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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Deep convective influence on the UTLS composition in the Asian Monsoon Anticyclone region: 2017 StratoClim campaign results" *by* Silvia Bucci et al.

Anonymous Referee #2

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Summary: This work undertakes a comprehensive analysis of the influence of convective transport on the UTLS airmass in the Asian monsoon region using trajectory modeling and in situ measurements during a Southern Asia aircraft campaign. The authors examine the effectiveness of kinematic and diabatic vertical velocity in two different reanalysis by comparing reconstructed CO from a trajectory model to in-situ aircraft observations of CO. The results show that the ERA5 product produces a more realistic representation of the observed CO compared to the ERA-Interim product. The diabatic vertical velocity option is also found to perform slightly better compared to kinematic vertical velocity. The authors break Asia into several regions to investigate the regional origin of air masses sampled during the aircraft campaign with the aid of

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Discussion paper

satellite observations of convective clouds. Two flights during the mission are examined in detail to determine the convective origin of the air masses as well as their age and altitude of cloud interaction. Statistics of convective origin for all eight flights of the campaign are also presented, organized by both flight day and measurement height.

General comments: Although the manuscript reports significant research results that are important for interpreting the StratoClim campaign data, and has the potential of becoming part of the work contributing to new scientific insights on the role of Asian Summer Monsoon in atmospheric chemistry and climate relevant processes, the current version has significant deficiencies. Scientifically, the manuscript needs to be revised to relate the study to outstanding scientific questions and to speak to scientific researchers beyond the StratoClim team. Presentation wise, the manuscript needs to be revised to have a structure that allows the readers to get the take home messages. For these reasons, we do not consider the current version of the manuscript as meeting the standard of publication quality. The revisions we recommend are detailed below.

Major comments:

1) 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.

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?

To resolve these issues, we have the following specific suggestions:

a. Revise the introduction to clearly state the objectives. If identifying the chemi-

cal 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.

b. 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.

2) 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.

3) 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.

4) We also suggest that the authors improve the figures. Specifically:

a. 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.

b. 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.

c. 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.

Specific comments:

1) 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 the goal of the analysis is to support the full scope of chemical composition analysis, in particular the short lived active species, etc.

2) 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.

3) 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.

4) 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.

5) 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?

Technical comments:

There is an inconsistency between Sections 1 and 2 about when StratoClim ended (beginning or middle of August). We recommend standardizing this. 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. P9 L6: Panel b of Figure 6 is never introduced in the text, so its importance is unclear. Figure 10: What is the pink region in the histogram of flight 1? That color is not in the legend. 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. Figure S1: Make sure to be clear in the caption that panel a is on a log scale.

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.”

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