

Interactive comment on “Contribution of air-mass transport via the South Asia High to the deep stratosphere in summer” by Yu Liu

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General points:

The manuscript presents a method for estimating meridional and vertical air-mass transport from the Asian Summer Monsoon (ASM) into the stratosphere using hydrogen cyanide (HCN) from the ACE-FTS satellite observations. This proposed alternative method to model simulations is interesting and promising, but there are several issues, which need to be clarified. These issues concern the uncertainty in the method hypothesis; uncertainty in the application of the method to ASM (sensitivity to the averaging area; lack of rigorous analysis, the sensitivity of the method to the used HCN trace gas), the uncertainty in the results (error in the observations, coarse latitudes,

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and longitudes resolutions), the lack of recent research update regarding the transport pathways and their contributions (a recent article in the SPARC StratoClim project). In addition, the motivation of the paper as claimed by the author that “there are some uncertainties in the model analysis methods due to error in vertical velocity estimates” is not convincing because the numerical approximation uncertainty in the calculation of the vertical and meridional velocities is the same as the numerical uncertainty in this method (e.g. equation 5 for instance) and therefore is very likely in the same order of magnitude in this method. The main uncertainties in the model-based analysis of the SAM pathway contributions are related to the differences in the reanalysis data, the different levels used to initialize the air parcels, and to the different levels at which the estimated contribution of the vertical and horizontal transport of monsoon air is made, therefore, rendering it difficult for inter-comparison values in different studies. Finally, most of the sections and paragraphs in the manuscript are poor, therefore, they need to be revised by enhancing the discussion about the scientific content and the results presentations, and improving the quality of the paper by discussing the appropriate and recent studies where it needed. Based on the amount of work, which needs to be done to improve the manuscript, I recommend very major revisions. It’s of particular importance to clarify these major points. In the following here are my major points and specific concerns:

Major points:

1. Regarding the uncertainty in the method hypothesis of neglecting the time tendency term needs to be estimated and its contribution quantified. The uncertainty related to neglecting the production and depletion term of the HCN can be addressed using a second trace gas such SF₆, CO, or CO₂ or others ACF-TFS or MLS observations (or SWOOSH) to evaluate the sensitivity of the vertical and meridional contributions. This will shed the light on the sensitivity to the used HCN trace gas.
2. One thing very curious is that fraction term G is not closed. Normally, G, as well as F, should be smaller or equal to (or 100%) but G does not hold that distribution

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hypothesis.

3. Concerning the uncertainty in the application of the method to ASM area, including the sensitivity to the averaging area; lack of rigorous analysis, sensitivity to the HCN trace gas, the author needs to first to estimate the ASM area at different levels using the PV contours; lapse rate tropopause (LRT) height; LRT potential temperature or cold point tropopause (CPT) height to identify the monsoon area, which contained the ASM air before averaging. In the area where there is not a clearly defined contour, one can tag the concentration of HCN mixing ratios which are above background levels. The author averaging areas of ASM air (20-40N or 10-40N) are very arbitrary chosen therefore induced uncertainties in the estimated values.

4. Other sources of uncertainty in the results concern the error in the observations, particularly the coarse latitudes and longitudes resolutions of the observations. The 5° (latitude) $\times 10^\circ$ (longitude) grid can lead to difficulties of defining the ASM area.

5. Most of the paragraphs are poorly written, appropriate references are not properly used at some places, and some sentences are vague (not specific). The updated research publications regarding the transport pathways and their contributions in the SPARC StratoClim project should be addressed. Please find a list of publications below. Minor points:

1. Page 1, line 1, the title can be changed to “Contribution of air-mass transport via the Asian Summer Monsoon anticyclone into the stratosphere” as the ASM is more commonly used and “deep” is more lower and middle stratosphere than “deep”.

2. Page 3, lines 82-83, this paragraph is misleading because the region located above the TTL is not “deep stratosphere” but just stratosphere, which divided into the lower, mid and upper stratosphere. Please rephrase it.

3. Page 4 line 112, this sentence is not correct because please see my general comments.

4. Page 4, line 119, satellite observations have also some uncertainties in the UTLS. The question is how large the uncertainty claimed here in the vertical velocity between different reanalyses compares to the uncertainty in the satellite observations in this region? The differences between kinematic & diabatic are not a result of the calculation but rather a reanalysis of intrinsic issues. In addition, reanalysis-driven simulations compared very well to the in situ observations. Please references below.
5. Page 6 line 177, please replace “ratio” and “portion” with “fraction”.
6. Page 6-7, section “Method & Model”, please keep the notation of the equation consistent. For instance, we have upper-case and lower-case of the “c”.
7. Page 7, line 194-201, the description “G” is not necessary as it’s the same as F but in the Asm area.
8. Page 7, line 205-211, this description of the data is very poor. No vertical resolution, uncertainty in the observations, period of the observations, previous studies using the observations are missing. In addition, the ERA-interim tropopause and wind should be both described here instead of all over the place in the manuscript.
9. Page 8, line 231-233, what is the vertical resolution of HCN observations?
10. Page 8, line 239, How these numbers compare with the previous literature?
11. Page 9, line 244-246, is there any explanation of the quasi-equal contribution of the vertical and meridional ASM air?
12. Page 9, line 248-250, is there any explanation of why are there so many differences in the HCN and air mass reported values?
13. Figure 1 caption, please add the period of the observations and make sure that the figure is enough described in the caption. Same argument for figure 2.
14. Page 10, line 280, the selection of averaged area is very arbitrary. Please see my suggestions in the general comments.

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15. Page 10, line 291, the values of G look weird as G is a distribution, it should be smaller than 100. This suggests that the method might fail.

16. Page 10, line 299-300, the ASM is an upward motion plus horizontal transport at the outflow level not only a vertical motion. Please rephrase it.

17. Page 15, line 420-424, all this paragraph, in particular the ASM contribution 2 times greater than the tropics need to be proved after addressing the major points.

References:

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