Response to Review 2

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We thank the anonymous referee 2 very much for the helpful remarks. They lead to an improvement of our manuscript. In the following, we answer all comments one by one. The referees comments are *italic and blue* our answers are displayed in upright font and black.

1 General comments:

My main concern regarding this manuscript is the missing of an evaluation of the main development, the modifications in CVTRANS related to entrainment/detrainment. Are the corresponding changes in the transport matrix well-founded?

Tost et al. (2010) showed that CVTRANS can reproduce measured vertical profiles of insoluble tracers reasonably well. However, from a physical/process oriented point of view it makes no sense that there is in some model levels no turbulent mixing when applying the Tiedtke-Nordeng scheme, especially keeping in mind that according to Tiedtke (1989) entrainment and detainment consist of both an organised and a turbulent portion. We included the adaptations in CVTRANS to be consistent with Tiedtke (1989) and Tiedtke-Norden Nordeng (1994). However, we only wanted to include a weak effect of the turbulence (as in CVTRANSnew) to avoid changing the results significantly because CVTRANS performs already fine in terms of the convective transport (compare Tost et al., 2010). The results of CVTRANSturb deviate substantially from CVTRANSold (see Tost et al., 2010) and CVTRANSnew. Therefore, CVTRANSturb most likely overestimates the turbulent mixing and is consequently not used for the simulation of the convective transport on climate time scales.

Due to the fact that the differences between CVTRANSold and CVTRANSnew are rather small, we assume that both perform are comparably good. We argue that the variation of the transport features due to the changes in turbulence treatment in CVTRANSnew vs. CVTRANSold are well within the range of the other uncertainties, i.e. convection occurs in models usually not at the exact location of the (real world) convection and the convection parametrisations make use of many approximations. For this reason, another evaluation would have no added value.

To validate our decision to use CVTRANSnew, we included (1) a statement about the good performance of CVTRANS for non-soluble tracers as shown by Tost et al. (2010) in the section where we describe the submodel, and (2):

"The goal of the modifications is to overcome the issues with the missing turbulent mixing and not to alter the results of the simulations strongly from the original version, as it is the case for CVTRANSturb because Tost et al. (2010) showed already that CVTRANSold reproduces observed vertical tracer profiles. Therefore, in Sec. 3.2 CVTRANSnew is applied to investigate historical changes in the convective transport characteristics, because the deviations between CVTRANSnew and CVTRANSold are smaller than the ones between CVTRANSturb and CVTRANSold."

in Sec. 3.3 and

"We used the old version of CVTRANS (CVTRANSold), which performs reasonably well in comparison to observations (Tost et al., 2010) in order to contrast it to the new version of CVTRANS with the adaptations for the turbulence (CVTRANSnew). CVTRANSturb is similar to CVTRANSnew but with enhanced turbulence. The latter was used to demonstrate the sensitivity of the convective transport to turbulence; however, too strong turbulent mixing weakens the transport to the upper troposphere to a great extent. Short-cutting the convective transport to such an extent is not beneficial keeping in mind that CVTRANSold performs well, as pointed out by Tost et al.

(2010). Therefore, CVTRANSnew is used a compromise to keep the results close to the ones of CVTRANSold and to include the consistent turbulent entrainment and detrainment."
in the conclusions.
English language is sometimes difficult to understand.
Thank you for pointing that out. We hope that we could improve the language a bit and trust in the typesetting.

The manuscript is rather long and should be shortened.
We put together the Figures 8, 9 and 10 in one Figure and also shortened the discussion about these Figures. Further, we shortened some passages. However, we included one more Figure + description and one Table to make it easier to follow our analysis. Therefore, in total, we were not able to shorten the manuscript significantly.

2 Specific comments:

Titel: The historical climate trend resulted in changed convective transport patterns in model simulations. I would suggest to be more specific in the title: ...changed vertical transport patterns in climate model simulations.
We changed that.

Page 1, line 6: The statement that convection transports material less efficiently to higher layers, but transports more to the tropopause region seems contradictory to me.
We replaced the formulation to "The upward transport increased on average to height levels of 130 hPa and above, but convection transported material less efficient to the upper troposphere in general from 2011 to 2020 in comparison to the 1980s."

Page 1, line 15: rapid vertical transport time scales -> rapid vertical transport or short vertical time scales.
Thanks. We change "These lead to rapid vertical transport time scales and to a major redistribution..." to "These lead to short vertical transport time scales and therefore, to a rapid redistribution...".

Page 3, line 61: MESSy is an interface –> MESSy is a software framework ... (see also www.messy-interface.org)
We adapted the description of MESSy accordingly.

Page 3, line 68ff: I suggest to provide a complete overview of the submodels used (similar as in Tost et al., 2010, their Table 1)
We included a table of all submodels in the supplement, because we do not want to make the manuscript longer (see your main comment) and all relevant submodels are discussed in the text.

Page 3, line 72: Are SSTs and sea ice really nudged or are they prescribed?
Thank you for this comment. The SSTs and the sea ice are prescribed by the nudging data. We adapted the text. We wrote in the manuscript: "Temperature, vorticity, divergence, and surface pressure are nudged towards ECMWF Reanalysis fifth generation (ERA5, Hersbach et al., 2020) and the sea surface temperature and the sea ice coverage are prescribed by the nudging data."

Page 5, line 18: detraimen or entertainment -> detrainment or entrainment
We changed that.

Page 6, line 149: How large is a small number of tracers? < 10?
We are thankful, that your comment draw our attention to this comment again. An error has sneaked in here. Now, we correct: "For large numbers of tracers ($\sim$ 100 tracers), the computational efficiency is similar when the convective exchange matrix is applied ..."
For small numbers of tracers, it is necessary to calculate the additional artificial tracers needed to derive the convective exchange matrix and to perform the matrix multiplication. This is, of cause, computationally less efficient than calculating only the redistributed tracer field directly.

Page 7, line 169: Where does the 6.96% come from? I looked for it in the main diagonal, as described in Fig. 1, but probably the color bar does not discriminate enough between the values. What is meant by "large-scale subsidence

is a strong process"?
In Fig. 2, there are orange boxes on the main diagonal. These correspond to values in the order of 1%. We could not think of a colour scale which would enable us to see the exact numbers due to the fact that they vary by orders of magnitude. We took the value 6.96% directly from the calculated data set which we used to produce the plot.

Concerning your second question: Please look at the diagonal directly below the main diagonal. The fractions in this diagonal are usually very high (reaches up to ∼90%). Therefore, the convectively driven large-scale subsidence has a huge influence ≃ is a strong process.
We changed "The large-scale subsidence is a strong, but also a slow process." to "The convectively induced grid-scale subsidence is a strong process that has a significant impact on the diagonal below the main diagonal." to be more clear that we are referring to the processes in one grid box and not to synoptic-scale processes. We also changed that in the caption of Fig. 1.

Page 7, line 173ff: I would suggest that pressure values should be rounded to full hPa. We agree and rounded all pressure values.

Page 7, line 182: I am surprised that the Tiedtke scheme can provide mass fluxes above the tropopause. Is the convective height in accordance with the height provided by the Tiedtke/Nordeng convection parametrisation? Please comment.
The Tiedtke scheme itself does not provide an information about the tropopause nor uses the tropopause as any kind of transport barrier. Variables as temperature or humidity are known in the Tiedtke scheme which provide (indirectly informations about the tropopause).
We argue that the mass fluxes above the (model) tropopause are quite reasonable: In an extreme case the level of neutral buoyancy can be directly below the tropopause. However the level of neutral buoyancy is not (necessarily) the top level of the convective cloud.
Nevertheless, we agree that it is questionable that it can be considered as "real" overshooting into the stratosphere as we discussed.

Page 8, Fig.2c: How can it be that from level 14 (origin level) the upward transports (in CVTRANSturb) is smaller than from the origin layer 13? In your Fig.2c there is a jump visible, which is also present in Fig. 2b (same levels), but smaller in level 13 (it is the dark red area above the diagonal with values in the thousandths of a % range). The results from CVTRANSturb seem unrealistic. Could you please comment.
Thank you for pointing that out. We found a small bug in the calculation and fixed it.
Page 8, line 208: I don't understand the reference to Tiedtke regarding "maximised turbulent mixing". What is meant by that (I could not find it the Tiedtke publication)?
With "maximised turbulent mixing", we referee to the mixing as used by the Tiedtke 1989 (see his equations (13), (14) and (15)). It is the maximum strength of turbulent mixing we considered and tested in this study (see CVTRANSturb).
We agree that the formulation is irritating and we changed it. Now it says: The turbulent mixing in this case is enhanced compared to CVTRANSnew. It is as strong as the turbulent mixing suggested by Tiedtke (1989).

Page 9, line 212: "CVTRANSturb is not efficient enough transporting material to the upper troposphere". This statement is a validation of the transport in CVTRANSturb, which however is not justified. See my main concern in the general comments.
Here we argue as above, see main comments.

Page 10, line 228: Couldn't one have shown the convective transport matrix only for the cases where convection has occurred? And differentiated according to the 3 types of convection (deep, mid, shallow)? Yes, one can. We believe there are a lot of things which can be looked at / studied with the convective exchange matrix. In this study, we focus on the overall changes in convective transport and its influences on global and regional scales. Due to the large number of pages of our manuscript we were a bit restricted, what we can show and discuss beyond that. The overall mean convective exchange matrix includes the combined effects of intensity and frequency to provide the holistic view on convective mixing. We are planing to write at least one follow up study, in which further details will be explored.

Page 10, line 238: the effects by downdrafts are striking
We wrote "are" instead of "is".

Page 11, line 258: I doubt that convective overshooting is present in the Tiedtke-Nordeng convection scheme. Did you find overshooting events in individual deep convective events? This should be clarified and the text adapted accordingly. May be this is an effect of averaging the tropopause height and should not be interpreted as "overshooting". Please check.
We checked in every grid box, whether there is a mass flux above the tropopause height (of the same box at the same time) and there are such events. That does not happen very often but it does happen!
To make it more clear we changed "Thereby, overshooting is defined as events where the updraft mass flux reaches beyond the tropopause in one column." to "We define one (tropopause) overshooting event as an event when the updraft mass flux associated with one convective event reaches beyond the independently calculated tropopause in one column and at the same time step."

Page 12, line 269: Is it the increased penetration of convection that leads to a rise of the tropopause? According to Meng et al., 2021 it is the warming/cooling of the troposphere/stratosphere that modifies tropopause height (https://doi.org/10.1126/sciadv.abi8065)
Meng et al. (2021) show a monthly mean correlation between the tropopause rise and mainly the tropospheric warming. This study does not explain the physical processes taking place in detail. Therefore, the study by Meng et al. (2021) does not exclude the possibility that convection plays a part in all this. To name only one option: Due to a higher surface temperatures, convection could lead to an increased transport of moist static energy to the UT which has a warming effect in the UT and could contribute to an (further) rise of the tropopause height. Additionally, transported water vapour can potentially have a radiative feedback.
However, as we argue in our manuscript, we do not believe in a strong influence of the changes in convection on the tropopause height on a global scale.

Page 13, symetric -> symmetric
We changed that.
Page 14, Fig. 7: You state "crosses" in the figure, but I only see filled circles!
You are right. We changed crosses to blue circles in the figure caption.

Page 15, line 302: I find it somewhat unfortunate to speak of "zonal trends", since in principle this also includes spatial trends (along a zonal band). Suggestion for a section title: Tropical and extra-tropical trends
Your suggestion for the section title is great and definitely better than our former section title.

Page 15: line 311: I doubt, that stratospheric levels are reached, can you please check the height from CVTRANS results with the actual tropopause height from the convection parametrisation?
There is no "actual tropopause height from the convection parametrisation" as we explained above. So we cannot use it. However, we are not the only ones finding tropopause overshooting convection using the Tiedtke convection parameterisation:
Simmons et al. (1999) applied the Tiedtke convection scheme in the ECMWF model and they wrote: "Moistening at 70 hPa occurs when convection overshoots the model tropopause at 90 hPa and detrains at the level above." (Simmons et al., 1999).
Moritz Menken (German Aerospace Center) also found updraft mass fluxes in EMAC simulations applying the Tiedtke-Norden scheme reaching above the tropopause using a different setup as we did (personal communication Moritz Menken).

Figs. 9 and 10 Caption: omit "in the tropics"
Thank you a lot. We had overlooked that.

Page 17, line 329 – 337: This text passage is difficult to follow. Could you please summarize the important differences with respect to the global and the NH case?
This passage is no longer existing as we decided to shorten the discussion for the NH and SH case to reduce the

overall length. Further, we tried to increase the clarity here.

Page 18, line 340ff: Did you mean: the definition of the upper troposphere is defined as the vertical region between the level of the tropopause (in hPa) + 150 hPa?
Yes, that is what we mean. We rewrote the sentence "The upper troposphere is heuristically defined as the region including the level of the tropopause down to the level where the pressure is equal to the tropopause pressure plus 150 hPa". The new version of this statement reads: "The upper troposphere is heuristically defined as the region vertically ranging from the tropopause level (in hPa) down to the level where the pressure is equal to the tropopause pressure plus 150 hPa."

Page 18, line 350: The authors evaluated the results of CVTRANSturb by a bold comparison between 2 different variables (convective rain against medium transport into the upper troposphere by CVTRANSturb). This is not convincing. Others, I would generally have expected that an evaluation of CVTRANSturb already took place earlier in the results section.A comparison of convective rain rates would only make sense, if convective rain rates of EMAC were presented, but an evaluation of Tiedtke/Nordeng rain rates is probably not the topic of the paper.

We compare the results of CVTRANSnew (not CVTRANSturb) with the precipitation. We realised that it was not that obvious which version of CVTRANS was used in Sec. 3.2 from the manuscript. We tried to make that more clear by adding "(configuration CVTRANSnew)" in the first sentence in Sec. 3.2.
The comparison to the rain is done at this specific point in the script to show that the Tiedtke convection parameterisation shows deficiencies in some areas (e.g., the Himalayas) in general. This comparison can not be used to evaluate the convective transport scheme itself nor does the convective transport scheme influences the convective precipitation. (Keep in mind, that water tracers are not considered by CVTRANS because the convection parameterisation handles water itself.)
The convective newly formed precipitation in each level is directly proportional to the updraft mass flux. Therefore, an overestimation of the updraft mass flux can lead to an overestimation of the convective precipitation and of cause to an overestimation of the convectively driven upward transport. Convective transport and updraft mass fluxes cannot be compared on a global scale with observations. However, precipitation can and that was already done in the past (see Tost et al., 2006). A comparison between the precipitation over-/underestimations and convective transport is based on the fact that both depend strongly on the updraft mass flux and can therefore give an rough estimate how reasonable the convective transport patterns are in certain regions due to the performance of the underling convection scheme.
We wrote in the manuscript
"It can be assumed that the Tiedtke-Nordeng scheme does not perform sufficiently well in these areas in general, because the precipitation rates calculated with the Tiedtke-Nordeng convection parameterisation within EMAC are to high compared to observations in these areas, as shown by Tost et al. (2006) in their Fig. 2. As the freshly formed precipitation is proportional to the updraft mass flux, a too strong updraft mass flux results in both an overestimation of the convective precipitation and of the convective upward mass transport."
to make our point more clear.

Page 21, line 414: . . . to identify climate change in convective activity.
We added "in convective activity".

Page 22, line 439: Are sea surface temperatures and sea ice nudged or prescribed?
They are prescribed.

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