Review of *Atmospheric Chemistry and Physics* manuscript 10.5194/acp-2022-143 by Nützel et al.: *Variability of air mass transport from the boundary layer to the Asian monsoon anticyclone*

General comments

1. Line 109: For trajectory calculations involving deep convection, both the space and time resolution of the wind fields are important. The 6-hour time resolution, in particular, and the 1.5° horizontal resolution of the ERA-Interim data are both rather problematic for calculating 'convective' transport. Equally significant is the hydrostatic nature of the underlying atmospheric model. While the total vertical mass flux due to convection may be roughly correct, the fact that the reanalysis system is based on a global hydrostatic model means that the vertical velocities are too small, probably by an order of magnitude or more, and occur over too large an area.

   The ERA5 reanalysis, which has been available for several years, has higher spatial and, more importantly, temporal resolution. (The authors note related issues in §5.2.). I recommend doing a test calculation (e.g., one season) to compare ERA5 trajectories with the ERA-Interim trajectories. If the results are similar, it would not be necessary to re-run all of the trajectories and the analysis. If not, the calculations should be re-done using the newer ERA5 reanalysis.

2. §2.2: Were the EMAC trajectory calculations done 'online', that is, with a time step equal to the model time step? What is the model time step? Why were the EMAC data output at 10 h intervals? That is an odd choice and could cause some unusual aliasing of the diurnal cycle.

3. Figures 3 and 12: I do not understand why the crossing maps at lower altitudes (e.g., 400 hPa and $\eta = 0.85$) bear so little resemblance to the distribution of monsoon precipitation, which is directly related to vertical motion and diabatic heating. The heaviest precipitation, which is strongly correlated with the occurrence of deep convection, is located along the west coast of India, the east coast of the Bay of Bengal, the northern Philippine Islands, and the Himalayan front. None of these features, except possibly the Bay of Bengal, show up in the transport from the PBL. The patterns of upward transport also differ from the GPM radar echo-top climatology (Liu and Liu, JGR, 2016). Have you compared the precipitation distributions in ERA-Interim and the EMAC model simulations with observations (e.g., TRMM TMPA)?

   At higher levels the ascent is presumably due to radiative rather than latent heating, so the difference from the precipitation distribution is easier to explain.

4. §3.1.2: By 'boundary layer source regions' do you mean the regions where the trajectories ascend out of the PBL (in the forward direction)? Air can spend a long time in the boundary layer and move from one region to another within the boundary layer before being entrained in a convective updraft and lofted out of the boundary layer.

5. §4 and Figure 18: The model results show much larger contributions from the IND and SEA regions and less from the TP, which corresponds better to the observed precipitation distribution.

6. The text is rather verbose and repetitive, and as a result the paper is longer than it needs to be. This can be corrected by thorough editing.

Minor comments

1. Title: The paper does address variability of transport to some extent, but the main focus is on the mean transport.
2. Line 54: How is ascent 'driven by the large-scale anticyclonic circulation'? Ascent in an isentropic sense must be driven by diabatic heating, which at these altitudes must be due primarily to net radiative heating.
3. Line 86: The sentence beginning 'Results from this model ...' is not clearly written.
4. Line 91: This paragraph is unnecessary and can be deleted.
5. Figure 5: Please add a pressure scale to the plots.
6. Figures 10 and 11: Can you combine these two figures into one (for easier comparison) or simply eliminate Figure 10? There is little difference between them.
7. Figure 15: Since you are plotting the relative contributions from different regions, the figure might be easier to follow if you plot the cumulative amounts across the regions (i.e., a stacked plot).
8. Figure 16: This figure does not add much information to what has already been presented in Figures 10, 11, and 15. I suggest removing it, or at least combining it with Figures 10 and 11.
9. Figure 17: It is difficult to flip back and forth between Figures 10 and 17 in order to compare them. These plots really belong in the same figure.
10. Figures 18 and 19: As with the box and whisker plots, it is difficult to compare these results with Figure 15. These should all be in one figure.
11. §6: This section is longer than necessary. A short statement of the principal results would be sufficient.
12. Appendix A: This appendix adds little information to what is already presented in §2.2.

Recommendation

This paper presents an analysis of vertical transport to the upper troposphere and lower stratosphere within the Asian summer monsoon circulation. The manuscript is rather long considering that the results largely confirm earlier studies (e.g., Garny and Randel; Bergman; and Vogel) while adding some new details.

The two main issues that I see with the manuscript are:

1. The ERA-Interim reanalysis has been succeeded by the ERA5 reanalysis. ERA5 offers improved spatial and temporal results, which could affect the trajectory calculations enough to change the results. The authors should compare trajectories from ERA-Interim and ERA5 to ensure that their results would not be affected significantly by switching to ERA5.

2. Scientifically my main concern with the manuscript is that the patterns for ascent of the air parcels do not correspond well to the observed locations of heavy precipitation and deep convection across the Asian monsoon region. The trajectories could be correct (in the sense that they are representative of the real world), and there could be a physical explanation for why the regions of ascent are displaced from the convection, but it could also indicate a systematic problem with the reanalysis, such as vertical ascent much slower than actual updraft speeds so that ascent occurs far from the convection. The latter would not be surprising given the hydrostatic nature of the reanalysis system model and the necessity for highly idealized convective parameterizations.

I recommend publication after addressing these two points.