Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-5-RC1, 2019
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Geoscientific Model Development
Discussions
Open Access

Interactive comment on "A Lagrangian convective transport scheme including a simulation of the time air parcels spend in updrafts" *by* Ingo Wohltmann et al.

Anonymous Referee #1

Received and published: 31 March 2019

This paper presents a Lagrangian convective transport scheme for chemistry transport models (CTMs). The scheme is implemented in the ATLAS CTM and idealized and realistic test simulations are shown to compare the performance of different variants of the scheme. The paper also includes some validation with wind profiler measurements in the tropics. Whereas I fully agree that the development of Lagrangian convective transport schemes is important and challenging, I found this paper unclear in many places, the novelty was not apparent to me, and the results are not discussed in depth. In its present form, the paper is unsuitable for publication. I recommend very major revisions (see comments below).

Major comments

A) The novelty of this study is not apparent to me. Which elements of this convective transport scheme are "standard" / have been used in other schemes, and which elements are new? 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 authors should discuss in greater detail how their scheme differs from existing schemes, e.g., the ones mentioned on p. 2 line 5.

B) Related to A) but more general: 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.

C) The writing is unclear in many places: - p. 1 line 1: what is meant by "ensemble trajectory simulations"? - p. 2 line 3: the same - p. 2 line 16: explain better what is meant by "instantaneous redistribution" - p. 3 lines 23 and 27: unclear to me what exactly is meant by "meteorological data" - p. 4 section 2.2 and p. 6 section 2.4: is this treatment of entrainment and detrainment "standard", i.e., as in other schemes, or is there some novelty here? - p. 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. - p. 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. - p. 10 line 3: I don't think that the character of the method is "random", most likely you mean "probabilistic" or "stochastic".

D) My most important concern is related to the fundamental question of how many air parcels / trajectories are required per reanalysis or GCM grid box in order to care about "updrafts". As is discussed in this study, the area covered by updrafts is relatively small even in a region with active deep convection. Figure 3 shows that this area covers less than 1/100 of a 190 x 190 km2 grid box (p. 8 line 19). To me this strongly indicates that many trajectories are needed per grid box in order to "resolve" / capture at least one or better several of the assumed updrafts in this grid box. However, if there are only relatively few trajectories per grid box, then it does not make sense to explicitly simulate the ascent in updrafts and the question how long a trajectory resides in an updraft becomes obsolete. If my rough estimation is correct, then all simulations performed in this study have far too few trajectories to capture updrafts: e.g., for the idealized experiments in section 4.1 there are 100'000 trajectories for a 60° x 60° domain. Given the ERA-Interim grid boxes of 2° x 2° used in this study, this means that there are roughly 30 x 30 = about 1000 grid boxes, and therefore there are 100 trajectories per grid box. Since (see above) the convective area is <1/100, there is on average not even 1 trajectory that captures an updraft. Things then get much worse for the Radon experiment (section 4.2) where only about 20 trajectories are initialized per 150 km x 150 km grid box (p. 14 line 11). Given such a model setup, I don't understand the general concept of the updraft residence time used in this study. Maybe this issue is addressed on p. 4 line 4 ("The mass of a trajectory ... is typically much larger than the mass transported in a single convective event"), but I could not understand this sentence. Either a much better explanation of the approach or a strongly increased number of trajectories in required to convince me about the feasibility of the convective transport scheme presented in this study.

E) A problem potentially related to D) (at least in my understanding) is the choice of the simulation timestep. 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?

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

i.e., the values must be adjusted if a model is run at higher / lower resolution? This should be discussed.

G) p. 9 line 7: This "deriving of the frequency distribution" is based on 500 hPa vertical velocity from reanalyses or a GCM. 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. The resulting lookup table is mentioned but nothing is shown. Hence, the reader remains unclear how this works and how the result looks like.

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.

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

p. 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.

p. 1 line 18: "correct" –> "accurate" or "appropriate" since we never know the "correct" value.

p. 2 line 28: no need for future tense "First, we present ... and introduce ...".

p. 3 line 14: "and" –> "times"

p. 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.

p. 4 and 6: combine Figs. 1 and 2 as two panels in one Figure.

p. 5 line 9: this sentence is awkward, please rephrase.

p. 5 line 13: “m/s” –> m s-1 (and in other places)

p. 6: why is section 2.4 not directly after 2.2?

p. 10 line 7: I would be curious to see pdf of wu for different regions.

p. 11 line 3: “simplified and non-realistic” –> “idealized”.

p. 11: Figure 4 is not discussed at all.

p. 13: combine Figs. 6 and 7 as two panels in one Figure.

p. 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.

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

p. 20 lines 3 and 13: sentences should not start with “i.e.” or “e.g.”.

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

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

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