Deep convective influence on the UTLS composition in the Asian Monsoon Anticyclone region: 2017 StratoClim campaign results

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Answer to reviewer 2

We thank the reviewer for the insightful comments. We will answer point to point to them in the following:

Scientifically, the manuscript does not adequately define the objectives of the study in the scope of the outstanding scientific questions. As a result, it is not clear what the key conclusions are. The only clear conclusion is related to the better performance of ERA-5 and diabatic vertical wind as opposed to ERA-Interim and kinematic wind. This conclusion does not support the title of the article.

We reformulated the objective of the paper in the abstract and the introduction. Indeed, the manuscript is intended to give the first in situ measurement-supported analysis of the convective origin for the air masses close to the tropical tropopause in the Asian monsoon region. As the StratoClim airborne campaign is the first ever conducted in this area, this is by itself an important result to report. Among the outstanding results there is the evidence of fresh pollution injection directly below the tropopause, the detection of the intense overshoots at the level of the tropopause, as well as the presence of typhoon injected air. The strength of this paper is based on the combination of a modelling approach that is tuned, and at the same time supported, by the in-situ observations. It is the first time that such comparison can be made in the AMA.

The lack of a clear objective is also reflected in the description of the depth of convective "injection" and the "age" of air in the samples. First of all, no clear definition is given to define "age". We assume it is defined by the length of the back-trajectory from flight track to convective top encounter, but what is the physical significance of this quantity?

We also better present those two quantities. The depth of convective injection is given by the cloud top pressure, that we suppose to be close to main level of convective detrainment. This is an approximation that takes in account that the cloud top identified by the SAF algorithm, which is that of main radiative emissivity, is not the optical highest point of the cloud**.** The age instead is indeed the time between the detrainement from the cloud (assumed to be instantaneous and coinciding with the first encounter of the clouds by the trajectories) and the moment of the observation (corresponding to the trajectories release), representing the life time of air in the UTLS which is highly relevant for chemical processes. Physically we can assume that this time is pretty close to the real time of transport from the boundary layer, since what is missing is the estimate of the time of vertical convective transport from the boundary layer to the cloud top. As the sustained vertical velocity of the updrafts inside convective towers is typically of the order of 10 m/s, the time span of upflit from the boundary layer is of few hours at most. We can therefore ignore this delay. The analysed vertical velocities used in many studies are much weaker but they are only representative of the mean motion over cells much wider than the convective updrafts.

Revise the introduction to clearly state the objectives. If identifying the chemical characteristics of convectively transported air from different regions is the ultimate goal of the data analysis, but the composition analysis is beyond this work alone, it still needs to clearly articulated. The regions used for "airmass origin apportionment" should also be defined with different chemical emission characteristics or convection behaviors in mind

Following the reviewer suggestion, we fixed the introduction to clarify this point. The reviewer raises the importance of the choice of the airmass origin apportionment. Indeed, for this work we looked for a good definition of the convective / most emissive regions. Nevertheless, the study has as a first broader objective to present a tool for the convective analysis in the UTLS that could be applied to different atmospheric components which path of injection is not always relatable to the CO emissions. Similarly, to define the regions based only on the convective occurrence, would be to limit the selection or to the campaign-related main source or to the more general convective activity frequency (that is differently distributed). Since the study wants to be an opener for more comprehensive analysis of seasonal convective injection while still relying on the campaign results, we decided to keep a more neutral division of the regions mainly based on the country borders. Country borders anyway are related to geographical natural borders (mountains, rivers, land-ocean separation) as well as being related to differences in economy and policies of anthropogenic emissions. The inclusion of the Tibetan Plateau region and the separation of the South China region from the North one has been intended indeed to take into account that those two regions are peculiar under the point of view of frequency of occurrence and/or in pollutants emissions. A more detailed identification of the sources can still be identified in the source distribution plots (as in panel a) of figures 7,9,S1,S2,S3,S4,S5,S6).

An implicit goal of the work is to investigate how often and how deep the convective transport is influencing the "UTLS" composition. Separating the UT from the LS is important. If the tropopause identification is not supported by the flights themselves, an estimate using ERA5 data could still be very helpful to quantify and characterize the height of convective "injection" relative to the tropopause. With the help of the tropopause location, quantifying the direct influence of convection relative to the tropopause, the relative contribution from the regions in the UT, and when and where convection influenced the stratosphere can be a significant conclusion of this work.

As the review points out, the intention of the paper is to emphasize the results on the influence of deep convective transport onto the UTLS composition, so we decided to reinforce this concept in the introduction._ The tropopause has been identified as a matter of fact by the flight measurements when extensive profiles were available, otherwise this is identified by ERA5 indeed, as shown in figures 6 and 8 panels a. ERA5 was recently demonstrated to have the best tropopause height estimate among the reanalysis (Tegtmeier et al. 2020). We therefore emphasized this better in the text. Furthermore, it is demonstrated in a companion paper (Legras and Bucci, ACPD, 2019) that the tropical tropopause is not associated to any discontinuity in the transport properties at the scale of the AMA when they are seen in the potential temperature framework. Therefore, if it remains important to localize the convection tops with respect to the tropopause, the transition from UT to LS is actually very smooth regarding transport properties.

From the presentation point of view, the manuscript suffers from a deficiency of too many details and lack of clear take home message. Although there are many details highlighting convective influence from different boundary layers (such as Northern India and the Tibetan plateau), there is no clear message why transport from these regions is important. For the two selected flights described segment by segment, the writing style is similar to that of a detailed flight report. Convective origin, or a sample's "airmass source apportionment", is a big focus of the analysis, but no significant chemical consequences are shown from the analysis or articulated in the introduction. After all these details, it is not clear what the significant findings *are or what is scientifically new. We suggest that the discussion and analysis be reorganized around new findings.*

As already stated, we are assessing the transport properties in the AMA based on the first ever high altitude airborne campaign in the region. This is by itself very new. We worked on the text to re-focus the conclusions on the main objective: the identification and quantification of the percentage and age of tropospheric air injected by deep convection into the UTLS, supported by in-situ observations. Moreover we clarify that giving a description of the chemical consequences is out of the scope of the paper, that is meant to focus on the purely dynamical aspect of the transport (using CO not for chemical study purposes but as a tropospheric transport tracer since it is an indicator of anthropogenic influenced air). On the other hand, this study is also meant to be a reference for transport characterization for the other StratoClim papers, that will be indeed aiming to describe more in details the chemical aspects. For those reasons we strengthened the objectives description while preserving the needed detailed segment by segment analysis as a reference for the future papers.

A number of sections are written as one paragraph. It seems largely due to the style of "flight logging" used throughout. This poses a challenge for the readers. We suggest that the authors highlight the main goal of each discussion, select significant details, and break the sections into a number of paragraphs according to the take-home messages.

We took this suggestion in account in the review of the main text, we therefore added the highlights of the main results of the flight analysis and restructured some of the paragraphs.

All the key information would need to be in the paper, not the supplement. For example, it is important to show the flight tracks relative to the flow pattern of the anticyclone

Since we want to preserve the details on the convective influence and the tropopause variability shown in the closer look of figures 6 and 8, but without expanding the main paper with further figures, we would still prefer to keep the wider circulation on the supplement. We agree nevertheless that it would be useful to have a view of the flight position with respect to the anticyclonic circulation and decided therefore to add the flight tracks to the panels of figure S7. Notice also that such general figures will be found in a forthcoming overview paper that will cap the series of papers in the StratoClim special issue of ACP.

It is also a good practice to make the figures, including the titles and axis labels, large enough to read in the printed version. There are a number of issues with this, including Figures 3, 6 and 8.

Following the reviewer advice, we enlarged the fonts of the figures.

The large number of regions defined in Figure 3 should be re-considered since the authors seem to have run out of colors to represent all the regions distinctly. For example, in the later figures, Tibetan plateau, MPac, Bangladesh and Pakistan are not always separable, especially in the print version.

To take into account the reviewer remark we remove the Japanese-Korean and the North-China regions, that are not significant convective contributors with respect to the other regions, and changed the colors of MPac and Bangladesh and Tibetan Plateau.

Specific Comments

We suggest that the authors address the issue of CO being the only chemical convectively influenced composition variable shown in the manuscript, since the paper is about the convective influence on the UTLS composition. We note that the use of reconstructed CO to diagnose the trajectory based convective transport identification is a nice piece of analysis in this work. CO alone, however, doesn't represent the objective of the campaign. It would be good to state the limited objective of using CO in this analysis and *the goal of the analysis is to support the full scope of chemical composition analysis, in particular the short lived active species, etc.*

The comment of the reviewer raised the need to clarify in the text the choice of the use of CO and that we focus on the transport properties rather than on the chemistry. As mentioned above, the CO mixing ratio is used here as an indicator for the tropospheric air presence, as it is a good tracer for anthropogenic pollution, with a lifetime in the atmosphere of around 2 months, compatible with our trajectories time. Therefore the scope is not to have a full chemical composition analysis but to give a reliable deep convective transport description backed up by the observations. This has been clarified in the text.

A statement about the magnitude of uncertainty in satellite-derived convective cloud tops would be beneficial since the results hinge very strongly on these being accurate.

Following the advices of both reviewers, we included a statement on the sensitivity study we performed with different cloud top altitudes corrections. See also the answer to the first reviewer.

If the assumption is made that once a parcel encounters a convective cloud top it is considered to simultaneously contact the boundary layer, it is an important assumption to be explicitly stated and justified.

When the parcel encounters a convective cloud top is considered to have been instantaneously detrained from there, while the time from the departure from the boundary layer can be estimated to be of the order of few hours but is not taken into account in the computations. This is now stated in the text, see also previous answers. One of the main point of this work is indeed that we do not use the vertical winds from the reanalysis for the convective transport as they are not representative of convective transport and result in unrealistic time of transport from the boundary layers (weeks instead of hours).

P11 L12-14: It is an inaccurate statement of "stratospheric intrusion" based on the observed CO-O3 structure without a tropopause analysis. It is also possible the flight sampled a filament of stratospheric air produced by the large scale stirring.

We simply mean indeed that it is a sample of stratospheric air, we correct this in the paper.

P4 L23-25: "The trajectories move . . ." needs to be revised. This sentence has no clear meaning. Do the authors intend to say "Only the trajectories moving within the domain 10-160 E and 0-50N are considered" in the analysis?

We intend to say that the trajectories are bounded to the limited domain of the meteorological fields (cutted to 10-160E and 0-50N for ERA5 for computational reasons), therefore they cannot be transported outside those boundaries since there would be no wind fields there. In this case the trajectories are considered there to be "dead". This fraction of "dead" parcels corresponds to the white space in the contributors' percentage plot. We clarify this point in the text.

There is an inconsistency between Sections 1 and 2 about when StratoClim ended (beginning or middle of August). We recommend standardizing this

We corrected this, the campaign ended on 10th of August so we decided to stick with "middle of August"

P4 L6: There are several places where the authors are not consistent with acronym usage (e.g. "COLD2" vs "COLD" and "MSG1" vs "MSG"). Make sure to stay consistent with these

We thank the reviewer for mentioning this, we fixed it in the text.

P9 L6: Panel b of Figure 6 is never introduced in the text, so its importance is unclear.

The image is meant to show the convective situation in the vicinity of the flight. Is possible to see there how the CO enhancement in the flight are not due to transport from close convective systems upwind, as well as showing that there is no overpass of the flight on convective systems (as instead happens in fight 8). We now mention it in the manuscript.

Figure 10: What is the pink region in the histogram of flight 1? That color is not in the legend.

It was the Japanese region, now removed according to the previous comments. We therefore updated the figure.

Figure 11: It is unclear why the "mean CO" black boxes represent a range. Is this supposed to be the area between the 5 and 95 percentiles? If so, a different name for this quantity should be chosen

The use of bars in the figure may indeed be misleading, since the values represented are not ranges but simple means of the CO anomaly (black), 5 percentile (light grey) and 95 percentile (dark grey). We therefore substituted it with a line plot.

Figure S1: Make sure to be clear in the caption that panel a is on a log scale.

Correct, we fixed the caption.

Typos: P2 L 32: Remove "for the" P4 L6: "Relative" P4 L18: "allows us to" P5 L29: "of" P6 L1: "Diabatic" in the section title P6 L24: "A higher amount of convective. . ." P10 L21: "system which developed" P12 L5: "precipitation" Figure 2: BoB is missing from the caption. Figure 7: For the description of panel e, say "below the convective cloud top." Figure 9: The caption should say that ozone is also plotted in panel c, not panel d. Figure S7: "campaign" and "27th." Table ST1: "ensemble."

We corrected the typos, we thank the reviewer for identifying them.

References:

Legras, B. and Bucci, S.: Confinement of air in the Asian monsoon anticyclone and pathways of convective air to the stratosphere during summer season, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-1075, revised sub judice, 2019.

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