We thank the referee for taking the time to read our manuscript and for their helpful comments!

General changes

- We have considerably changed the text throughout the manuscript to improve the logical order of the text and to improve the explanations and comprehensibility. We added new subsections and improved the use of the English language.

Major comments

- A) **The novelty of this study is not apparent to me.**

  The novelty is the explicit simulation of the upward transport of air parcels inside convective updrafts and of the variable residence time of air parcels in convection, in contrast to schemes which only redistribute air parcels from the entrainment to the detrainment locations in a fixed time step.

  Which elements of this convective transport scheme are standard, and which elements are new?

  The explicit simulation of the upward transport of air parcels inside convective updrafts is new, and the algorithm for detrainment has to be changed accordingly. The redistribution according to entrainment and detrainment probabilities, respectively, is standard. We have more clearly stated this in the description of the algorithm. See also the reply to two comments in major comment C below: comment to page 4, section 2.2 (entrainment) and comment to page 6, section 2.4 (detrainment).

  First, my impression was that the use of so-called random convective area fraction profiles is novel, but then this goes back to Gottwald et al. (2016)

  The stochastic parameterisation described in Gottwald et al. had so far not been implemented to estimate convective mass fluxes in convective transport models. The implementation of their method in a transport model is novel.

  The authors should discuss in greater detail how their scheme differs from existing schemes, e.g., the ones mentioned on p. 2 line 5.

  The scheme extends the approach in existing schemes by modelling vertical updraft velocities and the time that an air parcel spends inside the convective event. Apart from that, the convective transport part of all schemes (including our scheme), is similar (that is the redistribution of the air parcels given the entrainment rates, detrainment rates and mass fluxes). We hope that we have now more clearly stated the novelty and differences in the introduction and the description of the method.

- B) **There must be many studies about convective tracer transport, but very few of them are referenced and discussed.**

  The simulations of Radon-222 and SO2 are not discussed in the framework of the existing literature.

  We have added discussion to section 4.4 (section 4.2 in the original manuscript) on how well the results of other studies compare to radon measurements to put the comparison of our model to radon measurements into perspective. Other studies show differences between their models and the radon measurements of a similar order of magnitude (Jacob et al., 1997, Collins et al., 2002, Forster et al., 2007, Feng et al., 2011). More discussion of the validation of convective transport models was added in the introduction and in the conclusions. The large uncertainties in emissions, measurements, chemistry and microphysics of short-lived species generally pose a challenge for the validation of the simulation of these species, which we think is an important issue. We have added Feichter and Crutzen (1990) as an additional study to the references.

  We added a discussion of the implications of the differences in the simulation of SO_2 in the different sensitivity runs to section 5. In addition, we added a paragraph discussing very-short lived bromine species to show that the algorithm is also relevant for species other than SO_2.

  This is a technical paper presenting a new algorithm, which is intended as a technical reference to cite when this algorithm is used in an application. It is outside the scope of this paper to give a more detailed discussion of studies of convective tracer transport. Several review papers are cited in the manuscript for reference (e.g. Mahowald et al., 1995, Jacob et al., 1997, Hoyle et al., 2011).

  Changes to the manuscript: Added discussion in the introduction and conclusions of the validation by radon and the issue that the uncertainties in measurements, chemistry, microphysics and emissions pose a challenge for the validation of the simulation of short-lived species. Added discussion to section 4.4 (section 4.2 in the original manuscript) on how well other models compare to the radon measurements. Added an additional reference (Feichter and Crutzen, 1990). Added discussion of the implications of the differences in the simulation of SO_2 to section 5 and added three new references (Feichter et al., 1996, Kremser et al., 2016, Rollins et al., 2017). Added discussion of very short-lived bromine species to section 5 and added three references (Hossaini etal., 2012, Schofield et al., 2011, Wales et al., 2018).

- C) **Page 1 line 1 and page 2, line 3: What is meant by ensemble trajectory simulations?**

  We agree that it was not obvious what was meant.

  Changes to the manuscript: We added the following explanation to the introduction: "In addition, the scheme can be used for applications such as backward trajectories starting along flight paths or sonde ascents, where it allows for simulating the effect of convection when using a statistical ensemble of trajectories starting at every measurement location."

Page 2, line 16: Explain better what is meant by instantaneous redistribution

The Lagrangian convective transport schemes cited here use a short fixed time step to redistribute air parcels, which is not necessarily the same as the advection time step. Collins et al. use a fixed time step of 15 minutes for convection and of 3 hours for large-scale advection. That is, the time period between entrainment and detrainment is fixed to 15 minutes. Forster et al. also use a 15 minute time step. Rossi et al. use a time step of 30 minutes.

Changes to the manuscript: We rephrased several parts of the abstract and the introduction to make that more clear. We replaced all occurences of "instantaneous redistribution" by "redistribution in a fixed time step" to avoid misunderstandings.

Page 3, lines 23 and 27: Unclear to me what exactly is meant by "meteorological data"

Changes to the manuscript: We changed "meteorological data" or "meteorological analysis" to "meteorological analysis data" to make that more consistent throughout the paper and to make clear that we are referring to the same data.

Page 4, section 2.2: Is this treatment of entrainment [...] standard, i.e., as in other schemes, or is there some novelty here?

Yes, this part of the algorithm is standard, see e.g. Collins et al., 2002 and Forster et al., 2007.

Changes to the manuscript: We have added these references to the text.

Page 6, section 2.4: Is this treatment of [...] detrainment standard, i.e., as in other schemes, or is there some novelty here?

This part of the algorithm is not standard, since it explicitely simulates the upward transport of the air parcel inside the cloud. In other models, only the probability that an entrained air parcel detrains at a given altitude is calculated. The final probability that an air parcel detrains at a certain altitude is the same in our approach and the approach of Collins, Forster and Rossi.

Changes to the manuscript: Added to section 2.5 (section 2.4 in the original manuscript): "The approach for detrainment described above differs from the approach employed in previous Lagrangian convective transport schemes, since it takes into account the explicit simulation of the time that air parcels spend in convective updrafts, whereas schemes such as those employed in Collins et al. or Forster et al. assume a constant time that parcels spend in convection. The probability that an entrained air parcel detrains at a given altitude, however, is the same in both approaches."

Page 7, line 26: This important statement (?) requires much better explanation; it appears rather problematic that fu is not in agreement with the actual number of trajectories in updrafts.

This was not discussed properly and would leave the reader with the impression that there is a significant problem, which actually is not the case. f_{up} is very small, and the results of the validation runs show that the mass conservation is not noticeably affected by the uncertainty in the number of trajectories in convection.

As an alternative to f_{up}, the fraction of trajectory air parcels that are currently in convection in the model run could be used. This is however only possible for global runs. The mass flux of trajectories through a given surface is not necessarily balanced for non-global ensembles of trajectories. The approach would require to average the results over a volume that is small enough to allow for variations in the fraction, but large enough to contain a sufficient number of air parcels.

Another alternative would be to subside all air parcels and not only the air parcels, which are currently not in convection (see Collins et al., 2002). Subsiding air parcels which are currently in convection is however not only unphysical, but also can result in air parcels that descend while they are in convection and that possibly detrain at a lower altitude than they were entrained.

Changes to the manuscript: Extended discussion in section 2.6 (section 2.5 in the original manuscript) along the lines outlined above.

Page 8, line 18: What type of radar measurements? Since this profile (Fig. 3) is important for this study, it would be important to understand better what it is based on.

Changes to the manuscript: We added that the radar is a "precipitation radar" and that the profile is based on the data of two wet seasons (2005/2006 and 2006/2007). We added that the method to obtain the area fractions is "estimating the fraction of convection by comparing the area of convective precipitation to the total measured area".

Page 10, line 3: I don't think that the character of the method is "random", most likely you mean "probabilistic" or "stochastic"

Changes to the manuscript: Changed.

- D) **My most important concern [...] How many air parcels / trajectories are required per reanalysis or GCM grid box in order to care about updrafts? [...]**

  There is a misunderstanding here, namely that the convective updraft area is needed to calculate the number of trajectories affected by convection in a given time period or the probability for a trajectory going into convection, which is not the case! Possibly, this misunderstanding was caused by the formulation "since a grid box contains several convective systems that only cover a small fraction of the grid box, a statistical approach is necessary" (page 3, lines 1–2). This was misleading and has been rephrased.

The convective area fraction is not needed for calculating entrainment and detrainment probabilities and the probability is independent of the area covered by convection. It is *only* needed for the calculation of the vertical updraft velocities. Hence, it is not used in the descriptions of the other Lagrangian convective transport schemes (Collins, Forster, Rossi).

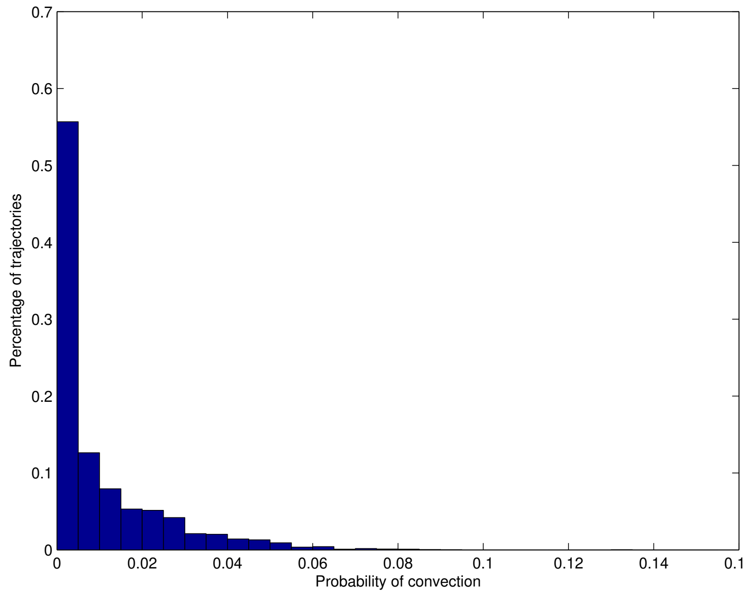
The quantity which is relevant for the entrainment probability is the entrainment rate integrated over altitude (with most entrainment typically at cloud base) and not the convective area fraction (see also discussion in section 2.2 of the original manuscript and Equation 3). It is only relevant how much air can be processed by entrainment in a given time period compared to the mass of the grid box. The probability of convection is therefore also dependent on the considered time period.

While the mass flux of the entrained air is proportional to the product of convective area fraction and vertical updraft velocity (see Equation 4 and discussion), these quantities are not needed for the calculation of the probablities, which only depend explicitly on the entrainment rate. A small updraft velocity and a large convective area and a large updraft velocity and a small convective area lead to the same result for the entrainment rate.

The only place where convective area fractions are needed in the model are the vertical updraft velocities, which cannot be deduced from the mass fluxes alone. The only reason for this is that the mass fluxes in ERA Interim are given as grid-box means, while the mass flux inside the cloud is needed.

To show that the number of trajectories is sufficient to capture the updrafts, we calculated a frequency distribution of the probability that a trajectory is entrained into a convective cloud for all trajectories below 2 km from the first time step of the run in the tropical Pacific described in section 4.1. 77 % of the trajectories have a probability greater than zero to entrain into a convective cloud in a time period of 10 minutes. The mean probability for entrainment for an individual trajectory (including zero values) is 1 % and the maximum value is 13 %. The figure on the next page shows the frequency distribution.

The trajectories which have a probability greater than zero to entrain are distributed over about 1000 grid boxes. About 20 trajectories per grid box have an average chance of more than 1% (each) of entraining into a convective cloud within 10 minutes. It is clear from these numbers that not only at any given point in time, there is large number of trajectories capturing an updraft, but also that all individual grid boxes are covered well.



Changes to the manuscript: Changed formulation at page 3, lines 1–2 to: "Typical resolutions of meteorological analysis data are of the order of 1° x 1°. A grid box of the analysis typically contains several convective systems which only affect a small fraction of the mass contained in the grid box, which necessitates a statistical approach."

Maybe this issue is addressed on p. 4 line 4 ("The mass of a trajectory [. . .]")

Part of the issue is addressed here. The equations of the model are independent of the mass of the trajectory air parcel (for example, Equation 3). Thus, in a global model where the trajectories fill the model domain, a larger mass associated with a trajectory parcel (i.e. a lower density of trajectories per volume) leads to a lower number of trajectories in convection at a given point in time, which balances the higher mass moved per convective event.

Also, in response to a comment of the other reviewer, we considerably rephrased and extended the paragraph.

Changes to the manuscript: We considerably extended the discussion at the end of section 2.1 and moved the discussion to a new section 2.2 (in response to the other reviewer).

- E) **In the examples shown, timesteps of 10 or 30 min (why this difference?) have been chosen. I regard these timesteps as way too large to apply the approach outlined in sections 2.2–2.4: since updraft velocities can be up to 20 m s^{-1}, a timestep of 30 min injects a near-surface air parcel deep into the stratosphere. How can this work?**

The simulation time step inside the convective event is 10 seconds and not 10 minutes (e.g. original manuscript page 3, lines 12–15 and page 5, lines 9–14). The choice of the timestep is discussed under consideration of the updraft velocities on page 5, lines 13–14.

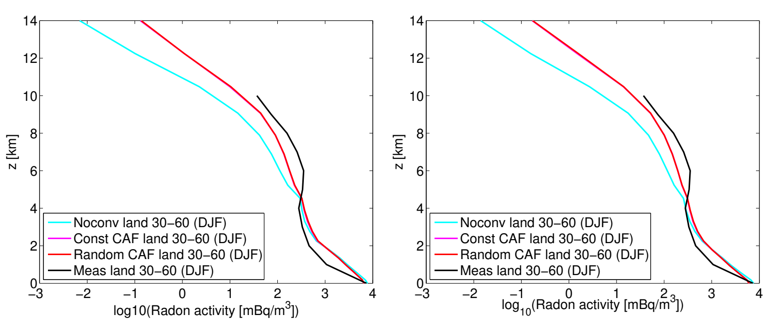
We are aware that the two time steps for the large scale advection outside convection (Δt) and for the updraft inside convection (Δt_{conv}) can easily be confused. We have now clarified some of the notation.

Changes to the manuscript: Clarified the notation. In particular, we have changed "trajectory time step" consistently to "advection time step of the trajectory model" and changed "intermediate time step" consistently to "convective intermediate time step".

. . . timesteps of 10 or 30 min (why this difference?). . .

The difference is due to computational constraints. The long-time run comprises more than 15 years. Simulation time is considerably reduced by changing the time step from 10 min to 30 min without changing the results significantly (the time step is still much shorter than the lifetime of radon).

1-year runs with a time step of 10 minutes, $0.75^{o} \times 0.75^{o}$ resolution of the analysis and a mean distance of the trajectories of 75 km have been performed to demonstrate that the results do not change significantly. They show that the runs with a time resolution of 30 min, a horizontal resolution of $2^{o} \times 2^{o}$ and a mean distance of 150 km give nearly identical results (see figure, left: $2^{o} \times 2^{o}$, 30 min from Fig. 10 manuscript, right: $0.75^{o} \times 0.75^{o}$, 10 min).



In response to a comment of the other reviewer, we increased the resolution of the ERA Interim reanalysis to $0.75^{o} \times 0.75^{o}$ for the high resolution run. In addition, the runs from section 4.1 and the SO_2 run are based on ERA Interim $0.75^{o} \times 0.75^{o}$ analysis data now.

Changes to the manuscript: Added to section 4.4 (section 4.2 in the original manuscript): "The change from 10 minutes to 30 minutes and from $0.75^{o} \times 0.75^{o}$ to $2^{o} \times 2^{o}$ is due to computational constraints. We performed 1-year test runs with $0.75^{o} \times 0.75^{o}$ resolution, a 10 minute time step and a mean horizontal distance of 75 km of the trajectories that show that the results of the run with the lower horizontal and time resolution are nearly identical.". Changed the resolution of the ERA Interim data in the runs in section 4.1 and section 5 to 0.75° x 0.75°.

- F) **Figure 3 is not properly discussed: how is this profile applied in the extratropics? There it does not make much sense that convection can reach an altitude of 15 km ... so the profile should be scaled with the local tropopause height.**

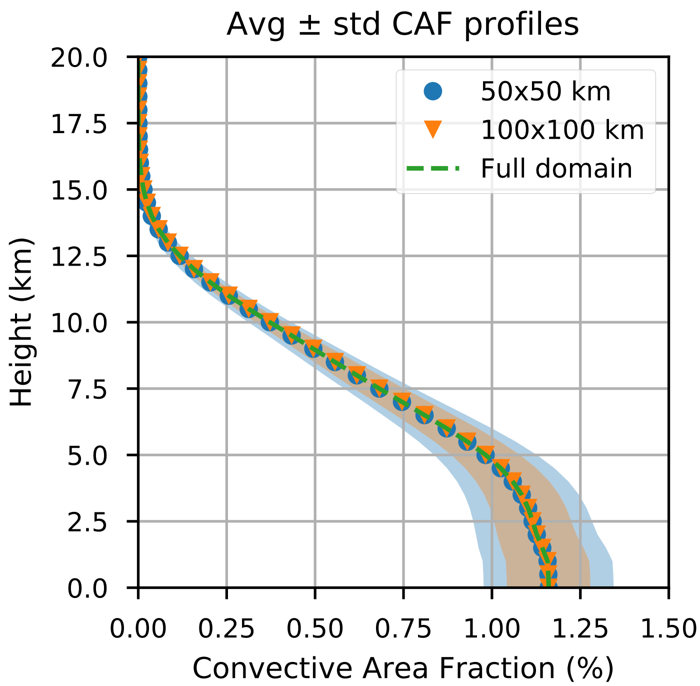
  We agree that this was not clear. The scheme was originally developed for an application in the tropics (original manuscript page 8, line 21). Strictly speaking, an application of the algorithm in the extratropics would require a different convective area fraction profile. However, the global long-time simulations of radon are not sensitive to the choice of the convective area fraction profile because of the globally constant lifetime of radon (see explanation in reply to comment I). Hence, using a tropical profile in the radon runs does not noticeably change the results compared to a run using a profile for the mid-latitudes.

  Changes to the manuscript: We added additional discussion along these lines in section 3.1 and a detailed explanation in new section 4.4.4 (see reply to major comment I).

  And the values for the convective area fraction, is it correct that they only make sense for a given grid size

  This is correct and we agree that it is important to discuss this in section 3.2. The frequency distribution of the measured convective area fractions depends on the domain size of the CPOL radar. The domain size should be comparable to the grid size of the meteorological analysis data to obtain a meaningful distribution of vertical updraft velocities. The full domain size of the radar is 190 x 190 km^2, which is comparable to the horizontal resolution of 2° x 2° of the ERA Interim data. As the domain size decreases, the frequency distribution approximates a bimodal distribution: In the limit of domain sizes below typical cloud sizes, the fraction can only be 0 or 1. That is, grid cells completely covered by convection and completely free of convection become more frequent (e.g. Arakawa and Wu, J. Atmos. Sci., 70, 7, 1977-1992, 2013).

  It is desirable that the method gives meaningful results for other resolutions than 2° x 2° and can be applied in the range of typical GCM and reanalysis resolutions. In fact, in response to the other reviewer, now also runs with 0.75° x 0.75° resolution are performed.



The figure shows the dependence of the standard deviation of the frequency distribution of measured convective area fractions on the used domain size of the CPOL radar. Results are shown for domain sizes of 190 x 190 km^2, 100 x 100 km^2 and 50 x 50 km^2. For the smaller domain sizes, the measurement domain of the radar has been divided into smaller subdomains. It is evident that the frequency distributions for different domain sizes differ significantly.

The current implementation of the algorithm does not consider this effect, and it is not clear if incorporating a distribution of the convective area fractions which depends on the grid size would lead to a significant change of the results of trajectory runs or not. An implementation of frequency distributions of the convective area fraction that depend on grid size is only planned for a future version, since this would mean a considerable additional effort.

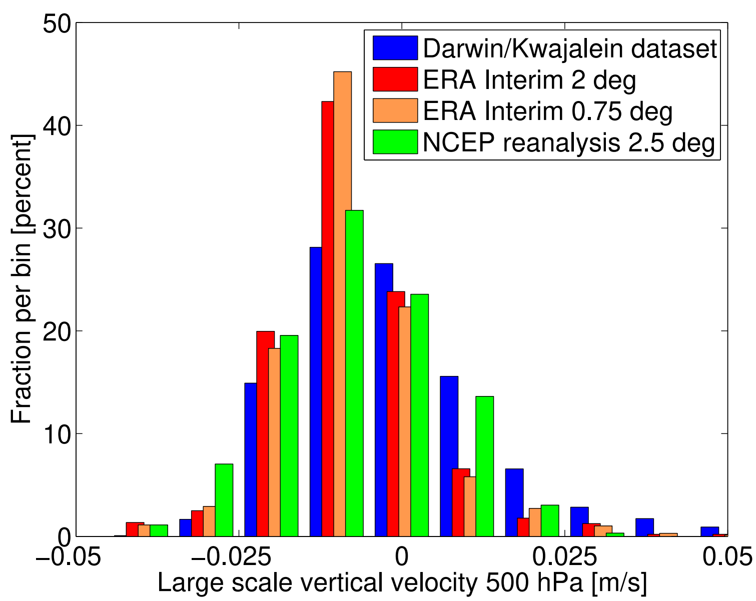
Changes to the manuscript: Added discussion to section 3.2 along the lines outlined above. Added figure of the standard deviations for different domain sizes.

- G) **However, quantitatively the vertical velocity field is extremely sensitive to the choice of the reanalysis (e.g., NCEP vs. ECMWF) and even more so on the resolution (e.g., ERA-40 vs. ERA-**

Interim). Therefore — it seems to me — the frequency distribution must be recalculated each time data is used from a different model / reanalysis. Please discuss.

This is a good point. It is important for the method that the large-scale vertical velocities from the Darwin/Kwajalein dataset and the large-scale velocities from the reanalysis used for the trajectory calculations have a similar distribution.

The figure shows the frequency distributions of the vertical velocity at 500 hPa from the Darwin dataset, ERA Interim and NCEP, and additionally, two different horizontal resolutions for ERA Interim (0.75^o x 0.75^o and 2^o x 2^o resolution). For the reanalysis data, the vertical velocity at 500 hPa at all grid points between 180^o E and 240^o E and 30^o S and 30^o N for the arbitrary date 1 June 2010 is used. The frequency distribution of the large scale vertical velocities of the Darwin dataset compares sufficiently well with the frequency distribution of the reanalyses and differences are acceptable in view of other uncertainties of our method, e.g. the uncertainties of the convective area fraction.

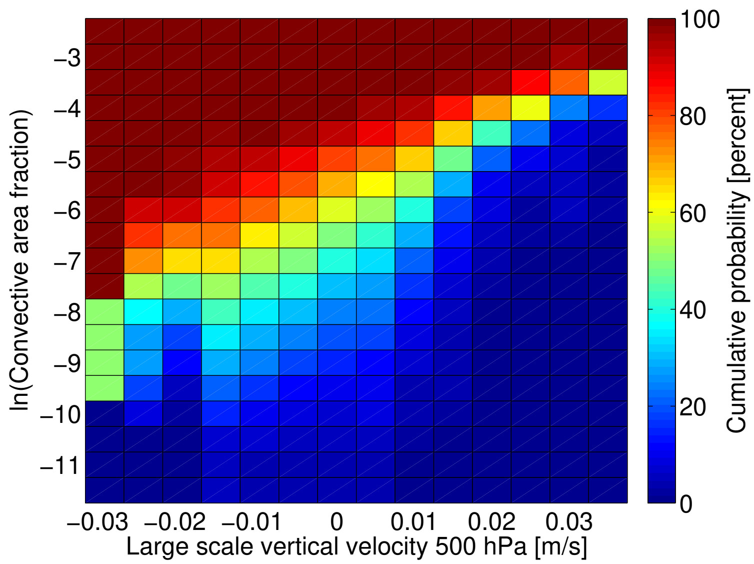


Hence, we did not apply a scaling or other correction to the large-scale vertical velocities from ERA Interim. But there may be cases where the vertical velocities from different reanalysis datasets have to be shifted or scaled to obtain a realistic distribution of the convective area fractions.

Changes to the manuscript: We added a paragraph and the figure above to section 3.2 and discuss the dependence of the method on the different distributions of the large-scale vertical velocity fields in the different reanalyses.

The resulting lookup table is mentioned but nothing is shown.

The figure shows the cumulative distribution of the convective area fraction as a function of the large scale vertical wind, which is used as the lookup table.



Changes to the manuscript: Added the figure showing the lookup table to the new manuscript.

- H) **Where simulation results are described and interpreted (e.g., p. 15 line 17), the paper is very brief. The reader would like to better understand the differences between the experiments.**

We expanded the discussion of the radon runs in section 4.4.4 (section 4.2 in original manuscript). We added that the runs with convection generally show higher radon concentrations than the runs without convection in the middle and upper troposphere due to the fast transport of radon from the boundary layer to the detrainment level. A more detailed interpretation of the profiles is however difficult due to the large-scale horizontal averaging.

We added additional discussion to section 4.4 (section 4.2 in the original manuscript) on how well the results of other studies compare to radon measurements to put the comparison of our model to radon measurements into perspective. Other studies show differences between their models and the radon measurements of a similar order of magnitude (see major comment B).

A discussion of the implications of the results of the SO_2 runs and of the scientific relevance of developing a convection model which simulates the time spent in updrafts was added: We added a discussion of the implications of the differences in the simulation of SO_2 in the different sensitivity runs to section 5 and a paragraph discussing very-short lived bromine species to show that this is also relevant for other species than SO_2.

Changes to the manuscript: Expanded the discussion of the radon runs in section 4.4.4. Added discussion to section 4.4 (section 4.2 in the original manuscript) on how well the results of other studies compare to radon measurements. Added discussion of the implications of the results of the SO_2 runs to section 5. Added a paragraph discussing very-short lived bromine species to section 5.

- I) **I must say that I don't understand the so-called "random CAF scheme". First, the description in Section 3.2 is not clear to me.**

  Vertical updraft velocities are obtained from combining convective mass fluxes from meteorological analysis data with a parameterization of convective area fraction profiles. We implement two different parametrizations for the convective area fraction, a parametrization using an observed constant convective area fraction profile as well as a parametrization which uses randomly drawn profiles to allow for variability in the convective area fractions. We rephrased the abstract, introduction and conclusions to make that more clear and rephrased section 3.2 to provide a more detailed explanation.

  Furthermore, we hope that the reply to comment F (dependence of convective area fraction on grid size) and comment G (dependence of large scale vertical velocity on reanalysis, figure showing lookup table) and the additional discussion in section 3.2 make it more clear what has been done.

  Changes to the manuscript: Rephrased abstract, introduction, section 3.2 and conclusions along the lines outlined above.

  Then, from Figs. 13 and 14 it looks like "random CAF" differs quite a bit from "constant CAF", but when looking at the tracer experiments (Figs. 9–12, 15), then the two schemes yield almost identical results. Why is this the case?

  The reason for the almost identical results for the radon simulations is that the lifetime of radon is globally constant. For a tracer with a globally constant lifetime, it makes no differences if it was transported slowly upwards from the emission at the boundary layer to 10 km in the last 10 days or if it first was transported quickly by convection to 10 km within one hour, and then stayed at 10 km for 9 days and 23 hours. The amount of radon that decays only depends on the time passed since the last contact with the boundary layer, when it was emitted (see original manuscript page 15, lines 21–26 and new section 4.4.4).

  Differences have to be expected for the SO_2-like tracer. These differences are relatively small in our model runs, which means that the results are insensitive to the uncertainties in the parameterization of the vertical updraft velocities.

Changes to the manuscript: We added additional discussion of the effect of globally constant lifetimes along the lines outlined above to section 4.4.4.

And why then should the reader and in general the CTM user community care about the difference between the two schemes?

It is not implied in the text that the community should care about the difference. It is a valid approach to try out several approaches in a new algorithm and to see what works best or if several approaches yield similar results.

Minor comments

- **page 1, line 15: this last sentence appears totally unrelated to the rest of the abstract. Include what the outcome is of this updraft velocity validation.**

  The sentence was directly related to the preceding sentence, which mentioned the validation of the mass conservation and validation with radon.

  Changes to the manuscript: We rephrased the abstract to include the main results of the validation.

- **page 1, line 18: "correct" $->$ "accurate" or "appropriate"**

  Changes to the manuscript: Changed.

- **page 2, line 28: no need for future tense**

  Changes to the manuscript: Changed.

- **page 3, line 14: "and" $->$ "times"**

  Changes to the manuscript: Changed to "multiplied by".

- **page 3, line 31: How does the updraft dominate the downdraft mass flux? By intensity? Integrated over the domain, they must be very similar, given mass conservation.**

  This is a misunderstanding caused by the confusion of the downdraft mass flux in the cloud with the slow subsidence outside of the cloud. The subsidence outside the clouds has to balance the convective mass flux inside the clouds (sum of updrafts and downdrafts), see section 2.6 (2.5 in the original manuscript).

  Changes to the manuscript: We added the phrases "updraft inside clouds", "downdraft inside clouds" and "subsidence outside clouds" at some additional locations.

- **page 4 and 6: combine Figs. 1 and 2 as two panels in one Figure**

  We would like to keep the separate figures. We do not see a benefit in combining the figures.

- **page 5, line 9: this sentence is awkward, please rephrase.**

  Changes to the manuscript: Split up into two sentences: "If a parcel is marked as taking part in convection, it is transported upwards for the vertical distance that it will be able to ascend in one intermediate convective time step Δt_{conv} (10 seconds). The vertical distance is determined by the vertical convective updraft velocity."

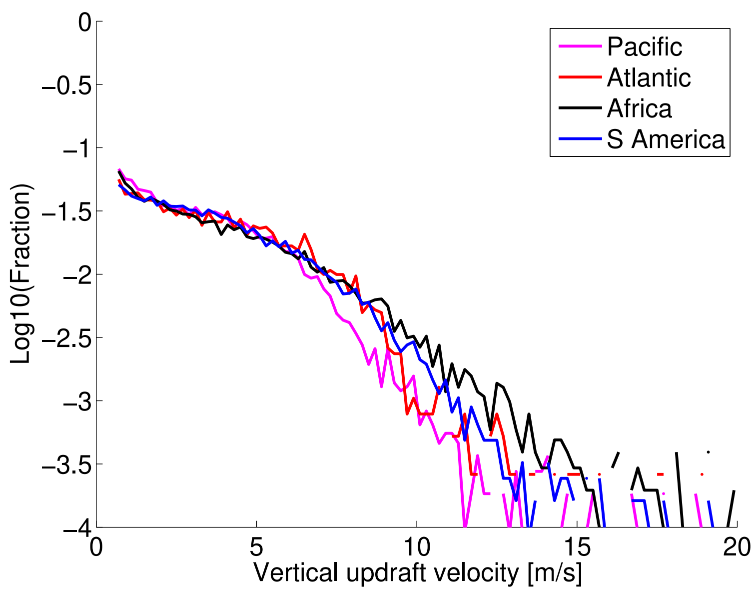
- **page 5, line 13: "m/s" − > $\mathbf{m\,s^{-1}}$**

  Changes to the manuscript: Changed throughout the manuscript.

- **page 6: why is section 2.4 not directly after 2.2?**

  This is the natural temporal order of the events: 2.2 entrainment, 2.3 upward transport, 2.4 detrainment. This is also the order of the steps in the algorithm (see original manuscript page 3, lines 11–16).

- **page 10, line 7: I would be curious to see pdf of wu for different regions.**



  The plot shows the pdf of the vertical updraft velocities derived from ERA Interim (model level 21, corresponding to about 520 hPa, June 2010) for four different regions: Pacific (180–240° E, 15° S–15° N), Atlantic (330–345° E, 15° S–15° N), Africa (0–45° E, 15° S–15° N), South America (285–315° E, 15° S–15° N). There are no significant differences for velocities below about 7 m/s. The percentage of velocities > 20 m/s is lower than 0.1 % for all regions.

- **page 11, line 3: "simplified and non-realistic" − > "idealized"**

  Changes to the manuscript: Changed.

- **page 11: Figure 4 is not discussed at all.**

  This is only intended as an example, and we feel that a short description is sufficient.

- **page 13: combine Figs. 6 and 7 as two panels in one Figure.**

  See comment to page 4 and 6 above.

- **page 15: the order of the sections is somehow strange: 4.3 would be better after 4.1 and 4.2 and 4.4 are also somehow related.**

  Changes to the manuscript: Changed as requested. Moved section 4.2 (original manuscript) to the end of section 4. Section 4.2 (original manuscript) is now section 4.4 (new manuscript), section 4.3 (original manuscript) is section 4.2 (new manuscript) and section 4.4 (original manuscript) is section 4.3 (new manuscript). Divided 4.4 into additional subsections.

- **page 16: combine Figs. 9-12 as four panels in one Figure.**

  See comment to page 4 and 6 above.

- **page 20, line 3 and 13: sentences should not start with "i.e." or "e.g."**

  Changes to the manuscript: Changed to "That is" and "For example", respectively.

- **page 20, line 2: why does the random CAF scheme lead to higher velocities? This is not clear to me.**

  The fact that the vertical updraft velocities are typically larger when a randomly drawn convective area fraction profile is used can be readily understood qualitatively: Assuming that M, T and p are fixed, the mean updraft velocity in case of a mean constant convective area fraction profile $\langle f_{\rm up}\rangle$ is simply $\langle w_{\rm up1}\rangle = \frac{MRT}{\langle f_{\rm up}\rangle p}$, where $\langle \ldots \rangle$ denotes the mean over all air parcels. In the case of a varying randomly drawn convective area fraction profile, the mean vertical updraft velocities need to be expressed as $\langle w_{\rm up2}\rangle = \langle \frac{MRT}{f_{\rm up}p}\rangle = \frac{MRT}{p}\langle \frac{1}{f_{\rm up}}\rangle$. Since $\langle \frac{1}{f_{\rm up}}\rangle \geq \frac{1}{\langle f_{\rm up}\rangle}$ due to the fact that the harmonic mean is always smaller than the geometric mean, we obtain the relation $\langle w_{\rm up2}\rangle \geq \langle w_{\rm up1}\rangle$. This implies that also individual realizations of $w_{\rm up}$ are on average larger for the random convective area fraction profiles.

  Changes to the manuscript: Added discussion to section 4.2 (section 4.3 in the original manuscript) along the lines discussed above.

- **page 22: Figure 15 clearly shows the most relevant and interesting result of the paper. I understand that no observations are available to verify these profiles, but I think a more detailed discussion of these profiles is important. The differences are fairly large. What does this imply for tropospheric chemistry?**

We agree that a discussion of the implications of the results of the SO_2 runs and of the scientific relevance of developing a convection model which simulates the time spent in updrafts is important. We added a discussion of the implications of the differences in the simulation of SO_2 in the different sensitivity runs to section 5. In addition, we added a paragraph discussing very-short lived bromine species to show that this is also relevant for species other than SO_2.

Changes to the manuscript: We extended the discussion in section 5 by adding paragraphs discussing the implications of the changes in the SO_2 simulations and a paragraph discussing very-short lived bromine species as an example for another species for which this could be relevant.

How would the results look like if using a convective transport scheme as implemented in other CTMs...

This is a question we are also interested in. We added discussion of how well the results of other models compare to radon measurements in section 4.4.4. A detailed comparison study of several convective transport models is outside the scope of this technical presentation of an algorithm. This would mean a considerable additional effort.

Differences between different models in other studies will often mainly be due to differences in the underlying convective parameterization (see e.g. Feng et al., 2011). This is however a very extensive and difficult topic (e.g. Arakawa, 2004), which is outside the scope of this study.

Changes to the manuscript: We added some discussion of how well the results of other models compare to radon measurements in section 4.4.4.

...or in FLEXPART?

FLEXPART does not provide single trajectories as output which one could use to run a box model. We are restricted to the build-in simplified chemistry schemes, which are an exponential decay with a fixed lifetime and a simple OH scheme (e.g. Pisso et al., Geosci. Model Dev., doi:10.5194/gmd-2018-333). Hence, it is not possible to do a meaningful comparison due to constraints in FLEXPART.