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Interactive comment on "Revision of the convective transport module CVTRANS 2.4 in the EMAC atmospheric chemistry–climate model" *by* H. G. Ouwersloot et al.

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We thank Referee #2 for his/her comments that contribute to clarify the manuscript. Furthermore, we are pleased that he/she appreciates the necessity of the presented improvements for realistic convective transport of atmospheric tracers. In general, the major comment of Referee #2 is the use of the word "significant" where no statistical significance is determined. This will be remedied in the revised manuscript. Below we respond to the comments point by point and include the modifications that will be applied to the revised manuscript. Original comments are displayed in italic font.

The use of a single experiment of one year's length for each configuration makes it quite difficult to gauge the significance of any differences. The manuscript uses the vocabulary "significant" throughout the manuscript with no foundation. I would recommend either running small ensembles, longer experiments, or as a minimum removing the word "significant" from the manuscript and substituting the vocabulary about definitive differences with more relative terminology (ie., the mixing ratio in the xxx experiment is 10% larger than in the xxx experiment).
and
General: Please remove the use of the word "significant" when discussing differences among experiments. Please use the value of the RMS relative to the mean mixing ratio, ie., 10% difference.

We agree with the reviewer that the use of the word "significant" is misleading, since it is not used in the statistical sense. Indeed, we don't use it to quantify whether induced changes are unlikely to be caused by chance (alone), but rather to say whether induced changes have an impact that affects numerical studies. To prevent misunderstanding, we will rephrase throughout the document and make use of synonyms that do not refer to statistical significance. As per Referee #2's suggestion, changes will be expressed in relative deviations everywhere.

The use of one of the sub-stepping experiments as the standard for other experiments seems unwarranted. At best an experiment with small time steps seems like a potential "ground truth". Without a better "standard for truth" the vocabulary about improvements and degradations has no basis. Please either use a short time step run as the standard, or amend the vocabulary about differences to remove the value assessments.

Linked to the previous comment, we will amend the vocabulary.
Furthermore, for the sake of clarification, it should be noted that all changes are actu-

ally significant (i.e. not the result of chance), based on two observations: first, (random) fluctuations in other components than convective transport of tracers do not influence the comparison between numerical simulations, since the convective transport of the inert tracers does not affect the atmospheric dynamics, leading to binary identical results where the atmospheric tracers are not concerned. (Note that convective transport of moisture is treated by the CONVECT module, as will be clarified in the revised manuscript.) Second, as presented in the response to Referee #1, the weighted Root Mean Square Deviations (RMSD) are similar for different seasons, showing that the deviations are not a matter of chance.
Since the deviations are shown to converge for smaller values of f_{maxfrac} and recirculation effects are captured better when using more intermediate time steps, we are confident that I001 is a solid base experiment, which is closest near the absolute truth and closer to that than to e.g. experiment I005. However, we do acknowledge that I001 is not the absolute truth itself and will rather represent it as "best representation" in the revised document. Moreover, Table 2 will be updated to also present the influence of the adapted updraft plume base and the convective cloud cover on experiment I001.

It is not clear whether CVTRANS is used for transport of chemical species only, passive tracers only, or for the transport of moisture and heat as well. it reads as though CVTRANS is NOT use to transport moisture (and cloud condensate). if that is the case, please discuss/justify.

The interpretation of the reviewer is right. In EMAC the CVTRANS submodel only determines the convective transport of tracers other than water. The convective transport of water is linked to the convection scheme and is therefore directly calculated by the CONVECT submodel. We will clarify this by including "for tracers other than water" in line 10 on page 3120.

In addition - the restriction on the convective in the control experiment is not quite clear - when the CFL criterion is violated, is all transport turned off, or is the transport limited to the amount needed to meet CFL?

In the original code, transport is indeed limited to the amount needed to meet the CFL criterion. This is stated on page 3122, lines 15 - 16: "if F_{up}^{k} exceeds $\frac{M^k}{\Delta t}$, it is truncated to that value in the CVTRANS calculations to prevent instabilities and negative mixing ratios that may arise".

The description of the "analytical expression" is also not clear. Does the control experiment not use this "analytical expression" for the change in mixing ratio below cloud base? So the subsidence does not extend down into the cloud base in the control experiments?

As the analytic expression is one of the applied modifications, it is not present in numerical experiment ORG. Subsidence does still extend down into the cloud base, but the effect of recirculation is not accounted for: all air that escapes has the original properties of the grid cell below cloud base and is replaced by subsiding air masses. The analytic expression "accounts for the [in certain cases] significant influence of the updraft plume on the sub-plume mixing ratio evolution within the time step". As explained in Sect. 2.2.2, this is done by applying $\left\langle C_{\mathsf{env}}^{k_{\mathsf{b}}} \right\rangle$ (expressed by Eqs. (10-11)) instead of $C_{\mathsf{env}}^{k_{\mathsf{b}}}$ in Eq. (5).

Please explain why you reduce the mass flux per unit ares in the CC experiment. One could imagine an option where the conv mass flux per unit area is unchanged and the assumption of total cloud cover would mean an INCREASE in total mass flux in a grid box.

As the mass flux is not determined by the CVTRANS module, it is not adapted by the applied modifications and differences in settings. As stated in Sect. 2.1, the total mass flux is determined by the convection module, CONVECT, which functions independently from CVTRANS. For CVTRANS, the cloud cover needs to be diagnosed to determine over what area the “leaky pipe” representation is concentrated.

the description of the experiments is confusing at best. a table listing the experiments and their names is sorely needed. How long did the experiments run for?

Although the naming convection is straightforward and explained in Sect. 3, we will include a summarizing table that contains the settings and run time of the numerical experiments.

There is no reference for the relevance of the magnitude of the standard deviations. For instance, P. 3127 Line 12 refers to standard deviations of 5% of mixing ratio. Is that large (as the text suggests?) or small, or within natural variability?

Considering that yearly averaged data is evaluated, a difference of 5 % is large. Of course, the importance of the deviation depends further on the application and as such has a subjective component. When e.g. two numerical models are compared with identical initial and boundary conditions, a 5 % difference will be more reason for concern than the same difference between numerical model results and observations, for which the uncertainty in initial and boundary conditions, as well as the uncertainty in observations, should be considered.
Furthermore, we would like to reiterate that in our numerical experiments the changes in CVTRANS only affect the distribution of atmospheric tracers, since the inert tracers

do not interact with the thermodynamics and dynamics. As a result, the dynamics are binary identical between the different numerical experiments. Therefore, all differences are solely attributed to the applied modifications. The modifications further lead to clear patterns that are e.g. consistent for different values for f_{maxfrac}. Additionally, we want to mention here again that when evaluating individual (shorter) periods, similar values for the RMSD are found (as presented in the response to Referee #1). This all indicates that the induced differences are systematic. Furthermore, the systematic nature of the changes is supported by the given that the presented resulting patterns do correspond with the expected shifts in tracer distribution.
Whether the systematic difference of 5 % is within the random natural variability is a reasonable question, but does not diminish the applicability of our modifications. When e.g. weather patterns are different, the induced differences could be stronger than the difference between the original and revised numerical representations. However, when averaged over a longer period, those random differences will disappear while these systematic differences remain.
For the quantification of the differences it is important to realize that convective transport of moisture is treated by the CONVECT module and is therefore not affected in this study. As indicated earlier in this response, this will be clarified in the revised manuscript.

A single experiment with each configuration of 2-year’s duration (where we see only the results of the averages for one of the years) is not sufficient to measure differences. Longer (or more) experiments would strengthen any argument about differences.

We disagree that for tracers with a maximum lifetime of 50 days a year of data after a year of spinup would be insufficient to measure differences. It even exceeds common practice like averaging over a month (Lawrence and Rasch, 2005) or 4 months (Tost et al., 2010). Again, the robustness of the differences is supported by the RMSD evaluation for different seasons.

Discussion of figure 1 - what is the surface value? i.e.., the figure shows values near 5-10 or less in the ORG experiment and another 30 in the 100I experiment. difficult to assess without knowing surface mixing ratio. is vertical transport in the ORG experiment almost eliminated? What is the behavior of the transport in this experiment with a smaller time step?

We chose to only present the figures that contain most information, but agree that relevant information for the interpretation of these figures is missing. To provide readers the opportunity to analyze the figures in further detail while keeping the manuscript concise, additional figures will be included in an electronic supplement. For each global difference plot (Figs. 1b and 6b), the reader will have access to the mixing ratios for both experiments, both at the surface and at a height of 700 hPa, as well as the mixing ratio difference, both absolute and relative to the original values at that height.

Not clear that both figures 1 and 2 are needed to show that the difference between an experiment with and an experiment without much convective transport of constituents is to find more tracer aloft. perhaps figure 2 suffices.

Both figures give different information. While Fig. 1 serves to illustrate the global patterns and indicates geographical areas where strongest differences are found, Fig. 2 shows the distribution with height.

Page 3129 lines 1-5 - by what criterion do you assess the ORG transport to be overestimated and the 100I experiment to be overestimated? If you have no basis for these terms please use relative terminology.

The underestimation of convective transport in the ORG numerical simulation directly follows from the procedure in which mass fluxes are capped to fulfill the CFL criterion. Furthermore, it is supported by comparing it with the results from numerical experiment I001, which is the best representation and close to the “ground truth”. Likewise, the overestimation of convective transport in numerical experiment I100 is to be expected since convection can transport majority of the air in grid cells away within one step without accounting for replenishing air being partly used for this outward transport as well. It boils down to a differential equation that is numerically solved with coarse time steps. As a result, the air that is removed is less influenced by subsiding air. Since the subsiding air is characterized by lower mixing ratios for exponentially decaying tracers that are emitted near the Earth’s surface, convective transport is overestimated. Again, this is confirmed by comparing this numerical experiment to numerical experiment I001.

The material in this section illustrates the issue with not having any sort of objective criterion about which transport is correct. Please add some discussion in either the introduction or in the section describing the model of the performance of the control (ORG) simulation with realistic tracers as compared to observations.

It is clear that numerical experiment I001 represents the “real transport” best. In this case convective tracer transport is resolved with the finest time steps (only allowing maximum 1 % of a grid cell to flow out of the control volume within one time step). As such, constrained by the representation of the mass fluxes in the CONVECT subroutine, this is the most accurate representation of convective transport of atmospheric tracers.
We will emphasize the use of I001 as best representation to quantify the RMSDs in the revised document.

the discussion about small (and probably not statistically significant) differences between two experiments should be removed. if the differences cannot be shown to stand above noise then there are no differences.

As stated before in this reply, there is no noise in the comparison between numerical experiments, since the dynamics are binary identical. Furthermore, there are differences present that are expected based on reasoning and are represented using Large Eddy Simulation Studies. The differences might be small, but the effect is systematic. Considering the recent literature and the fact that it is part of the presented model development, we will retain this section.

line 3 on page 3133 says that the differences are "very significant". please remove this as it has not been shown. Differences of 4% would probably we within the noise, and 27% may or may not be. The description of the results of these experiments can be removed.

The removal of the word "significant" will be included in the aforementioned rephrasing throughout the document. We disagree that the systematic change is unimportant due to noise. Furthermore, we stress that the resulting patterns, as shown in the figures, are consistent with expectations based on theory and indicate that deviations are not randomly distributed.

Figures 2,3 - please add some values to the vertical axis other than 1000 and 100. In addition, because the color bar chosen makes it difficult to see the zero line, please add a zero contour.

We will add the zero contour line and add additional information on the pressure axis.

figure 4 - the text in the legend is garbles in the pdf file.

We will clarify the use of I### by adding "Numerical experiments" in front.

Panel 'a' of figures 2,3,5,6 can be removed. it is the difference that is being discussed in the manuscript.

It is true that the differences shown by the figures are discussed in the manuscript, but that does not warrant removing the figures. We would like to emphasize that the differences shown by the figures are explicitly not equal to the quantification by the RMSD and assist to provide insight into the (changes in) distribution of atmospheric tracers. They indicate where convective transport is active and where it is enhanced or diminished. Furthermore, they reinforce that deviations are not random, but result in systematic patterns, which is shown to be important in this reply as well. As such, these figures support the manuscript.

References

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