemical transport model and diagnosed the updraft mass flux, convective precipitation and cloud top height performing various model simulations using different meteorological (re)analyses."

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.

Done

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?

Section 2.1 was revised.

To state more clearly the phrase "The NIES TM simulations discussed in this paper use data from a reanalysis dataset produced by the Japan Meteorological Agency (JMA) and the Central Research Institute of Electric Power Industry (CRIEPI)." in section 3.1 is revised as "All meteorological data used in NIES TM simulations discussed in this paper (except PBL height) are provided by reanalysis dataset produced by the Japan Meteorological Agency (JMA) and the Central Research Institute of Electric Power Industry (CRIEPI)."

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

Yes. It was used. The state was clarified.

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

Reanalysis model grid is the grid of the model used to produce reanalysis. Reanalysis data grid is the grid on which data are distributed, when they are available for users. For example, Global Point Value (GPV) a special product prepared by the Japan Meteorological Agency Global Circulation Model (JMA-GSM), which is a high-resolution global atmospheric circulation model developed by the Japan Meteorological Agency (JMA) and the Meteorological Research Institute (MRI) of Japan. The current version of the model uses a reduced Gaussian grid TL959L60 with a resolution of approximately 20 km in the horizontal and 60 layers up to 0.1 hPa in the vertical (Mizuta et al., 2006, Iwamura and Kitagawa, 2008). However, for users this meteorological dataset is available with a resolution of 0.5°×0.5° for 21 pressure levels.

Mizuta, R., Oouchi, K., Yoshimura, H., Noda, A., Katayama, K., Yukimoto, S., Hosaka, M., and Kusunoki, S.: 20-km-mesh global climate simulations using JMA-GSM model – Mean climate state, J. Meteor. Soc. Japan, 84(1), 165–185, 2006.

Iwamura, K. and Kitagawa, H.: An upgrade of the JMA Operational Global NWP Model, CAS/JSC WGNE Res. Act. in Atmos. and Ocea. Modelling, 38, 6.3–6.4, 2008.

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.

Section 2.1 and 3.2 were revised and elaborated. Please check the manuscript text.

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.

Statement "Mixing ratio of water in the environment and in the updraft q_E, q_U are calculated using water mixing ratio, the air temperature and pressure supplied by meteorological reanalysis." was added to section 2.1.

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

Reworded. These parameters we additionally need to standard set of meteorological fields available in reanalysis, i.e. water mixing ratio, the air temperature and pressure.

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.

CMAP provided total precipitation. Text, table 1, figures 1-2 and captions for them are revised.

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.

We have added some additional information to Section 3.1.

The MERRA dataset has been produced using the National Aeronautics and Space Administration (NASA) global data assimilation system, which employs NASA Goddard Earth Observing System global atmospheric model version 5 (GEOS-5). Using a non-hydrostatic finite-volume dynamical core coupled with advances in the moist physics and convective parameterization the model has been used to perform cloud-system resolving experiments at resolutions as fine as 3.5- to 14-km globally (Putman and Suarez, 2011).

MERRA reanalysis data was analyzed in several works and shows good results especially in tropical region. For more details please check:

1. Kennedy, Aaron D., Xiquan Dong, Baike Xi, Shaocheng Xie, Yunyan Zhang, Junye Chen, 2011: A Comparison of MERRA and NARR Reanalyses with the DOE ARM SGP Data. J. Climate, 24, 4541–4557.
2. Putman, W. M., and Suarez , M.: Cloud-system resolving simulations with the NASA Goddard Earth Observing System global atmospheric model (GEOS-5), Geophys. Res. Lett., 38, L16809, doi:10.1029/2011GL048438, 2011.
3. Robertson, F.R., and Roberts, J.B., 2012: Intraseasonal Variability in MERRA Energy Fluxes over the Tropical Oceans. JOURNAL OF CLIMATE, 25(17), 5629-5647.

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.

Statement about limitation was added.

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 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.

Equation (7) represent only convection accompanied by convective precipitations, however precipitation occurs, only when a cloud water threshold is exceeded, so convection may exist without precipitation.

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

Here we mean "small intensity" rather than "small scales".

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

Modified.

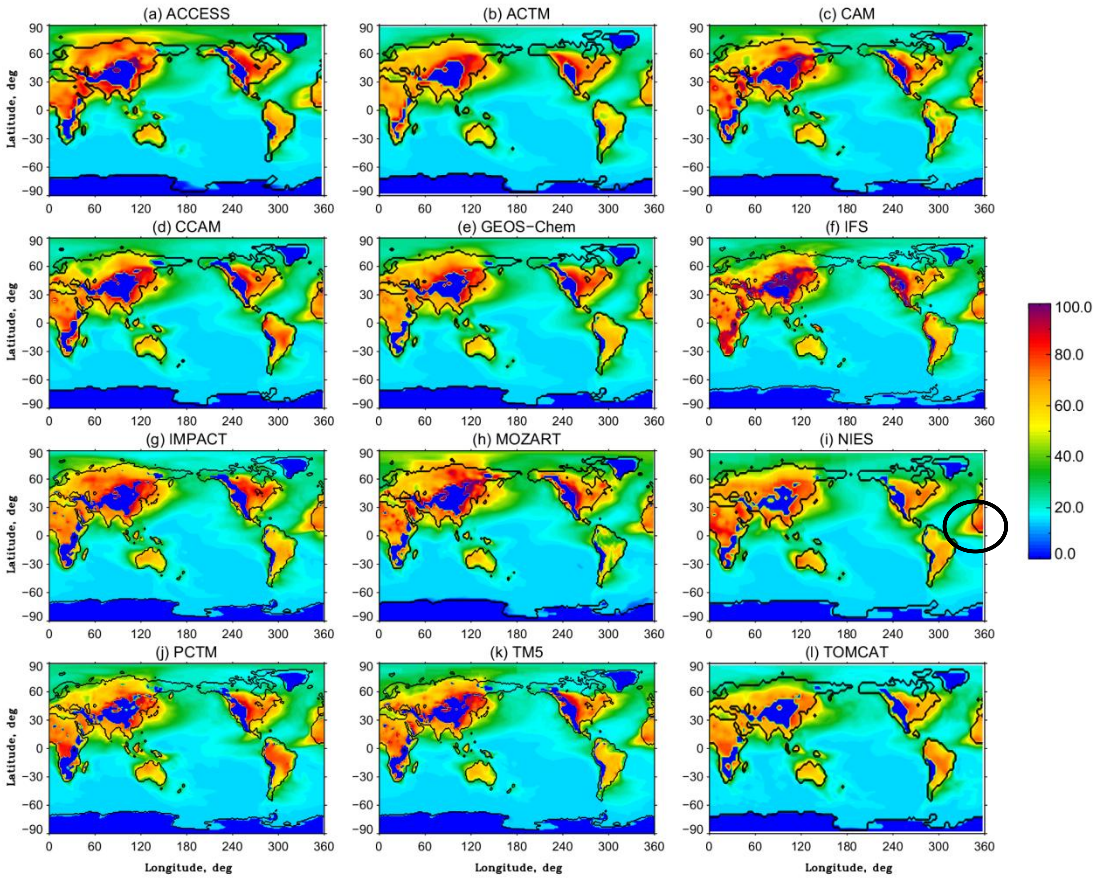
"… consider the wider spectrum of processes that influence vertical transport."

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).

Here we stated, that there is no difference between the described model version and model participated in TransCom-CH_4 TMI by Patra et al. (2011).

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

These filaments are visible between Africa and South America (0°-30°N, 330°E-360°E) in all panels of Fig.4 (see below).



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.

Yes. Main reason is 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.

It should be a text processing error, as in the original manuscript file we state "transport of ^{222}Rn throughout the year over 15°S-40°N". We will correct it during final proof preparation for ACP.

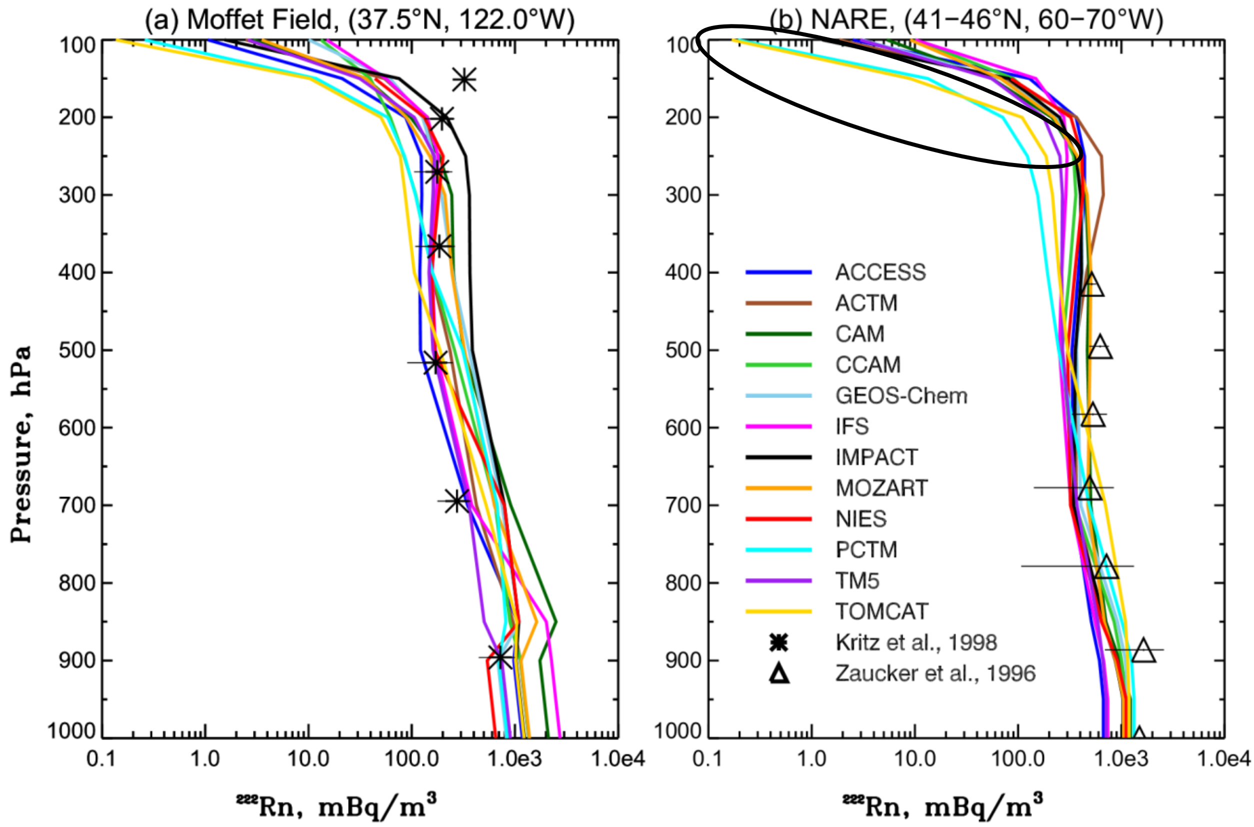
The strong vertical transport is not symmetric around the equator (extended more to the north), because emissions are much smaller over the ocean to the south of the equator and more powerful to north of the equator

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.

Revised. Limitations are described in Discussion.

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

This is visible in Fig. 7d, 8a,b. PCTM and TOMCAT simulated radon mixing ratio start to decrease at level of 200hPa. Profiles of other models are above. (see below)



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.

We added references to Fig. 9-10 to show suitability of used data and limited number of in situ measurements (compiled for several decades) as well. Direct comparison to results of Feng et al. or Zhang et al. is not reasonable here, as we already have set of models.

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

Done (see below).

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.

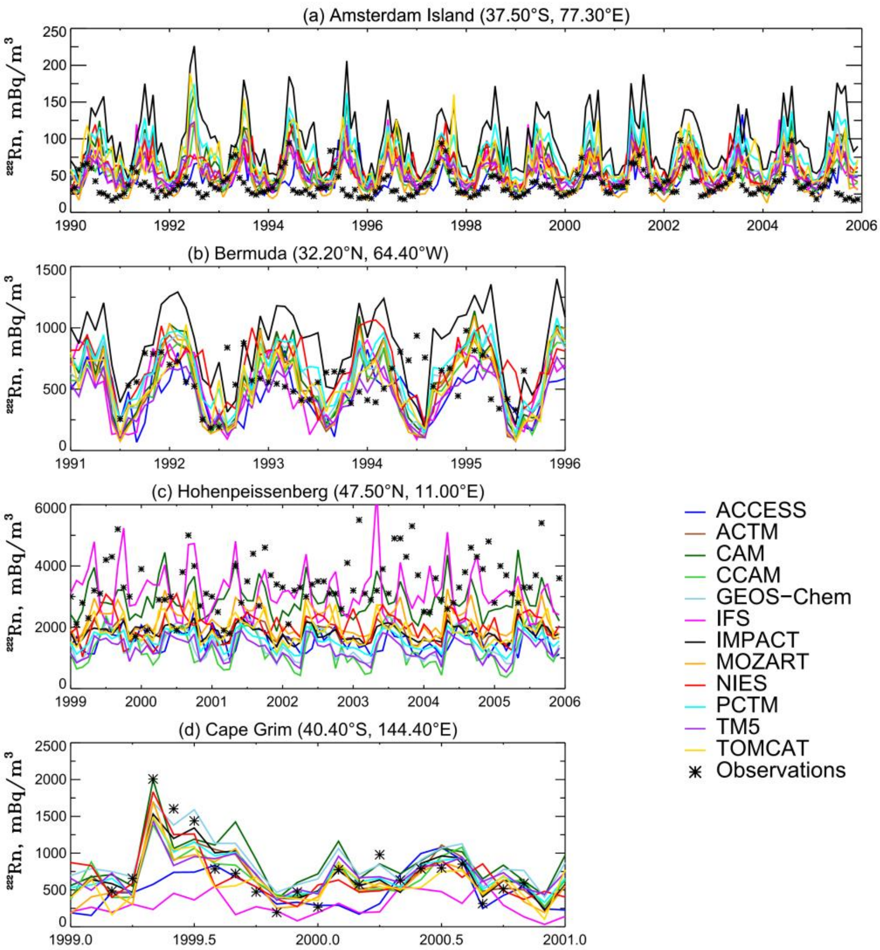
The phrase "The models are able to reproduce monthly mean variations in radon concentrations at a variety of surface sites (Fig. 9). This agreement between simulations and observations indicates that the assumed radon emissions produce realistic distributions of radon at the surface." was reworked as "The models are able to reproduce the phase of monthly mean variations in radon concentrations at a variety of surface sites (Fig. 9). This comparison between simulations and observations indicates that the assumed radon emissions produce realistic seasonal variation of radon at the surface. However reproducing of the variation amplitude is complicated."

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.

It was simply an error in the plotting program.

Figure is updated.

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.



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.

It was simply an error in the plotting program.

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

Done

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.

Unfortunately, from available sources we could find 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.

Reworded as follows

"The observed seasonal cycle amplitude of radon concentrations at Dumont d'Urville (Fig. 10g) is similar to the seasonal cycle's amplitude at Crozet and Kerguelen, but the annual maximum occurs during austral summer rather than during austral winter, so 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.

Phrase "Moreover, the modified scheme is more flexible, as it is able to use convective precipitation data from different reanalysis." was removed.

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.

Agree. The statement was extended as follows: "The proposed convective scheme can successfully reproduce deep cloud convection, as shown from comparison with MERRA reanalysis, radon vertical and near surface profiles."

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.

The statement was revised as follows: "As a result, the model with the new scheme has larger ^{222}Rn concentrations in the northern hemisphere above 200 hPa (Fig.6) and overestimates the concentrations in the levels around 200 – 150 hPa (Fig.7a,e)."

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

The statement was revised: "Simulated seasonal cycles in ^{222}Rn concentrations are generally consistent with observed seasonal cycles at oceanic and coastal sites."

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.

This statements is made for short-lived radon tracer, which is very sensitive to parameterization of deep convection. To avoid confusing we revised the statement as:

"On the other hand, the model-to-model difference in ^{222}Rn concentration is still large indicating very different performance of DCC parameterizations."

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

We proposed off-line algorithm for calculation of vertical tracer transport due to deep convection suitable for off-line transport models. In online transport models all necessary parameters can be calculated in coupled GCM (see 20242, L20-24).

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.

Conclusions were reworded and elaborated. Limitation of the study addressed in Discussion.

Technical corrections

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

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.

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

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.

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

Should be revised "… oceanic regions of the western coastline …"

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

Revised with "… where land exists …"

Page 20257, L 20: remove one 'obtained'

Done

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

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.

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

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.

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

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.

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

Done

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

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.

Page 20264, L 13: Specify: Radon concentrations

"^{222}Rn" added

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

“coarse models grids” rephrased with “coarse grids of considered models”

Page 20267, L 3: Specify: Radon concentrations

“radon” added

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

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.

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

Longitude axis in figure 2 was changed from 180W-180E to 0E-0W. (see in response to the referee #1)

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

Done