Responses

Adrienne Jeske and Holger Tost

May 26, 2025

Response to referee 1

We thank Wiebke Frey (referee 1) for the detailed comments and the comprehensive remarks. They are very helpful and allow us to improve our manuscript. In the following, we respond to all comments. The referees comments are *italic and blue* our answers are displayed in upright font and black.

General comments:

Section 2:
I suggest re-ordering this section to have the model and submodel described first and then the simulation setup as the last subsection.
We re-orderd the section as suggested. We added
"The submodel CONVECT by Tost et al. (2006) is applied with the Tiedtke-Nordeng scheme to calculate the parametrised convective properties needed in CVTRANS to calculate the convective exchange matrix." to make clear that CONVECT and CVTRANS were used.

In addition, I am surprised that the CVTRANS does not get mentioned in the simulation setup – but it is used? Additionally, the simulation setup describes the setup of the global simulations, but nothing is mentioned in the manuscript about the setup for the simulations in section 3.1, other then different versions of CVTRANS.
We describe the simulation setups used in 3.1 and 3.2:
"The submodel CONVECT by (Tost et al., 2006) is applied with the Tiedtke-Nordeng scheme to calculate the parametrised convective properties needed in CVTRANS to calculate the convective exchange matrix. Three simulations were performed with slightly different CVTRANS versions: (1) One with the old version of CVTRANS but including the Convective Exchange Matrix (CVTRANSold), (2) one with CVTRANS 3.0 and enhanced turbulent mixing (CVTRANSturb), and (3) one with the standard CVTRANS 3.0. (CVTRANSnew). (1) and (2) are only used to show the impact the turbulent mixing has on the transport by one deep convective event. Therefore, only a short simulation period is chosen. The simulation period of (3) spans the time from the beginning of 1979 until the end of 2020, because (3) is used to study the climate impact on the convective transport in this study."

Table 1: Three global simulations were performed with different setups to test the effect of turbulent mixing on the convective transport. The simulations only vary in the used version and configuration of CVTRANS as listed above and in the simulation time.

Name	Version	Turbulence adjustment	Used in Section
CVTRANSold	v2	no adjustment	3.1
CVTRANSturb	v3	strong turbulence	3.1
CVTRANSnew	v3	weak turbulence	3.1 & 3.2

CVTRANS and convective exchange matrix:
Do I understand correctly, that the exchange matrix is a tool that does not itself calculate the transport but uses the

transport as calculated by CVTRANS? This could be a bit clearer in the description.
Yes, that is correct. We added "The redistribution is calculated with the transport routine within the submodule CVTRANS." in the caption of Fig. 1. Further, we changed the text and wrote "The time integration, i.e., the vertical redistribution by convective transport of the pseudo tracer field, is performed based on the transport routine in CVTRANS compare Tost et al. (2010). The considered processes are shown in Fig. 1(b). The time integration results in the convective exchange matrix (**TrMa**).".

Regarding section 3.1, where you only compare the exchange matrices of different versions of CVTRANS: How can you know that the transport itself is simulated well? Thus, that all your results and conclusions of the following global study are meaningful? This is a crucial point, so please elaborate.
Tost et al. (2010) showed in their publication that CVTRANS can reproduced observed profiles of (non-soulable) tracers well. The convective exchange matrix itself does not affect the convective transport itself. It is only a tool for tracing and calculation. The changes we performed conserning the turbulence are only bug fixes. We avoid large changes in the simulation results by using CVTRANS 3.0 with relatively small turbulent mixing and not using the version $\text{CVTRANS}_{\text{turb}}$ in Section 3.2.
We added "This submodel can reproduce observed changes of the vertical profiles of non-soluble tracers due to convection sufficiently well (Tost et al., 2010)." to justify the use of CVTRANS and to be even more clear, we inclosed "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. 3.1.
In section 3.2 you – for the first time – mention CVTRANS 3.0. Is that the version CVTRANSnew (section 3.1) or CVTRANSturb, or yet a different version? Would probably be good to also mention version names in section 3.1 (also for CVTRANSold).
We added the Tabel 1 for clarity and described all simulation setups in the simulation setup section former Section 2.2 now Section 2.4.

Model levels:
As model levels are arbitrary, your reader (and I) might (or do) struggle when you indicate the analysed altitude level by model level numbers. Sometimes, you provide the respective altitude in hPa in brackets, sometimes you reverse the annotation the "hPa (model level #)", sometimes only hPa, sometimes only model level number (for example throughout the second half of page 7).
Thank you for pointing out that the notation using the model level is confusing. We agree. Sadly, we can not use pressure levels instead because transport artefacts would arise due to the interpolation; e.g., the vertical extend of the model levels in mountain areas is compressed in comparison to the levels in grid boxes over the ocean. If we want to interpolate from model levels to pressure levels and make use of the same pressure levels for all boxes (i.e., the vertical global mean pressure profiles) we would lose some matrix entries in mountain areas. Moreover, we would divide entries of the convective exchange matrix from one to two levels due to the interpolation from two model levels to the pressure level chosen in between. In the worst case, that could lead to the display of convective transport where no convection took place. For this reason, we decided to stick to model levels and only to refer to associated pressure levels.
You are right that we should stick to one notation. We are now using the roughly pressure levels in hPa or terms as upper troposphere / lower stratosphere and denote the model levels in brackets. Furthermore, we adapted Fig. 1 and the former Fig. S1 now Fig. 2. Now, these Figures contain information about the order of the model levels.

Please, 1) stay consistent and 2) provide a 'human readable' format, like hPa. If you really want to keep the model levels in the text, than please put these into brackets. Also, when you talk about higher/lower levels, it is sometimes not clear whether you mean higher/lower in model level world or in 'real' world. This needs more clarity!
We changed that accordingly.

On lines 221-225 you make a remark on the model levels vs hPa for the global simulations. Please, to also help your reader here, provide a 'human readable' altitude equivalent for the whole profile, such that your reader can identify

whether a certain model level might be rather located in e.g. the BL, lower free troposphere, upper troposphere, tropopause region, etc. It should suffice to at least say which model levels they "roughly correspond to". Also here, use the 'human readable' format in the description of your results.
Concerning our remark on lines 221 to 225 we added a table with reference values to the supplements:

Table 2: Reference values for the tropopause and the boundary layer height. In this table information, is provided which model and approximate pressure levels correspond to the simulated tropopause. Additionally, the approximate reference model level and heights in meters are shown for the boundary layer height. In this study, we relate the tropics to the area between 30°N and 30°S and the extra-tropics to the area between 30°N/S and 60°N/S. We do not claim that this table is exact nor includes all possible model levels, pressures and heights of the tropopause and the boundary layer. The given values should be considered as rough guidance only.

Reference	Model level tropics	Pressure tropics	Model level extra-tropics	Pressure extra-tropics
Tropopause	15 to 4	340 to 60 hPa (130 to 100 hPa)	19 to 6	410 to 110 hPa (390 to 170 hPa)
Reference	Model level tropics	Height tropics	Model level extra-tropics	Height extra-tropics
Boundary layer	31 to 22	0 to 3000 m	31 to 19	0 to 4400 m

At the same time, I am wondering, that e.g. in Figures 8, 9, and 10 you provide equivalent pressure levels that do not change between the figures, even though following your remark, I would have expected this?
The pressure levels in the in Figures 8, 9, and 10 (and 4, 5) refer to the a standard pressure of 1013.25 hPa in the lowest level. It is not a mean profile. To address this issue we added "The equivalent pressure is presented alongside the model levels to allow a rough estimation of the pressure. This equivalent vertical pressure is based on a vertical profile when the pressure in the box closest to the surface is 1013 hPa."

Grammar:
Throughout the manuscript, I had to re-read several sentences as the order of the words in the sentences was mixed up (wrong sentence structure), e.g. "mostly starts/starts mostly" or the placement of the verb. I believe, a lot of text editors have a grammar check, please make use of it! I won't correct these instances in the detailed comments.
This is absolutely right. We tried to improve the language and the grammar.

Detailed comments:

line 24: "... and the related scavenging"
Scavenging of what? (aerosols, tracers, ...)
We changed "... and the interplay of convective transport and the related scavenging ..." to "... and the interplay of convective transport and the related scavenging of tracers ..."

line 27: "... it is a hot topic in literature"
If this is a hot topic, then I would expect more references and their conclusions in the following. However, there are basically three studies mentioned, of which only one is from the recent years (Lepore et al., 2021). However, you mainly focus only on one study (Stevenson et al., 2005). I think, the discussion around the Stevenson paper could be condensed while the introduction would severely benefit from including more references and details about the conclusions of other recent studies looking at this 'hot topic'.

We appreciate your comment a lot and agree fully. Our formulation "hot topic" was most unfortunate. We used a different formulation, shortened the discussion about Stevenson et al. (2005) and included two more studies (a recent one and one a bit older, but it is supporting the point we want to make).

line 34: "Thereby" I think, this is not the right word, did you mean "They found that" or something along that line?
We removed this sentence from the manuscript to shorten the discussion about the study by Stevenson et al. (2005) (see your comment above).

line 41: "Therefore, this study" -¿ 'Therefore, our study'
We changed that.

line 42: typo 'specifically influence'
We corrected the typo.

line 42/43: check for missing commas (,). We added the commas and rewrote the question(s) because it was a bit complicated. Now: "How does climate change specifically influence the convective transport? How has the transport efficiency and extent of the updraft, the downdraft, and the balancing subsidence changed with time?"

line 68/69: shorten to "... utilising EMAC." (had been introduced in the subsection before)
We shortened the sentence and adapted it to plural form to take also the different simulation setups into account (compare major comments). We wrote "Three dimensional global simulations have been conducted with the convective exchange matrix implemented utilising EMAC."

line 69: "10 Pa" – Do you mean 10 hPa?
Yea, we meant 10 hPa and changed that in the manuscript. Thank you!

line 70: T63 – for readers not familiar with EMAC, please state what this resolution means in terms of spatial resolution (km).
We included additional information about the special resolution in km.

lines 80-83: These are rather statements for the acknowledgements.
We removed the sentence regarding the computational resources within the paper. We mention them in the acknowledgments.

line 86/87: Here, you state that the convection is parameterised based on Lawrence and Rasch (2005), while in the results, you often stress the Tiedtke-Nordeng convection parameterisation. Which is used?
The Tiedtke-Nordeng scheme is used within the submodel CONVECT to calculate the convection in EMAC. The bulk formulation as used by Lawrence and Rasch (2005) is utilised by Tost et al. (2010) to parameterise the convective transport (not the convection itself). The convection parametersiation (in our case) Tiedtke-Nordeng provides necessary quantities, i.e., the mass fluxes which are used in the convective transport parameterisation to calculate the redistribution of tracers / air masses. Beyond that, both are independent of each other. We agree that our formulation was unfortunate. We wrote "a single plume/bulk convective transport parameterisation" instead of "a single plume / bulk convection parameterisation".

line 120: "A formulation following..." Unspecific – what exact formulation? Or is there only one, then say "the formulation"
We changed "A formulation..." to "The formulation ...".

lines 122-124: "The adaptive time stepping by Ouwersloot et al. (2015) must be applied" & "Hence, we argue that adaptive time stepping should be applied " Double! Please remove redundancy.
We removed "Hence, we argue that adaptive time stepping should be applied in every simulation using CVTRANS.".

line 130: What is "C"?
The concentration of a tracer or air mass. We added "Thereby, C denotes the concentration of a tracer." after equation (4).

line 132: "First, in every vertical model level (1,2,...,N)" Please include the information about the model level ordering that follows in lines 143/144 here, to avoid confusion (e.g. otherwise the reader would think that in line 143 i should be smaller j not the other way around).
We added: "An important remark: By definition, the vertical model level with the highest number, level N, is closest to the surface."

line 146: "in the electronically supplements" -> remove 'electronically'
We removed the whole sentence "The illustration Fig. S1 in the electronically supplements can be used as guideline for the interpretation of the convective exchange matrix" because we decided to follow your advice to include Fig. S1 in the paper itself.

Figure 1: I would suggest to include model level numbers (1, 2, ..., N) in the Figure to help guide the reader. You mention downdraft and large-scale subsidence in the caption, I would suggest to also include these with arrows in the schematic in the middle of the figure for completeness.
We included the model levels in Fig. 1. Now, downdraft and subsidence are incorporated as well.

Section 3.1
I am missing information on the simulated case. Nothing is mentioned in the simulation setup section, neither is it here (the only mention is in line 167 "for one exemplary deep convective event"). Is the case that you simulate an idealised storm, or is it a real case?
We changed Section 2 and explain the simulation setups in this section. To be more specific we also change "We performed three simulations to demonstrate the effects of the described changes in the submodel CVTRANS." to "We performed three global simulations to demonstrate the effects of the described changes in the submodel CVTRANS (Table 1)." in Section 3.1.
For the comparison, we had chosen an arbitrary deep convective event which occurred in the simulations. In all three simulations, the same convective event is simulated because the changes in CVTRANS have no feedback on the meteorology. This is the case because no chemistry is considered. Every other deep convective event from the simulations could have been selected also to show the differences between the three configurations of CVTRANS qualitatively.
To make the reader aware, we decided to write in the caption of the former Fig. 2 "Convective exchange matrix for one snapshot of one specific event."
line 173: "... and subsides..." -> remove 'and'
We deleted "and".

line 174: "The large scale subsidence together with the downdraft balance the updraft." Are there no large scale upward motions in the model?
There are large scale upward motions. However, large scale processes and convection as well as convective transport are treated separately and independently as long as convection parameterisations are considered. Therefore, the large scale motion is not able to balance the convective induced movements. This has a rat tail but it is a general issue with coarse resolution model simulations. However, we cannot perform climate and especially not fully coupled chemistry-climate simulations at convection permitting scales within a reasonable amount of computing time at this point. Maybe that will become feasible in the future with increasing computer power.
We added in the paper that we refer large scale subsidence as the grid-scale subsidence and we added "Synoptic-scale processes are not considered." in the caption of Fig.1. We hope that makes it easier to understand for the reader.

line 175/176: "This is realised by allowing the subsidence to transport mass to the model level directly below the original level only "
Why does this make the subsidence slower?
Velocity is distance divided by time: If we now prescribe that a molecule is only allowed to subside one model level instead of multiple levels, we decrease the distance it can possibly cover within the same amount of time. Therefore, the effective velocity will be smaller.
Thank you for pointing out that this is not as clear for the reader on first sight. We included "by reducing the transport distance" in the paper to make it more obvious.

Figure 3: Comparing to Figure 2, it seems that in some places of the figure there should be a coloured shading, e.g. around origin level 16/destination level 26-31.Maybe adapt the colour scale?
We changed the colour scale of Fig. 3 (now Fig. 4).

line 232: "The convective transport based on the Tiedtke-Nordeng convection parameterisation indicates typical features. " Are these features typical for the Tiedtke-Nordeng parameterisation? How is it different for other

parameterisations? Please discuss/describe. If it is not specific for this parameterisation, the sentence could be removed because of redundancy. We removed the sentence.

line 233: "reaches not" -> 'does not reach'
We changed that.

line 239: a) "distinguishable increase" – increase compared to what? b) "lowest three model levels" Is that now level 1-3, or 31-29? See main comment on model levels.
a) We reformulated the sentence. Now: "For the lower to mid-tropospheric origin levels, the downdrafts transports air mass especially to the three levels closest to the surface."
b) We wrote "three levels closest to the surface" instead of "lowest three model levels".

lines 241-243: These sentences are somewhat contradictory: The upward transport to these levels has been strengthened, but at the same time these levels are only occasionally influenced by convection? Maybe better to discuss together with the matrices that separate tropics from subtropics?
As you supposed, we decided to remove the sentence here as we discuss the differences between tropics and extratropics later anyway.

line 259: "Thereby, overshooting is defined as events where the updraft mass flux reached beyond the tropopause in one column" Thereby is probably not the right word. Do you mean that the Tiedtke-Nordeng scheme defines overshooting like this? Is that definition consistent with the definition of overshooting in Wu et al. (2023), which you compare to in the following? As convective overshooting does not only happen at the tropopause, definitions might differ and thus, would need to be discussed here.
This is a good point. Thanks you for bringing it to our attention. We change the from "Thereby, overshooting is defined as events where the updraft mass flux reached 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."
We do not make use of the same definition of overshooting convections as Wu et al. (2023). Wu et al. (2023) use the term tropical overshooting deep convection without defining it specifically. They mainly look at deep convective events reaching approximately altitudes between 14.5 and 18.5 km. They sometimes show deep convection which at least reaches above 15.5 km (see their Fig. 1) and also show overshooting convection based on model results when the deep convective cloud top height penetrates higher as the tropical cold point tropopause height in their Fig. 3. We agree that our comparison to Wu et al. (2023) was not explicit and specific enough. Therefore, we decided to compare only the changes Wu et al. (2023) detected for tropical overshooting convection reaching beyond the cold point tropopause with our results.

line 273: "The upward transport has decreased..." Repeat what you are comparing.
We added "for the most recent time period in comparison to the 1980ies" at the end of the sentence.

line 275: "Less deep updraft convective mass fluxes..." Do you mean 'Fewer occurrences of ...'?
Yes, indeed. We reformulated and wrote "Deep updraft convective mass fluxes occurred less...".

Figure 6 (caption): "colors" in the caption of Fig. 5 you use 'colours', please check consistency of the usage of British vs American English throughout the manuscript.
We changed that. Thanks!

line 314: "A similar picture..." Similar to what? Actually, I have the impression that the whole sentence is not necessary and can be removed.
We deleted the statement.

line 319: " The downdrafts shift to higher origin levels. " Here, I am not sure what you refer to with 'higher levels' is it higher model level numbers (so lower levels in the atmosphere) or higher in the sense of altitude in the atmosphere? See also major comment on model levels; I would strongly suggest using a notation that goes from low levels at the ground to high levels at altitude.

We used the pressure notation with the model level information in brackets to be more clear and wrote "The downdrafts shift as well to starting levels located further up with regard to the surface,... "

line 329: remove 'although'
We removed "although".

line 329/330: "A shift of the downdrafts"
A shift from where to where?
We rewrote the whole section to increase the comprehensibility.

Figure 11/line 349: Do I understand correctly, that you identify transport from the boundary layer to the upper troposphere based on the transport that happens within one 12min time step? Is that not too short? I would think, that the BL to UT transport happens on time scales of about 30min (e.g. Thompson et al., 1997), so that would be 2-3 time steps in your simulation. Thus, you would underestimate the transport if only using one time step?
In general, yes the transport of one convective event would be underestimated. However, we aimed to show how the convective transport had changed in average over roughly the past 40 years. The climate signal is not underestimated using the model time step especially because our focus lies on the convective transport patterns. Fig. 11 cannot be used to give a direct estimate how much material is reaching the upper troposphere in general. To investigate this, the convective exchange matrix needs to be applied consecutively for the duration of each convective event. Depending on the specific question, the use of tracers might also be an option. However, this is beyond the scope of this study.

lines 357-360: Why are too high precipitation rates an indicator for overestimated convective transport?
We explained it further in the manuscript
"(2) 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."

line 449: "... convection reaches further..." -> reaches 'higher'
To be more specific we decided to use the term "penetrates higher".

lines 454-459: Here, you compare updraft speeds as studied by Del Genio et al and mass fluxes. These are not directly comparable, as for the calculation of the mass fluxes you need to take into account the updraft width/area (or area fraction). Did you check whether this also changed?
The convective mass fluxes in EMAC with resolution T63 are given with regard to the whole model box. A convective fraction is not considered. Therefore, changes in the updraft mass flux can be directly linked to the updraft velocity as long as the grid box size is taken into account.
We also analysed whether the areas of convective activity changed substantially over time; however, this is not the case, such that the strengthening of the updraft mass flux is directly proportional to an increase in vertical velocity.

line 462: "high altitude levels" -¿ Would be good to stick to the UT/LS notation to make clear what exactly you identify as high altitude here.
We decided to shorten this paragraph. Therefore, the phrase you are referring to was removed.

line 466: "... at lower temperatures as the colder point tropopause..." Did you mean 'at the cold point tropopause'?
Yes, we mean 'at the cold point tropopause' and changed that in the manuscript.

line 470: typo CVTRANS
Thank you! We corrected the typo.

line 470/471: I suggest to swap the two sentence parts (before/after the comma) to start the conclusions more general.

We wrote "We investigated the changes in convective transport due to climate change applying the submodel CVTRANS which ..."

line 471: "Thus"
I don't think, thus is the right word here – do you want to say that you take CVTRANS a step further (further to where?) because you apply it?
We changed the formulation to "We made the following adjustments to CVTRANS: (1) established consistency with the underlying convection scheme (Tiedtke-Nordeng) concerning the turbulent detrainment and entrainment."

line 473/474: "(2) In addition, the entrained air into the downdraft can be directly detrain in the same level of the downdraft comparable to the proceed concerning the updraft."
I don't understand what you want to say here, please rephrase.
We reformulated and wrote "(2) In addition, the entrained air into the downdraft can be directly detrain at the same level. This is comparable to the procedure for the updraft that was already included in CVTRANS before."

line 475: "more realistic handling of the turbulent mixing"
We wrote instead "more consistet handling of the turbulent mixing".

I am not a 10% convinced here. At the end of section 3.1 you even state that "the transport to the upper troposphere by CVTRANSturb is not efficient enough". How do infer, that your setup results in a more realistic handling of turbulent mixing? In fact, I only see the differences between the CVTRANS versions described (in section 3.1), but no discussion about which version provides the most realistic treatment anywhere in the manuscript. That relates to my comment on CVTRANS version, which version do you refer to in the conclusions, which version do you use in the global simulations, which versions are CVTRANSold/CVTRANSnew/CVTRANSturb?
We incorporated in Sec. 3.1:
"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 as the ones between CVTRANSturb and CVTRANSold."
and include the following statement in the conclusions:
"We used the old version of CVTRANS (CVTRANSold) which performs reasonable 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, but this version inhibits the transport to the upper troposphere to a great extend. Short-cutting the convective transport to such an extend 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."
To make the point more obvious, we changed panel (b) of Fig. 3. It shows now the CVTRANSturb-CVTRANSold and not CVTRANSnwe-CVTRANSturb.

line 476: "redistribution OF air masses"
We inserted "of".

line 478: "... disentangled from the other processes."
What are 'the other processes'? If you write 'the', you have to be specific.
We removed "the".

line 483/484: Remove "Respectively"
Done.

line 484: replace model level notation. A lot of people read the conclusions before the main manuscript, thus, they

will not understand which altitude you are looking at.
We removed the model levels in the conclusions and used reference terms, e.g., free troposphere.

line 486: "smaller mean transport" -> 'weaker mean transport'
We changed this as suggested.

line 489: " first insights in " -> insights into
We adapted the formulation.

line 490: "becomes emerging" -> emerges
line 490: "climate change open the path for higher but in total less upward transport" -> climate change opens the path for stronger but in total weaker upward transport
These formulations are no longer included in the manuscript.

line 491: "Furthermore, this study..."
Do you really mean 'this study', or 'the convective exchange matrix'?
We mean the convective exchange matrix. However, we removed this sentence because the manuscript is quite long.

Supplement:
Figure S1: I think this figure is extremely helpful and should thus be included into the main manuscript, including a good description of the figure in the main text.
I think, that "Very small to zero transport" in the figure is already an interpretation of simulations you run, while it should rather describe the potential process – which would be (in the style as you describe the updrafts) "extreme downdrafts". Also, what about upper level downdrafts?
We included Fig. S1 (now Fig. 2) and a description of the figure in the paper. We rewrote "Very small to zero transport". Now, the statement "Extremely strong downward motion" is included.
We added a second panel to the former Fig. S1 to make the difference between the tropics and mid latitudes more obvious for the reader.

Response to referee 2

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.

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.

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 $\sim$90%). Therefore, the convectively driven large-scale subsidence has a huge influence $\simeq$ 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.

References

H. Hersbach, B. Bell, P. Berrisford, S. Hirahara, A. Horányi, J. Muñoz-Sabater, J. Nicolas, C. Peubey, R. Radu, D. Schepers, A. Simmons, C. Soci, S. Abdalla, X. Abellan, G. Balsamo, P. Bechtold, G. Biavati, J. Bidlot, M. Bonavita, G. De Chiara, P. Dahlgren, D. Dee, M. Diamantakis, R. Dragani, J. Flemming, R. Forbes, M. Fuentes, A. Geer, L. Haimberger, S. Healy, R. J. Hogan, E. Hólm, M. Janisková, S. Keeley, P. Laloyaux, P. Lopez, C. Lupu, G. Radnoti, P. de Rosnay, I. Rozum, F. Vamborg, S. Villaume, and J.-N. Thépaut. The era5 global reanalysis. *Quarterly Journal of the Royal Meteorological Society*, 146(730):1999–2049, 2020. doi: 10.1002/qj.3803. URL `https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.3803`.

M. G. Lawrence and P. J. Rasch. Tracer transport in deep convective updrafts: Plume ensemble versus bulk formulations. *Journal of the Atmospheric Sciences*, 62(8):2880 – 2894, 2005. doi: 10.1175/JAS3505.1. URL `https://journals.ametsoc.org/view/journals/atsc/62/8/jas3505.1.xml`.

L. Meng, J. Liu, D. W. Tarasick, W. J. Randel, A. K. Steiner, H. Wilhelmsen, L. Wang, and L. Haimberger. Continuous rise of the tropopause in the northern hemisphere over 1980–2020. *Science Advances*, 7(45):eabi8065, 2021. doi: 10.1126/sciadv.abi8065. URL `https://www.science.org/doi/abs/10.1126/sciadv.abi8065`.

T.-E. Nordeng. Extended versions of the convective parametrization scheme at ecmwf and their impact on the mean and transient activity of the model in the tropics. *ECMWF Technical Memoranda*, (206):1–41, 09/1994 1994. doi: 10.21957/e34xwhysw. URL `https://www.ecmwf.int/node/11393`.

A. J. Simmons, A. Untch, C. Jakob, P. Kållberg, and P. Undén. Stratospheric water vapour and tropical tropopause temperatures in ecmwf analyses and multi-year simulations. *Quarterly Journal of the Royal Meteorological Society*, 125(553):353–386, 1999. doi: https://doi.org/10.1002/qj.49712555318. URL `https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/qj.49712555318`.

D. Stevenson, R. Doherty, M. Sanderson, C. Johnson, B. Collins, and D. Derwent. Impacts of climate change and variability on tropospheric ozone and its precursors. *Faraday Discuss.*, 130:41–57, 2005. doi: 10.1039/B417412G. URL `http://dx.doi.org/10.1039/B417412G`.

M. Tiedtke. A comprehensive mass flux scheme for cumulus parameterization in large-scale models. *Monthly Weather Review*, 117(8):1779 – 1800, 1989. doi: 10.1175/1520-0493(1989)117⟨1779:ACMFSF⟩2.0.CO;2. URL `https://journals.ametsoc.org/view/journals/mwre/117/8/1520-0493_1989_117_1779_acmfsf_2_0_co_2.xml`.

H. Tost, P. Jöckel, and J. Lelieveld. Influence of different convection parameterisations in a gcm. *Atmospheric Chemistry and Physics*, 6(12):5475–5493, 2006. doi: 10.5194/acp-6-5475-2006. URL `https://acp.copernicus.org/articles/6/5475/2006/`.

H. Tost, M. G. Lawrence, C. Brühl, P. Jöckel, and The GABRIEL Team and The SCOUT-O3-DARWIN/ACTIVE Team. Uncertainties in atmospheric chemistry modelling due to convection parameterisations and subsequent scavenging. *Atmospheric Chemistry and Physics*, 10(4):1931–1951, 2010. doi: 10.5194/acp-10-1931-2010. URL `https://acp.copernicus.org/articles/10/1931/2010/`.

X. Wu, Q. Fu, and C. Kodama. Response of tropical overshooting deep convection to global warming based on global cloud-resolving model simulations. *Geophysical Research Letters*, 50(14):e2023GL104210, 2023. doi: 10.1029/2023GL104210. URL https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2023GL104210. e2023GL104210 2023GL104210.