Author Comment to manuscript ACP-2022-143 (https://doi.org/10.5194/acp-2022-143, in review, 2022): "Variability of air mass transport from the boundary layer to the Asian monsoon anticyclone"

by M. Nützel et al.

June 23, 2022

We thank the referees for taking time to review our paper and appreciate the referees' efforts to improve the manuscript. In the following we address each review comment (*black italics*) by stating our reply (blue). In addition we appended a manuscript version which highlights the changes between the ACPD version and the revised version.

Reply to comments from Referee #1 (https://doi.org/10.5194/acp-2022-143-RC1)

Below we will address all comments of referee #1 and will state corresponding changes in the manuscript. Again, we would like to thank referee #1 for taking the time to review our manuscript.

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.*

Reply: We agree with the reviewer that for many aspects higher temporal and spatial resolution is favourable. We address this issue in the discussion (Sect. 5.2) by referring to the study by Smith et al. (2021). However, we also note that this is a rather general issue that applies to many problems in our field. Here, we would like to point out that the storage of input and output data as well as the calculation of the trajectories is an issue that needs to be taken into account when conducting such experiments. Our explicit focus was on trajectory studies for many years - and not sensitivities with respect to the reanalysis product or the temporal/spatial resolution. Acquiring the input data for ERA5 (higher temporal and spatial resolution) alone would have been a huge effort. As to the one year sensitivity: using any other reanalysis data (or resolution) would likely influence the quantitative results, however, we assume that the qualitative results would still hold. Such a sensitivity is beyond the scope of our study and as mentioned in the text has been conducted by Bergman et al. (2013). They come to the conclusion that concerning the PBL contributions, when only accounting for PBL crossing trajectories, the effect is relatively limited. We want to point out that we show the results from the free-running EMAC-ATTILA simulation which features the impact of (simulated) convection explicitly. Further, we emphasize that the results from Legras and Bucci (2020) for 2017 with respect to their so-called convective impacts from ERA-Interim and ERA5 data show similar features as our boundary layer source maps (see definition of boundary layer source as reply to your general comment #4). To our understanding the issue of the hydrostatic model would remain for ERA5 as in Section 4 in Hersbach et al. (2020) no transition to non-hydrostatic modelling is mentioned. We also note that comparability with previous studies is an issue and as ERA-Interim has been used often and we had to use ERA-Interim in a related project (because of the mentioned data storage issues), there are also advantages of using ERA-Interim. We further want to note that the reviewer's scepticism with respect to the ERA-Interim trajectory results is likely also related to the reviewer's general remark #3, which we clarify below.

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.

Reply: Yes, the EMAC trajectory calculations were done online with a model time step of 600 s using the submodel ATTILA (Brinkop and Jöckel, 2019). In the revised version a sentence was slightly modified to be more precise: "Within these two EMAC-ATTILA simulations - which have the same grid point meteorology - about 1.16 million air parcels, which represent the global atmosphere, are initialized once at the beginning of the simulation and are consequently transported online with a model time step of 600 s according to the CCM's meteorological fields (Brinkop and Jöckel, 2019)." The "odd" output interval is actually chosen on purpose: The EMAC-ATTILA simulations were not specifically designed for this study and it is common in our simulations to write output data every 10 hours. This is done to capture every second hour of the day (every once in a while). This choice is made to have a reasonable representation of the diurnal cycle and to get better temporal averages in a long-term statistical sense, while limiting the output.

3. Figures 3 and 12: I do not understand why the crossing maps at lower altitudes (e.g., 400 hPa and η = 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.*

Reply: We agree that at first this difference can seem disturbing. However, we want to point out that our analysis is conditioned on trajectories that reach the AMA at 150 hPa. This means we only analyse air masses that find their way to the AMA at 150 hPa. Maps showing precipitation patterns do not have these restrictions. The discrepancy between precipitation maps and source maps has already been noted by Legras and Bucci (2020) (see end of their section 3.1) and also Bergman et al. (2013) touch on this subject (see their Fig. 7 and section 5). We note that precipitation maps from observations (e.g. Xie et al., 2006, their Fig. 1) also do not directly correspond to high cloud distributions in the Asian monsoon region as shown by Devasthale and Fueglistaler (2010). Further, it is noted by Shige and Kummerow (2016) that orographic precipitation over west India is often related to low clouds. Based on these previous studies and our analyses, our understanding is as follows: low- to mid-level convection might be important for the precipitation patterns but air parcels that are transported upwards in this convection need to find a region of onward transport to the AMA. Seemingly, for some of the regions with heavy precipitation this rarely happens. Finally, the maps of convective impact shown by Legras and Bucci (2020) show similar patterns as our analyses, despite the different modelling approaches. This lends further credit to the consistency of our analyses.

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.

Reply: Yes, we account for the last crossing points of trajectories with the top of the PBL, i.e. starting from the initialisation and going back in time, we note where the trajectory first encounters the top of the PBL. We point that out more clearly in the revised version to avoid any confusion. For example, in section 2.3 we now write: "When the pressure at the trajectory position is larger than 0.85 times the surface pressure below the trajectory, we assume that the trajectory has encountered the PBL as described by Bergman et al. (2013). The first location where this happens backward in time will be referred to as boundary layer source of the trajectory." Additionally, at some instances we changed "from the PBL" to "from the top of the PBL" and we changed the wording in the last paragraph of the introduction of the revised version to: "...are followed backward in time to their first crossing of the top of the PBL...". Further, we agree with the referee and we note that we addressed this issue in the discussion (L483-490 in the ACPD version).

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.

Reply: As outlined in our reply concerning your general comments #2, the precipitation distribution does not have to match with the boundary layer source distributions. In accordance, Legras and Bucci (2020) show strong convective impacts from the Tibetan Plateau at and above approx. 360 K with their combined reanalysis/observation modelling approach. Moreover, we have veryfied that the 2D PBL source distribution looks similar for EMAC-ATTILA (not shown) as for the TRJ data, with the main difference that the contribution of the Tibetan Plateau is less pronounced. The differences between EMAC-ATTILA and the TRJ data data are discussed in the lines 360-364 in the ACPD version.

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.

Reply: We shortened the paper and made it more concise. For example, the text in Section 2 before Section 2.1 was partly (re-)moved, the Appendix A1 was deleted and parts from Section 3 have been deleted or shifted to Section 5 and vice-versa.

Minor comments

1. Title: The paper does address variability of transport to some extent, but the main focus is on the mean transport.

Reply: We think that we present a number of analyses showing interannual and intraseasonal variability, e.g. Figs. 2, 7-8, 10-19 of the ACPD version contain information regarding interannual or intraseasonal variability. Of course, we also present many climatological views, which we see as a prerequisite to be able to address interannual and intraseasonal variability. To account for the fact that we present this climatological perspective (as stated in the abstract of the ACPD version), we changed the title to: "Climatology and variability of air mass transport from the boundary layer to the Asian monsoon anticyclone".

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.
Reply: We thank the reviewer for spotting this error: "driven" should rather be "follows". We changed the text accordingly.

3. Line 86: The sentence beginning 'Results from this model ...' is not clearly written.
Reply: Is changed to "Results from the Lagrangian model ..."

4. Line 91: This paragraph is unnecessary and can be deleted.
Reply: As per the reviewer's request, the paragraph containing the manuscript's outline was deleted. The references to Sections 3 and 4 have been shifted to the paragraph above.

5. Figure 5: Please add a pressure scale to the plots.
Reply: We have thought about adding a pressure scale to the plots Figs. 5, 6, 8, 14 and B2 (ACPD version). However, we decided against it, for the following reasons: a) the densities of the trajectory positions have exactly been constructed with log-p height as vertical axis and hence the corresponding units contain the factor km^{-1}, b) the busy figures would get more busy with no real information added as, c) the conversion from log-p height to pressure is straight forward (see updated Figure caption).

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.
Reply: We have combined Figs. 10 and 11 in the revised manuscript.

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).
Reply: We have thought about such a plot, however, we think it is sometimes harder to actually tell the exact quantities as the base for each source region would then vary. Hence we opted for single lines relative to zero.

8. Figure 16: This figure does not add much information to what has al-

ready been presented in Figures 10, 11, and 15. I suggest removing it, or at least combining it with Figures 10 and 11.

Reply: We decided to keep the figure as no interannual variability is given in Fig. 15, whereas it is presented in 16. Figs. 10 and 11 do not show the individual variability of the PBL source contributions according to the different months (June, July and August). The respective text has been shortened and the figure is now combined with the previous Fig. 15.

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.

Reply: As we have already combined Figs. 10 and 11 as the reviewer suggested, we do not see the option to add another data set here. The plots will get too crowded. Further, we agree that the comparison would be easier if everything is in the same figure as subplots. However, we think it is more important to distinguish between the data sets as our focus lies on the TRJ data. Keeping the analyses for EMAC-ATTILA data separate from the TRJ data avoids mixing up the results and is in accordance with the structure of the text, i.e. first the results from the TRJ data and then the results from EMAC-ATTILA. Nevertheless, we included the TRJ results as faint blue dots and whsikers to facilitate the comparison.

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.

Reply: We combined Figs. 18 and 19, however, we kept them separate and also separate from the TRJ results. See also our reply to your minor comment 9.

11. §6: This section is longer than necessary. A short statement of the principal results would be sufficient.

Reply: We shortened the respective section, however, we would like to keep the structure of answering our question from the introduction.

12. Appendix A: This appendix adds little information to what is already presented in §2.2.

Reply: We assume that you are referring to the section A1 as this section corresponds to section 2.2. Hence, we rephrased Section 2.2 and removed the Appendix A1.

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.

Reply: Please consider our reply concerning your general comment #1. We assume, that the scepticism regarding our results is likely also related to the second recommendation of the reviewer. Taking our reply with respect to that comment into account, we do not see any indications of inconsistencies. Of course the quantitative results will change using a different reanalysis or resolution, but the main qualitative results will likely be robust.

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.

Reply: Please consider our comments regarding your general comment #3. In particular, that high clouds, which partly might effectively feed into the AMA and precipitation maps do not necessarily have to align. Again, we want to stress that Legras and Bucci (2020) find similar distributions for their analysis of convective impact at and above approx. 360 K based on ERA5 reanalysis and observational cloud data. Hence, although the distributions of precipitation and source regions are different, there is no scientific inconsistency.

I recommend publication after addressing these two points.

Reply: We hope, that we have been able to sufficiently address the reviewer's comments.

Reply to comments from Referee #2 (https://doi.org/10.5194/acp-2022-143-RC2)

Below we will address all comments of referee #2 and will state corresponding changes in the manuscript. Again, we would like to thank referee #2 for taking the time to review our manuscript.

The paper analyses the PBL sources and the pathways of transport in the AMA UTLS region at climatological level, by use of multiannual back-trajectories and, to understand the convection contribution, CCM simulations.
General comments:
The paper gives an exhaustive view of the transport processes in the region, it's well written, structured and the figures are well presented. The major problem of this paper lies in its verbosity and repetitiveness, which makes the manuscript extremely long and dispersive. I would therefore encourage the paper for publication, after some editing and after addressing some minor points.
Reply: We thank the reviewer for the positive feedback regarding the general presentation of the mansucript. We made the presentation more concise in our revised version. Some of the requested changes from reviewer #1 aim at the same direction. Below, we will reply to all comments made by the reviewer.

Specific comments: The abstract is one particular example of a section that needs to be more concise. It should rather focus on the main points that the authors think the paper is addressing without diluting with too many unnecessary details!
Reply: We shortened the abstract by slightly rephrasing it.

Similarly, between the Introduction and the Data and methods sections, there are several repetitions on the models description and how they will be used.
Reply: We have shortened the Introduction as suggested by reviewer #1. Further, we restructured Section 2 with the aim to reduce repetitions and be more concise.

Line 118: The authors say "Therefore" a modified version of the so-called SAHI index has been used. It would be useful to have a short explanation of what the SAHI is and a more precise explanation of which are the reasons why *it has to be modified for the purposes of this analysis.*

Reply: The corresponding section was rephrased and moved to 2.3.1. It now reads: "For the selection a modified version of the so-called South Asian High Index (SAHI; Wei et al., 2014), which measures the east–west displacement of the AMA, has been employed. The modification, which uses the geopotential height at three pressure levels - compared to one as originally defined by Wei et al. (2014) - is supposed to better capture the 3D structure of the AMA. A detailed explanation for the choice of the years and a description of the selection process is given in the Appendix A2." We hope that the description is clearer and easier to follow now.

Line 152: What does it mean by "Pressure below the trajectories"? Is it the pressure right below the lowest trajectories or right below the mean position of the trajectories? Or the mean value of the pressure in the whole layer below the trajectories?

Reply: Thank you for the comment. The statement was unclear. It is corrected in the revised version: "When the pressure at the trajectory position is larger than 0.85 times the surface pressure below the trajectory, we assume that the trajectory has encountered the PBL as described by Bergman et al. (2013)."

Line 160: It is not clear to me how the choice of the 295m threshold value for the AMA has been made. Is it by comparing the AMA boundaries shape with what obtained from ERA-Interim data?

Reply: To avoid a lengthy description in the text, we referred the reader to the Appendix A2. As the previous description was misleading, it has been updated in the revised version and we hope that the description is easier to follow now. The corresponding part in the Appendix (A1 of the revised version) now reads: "...In principal, we have determined suitable threshold candidates by deriving a single GPHA value, which on average represents the strongest anticyclonic circulation. This was done by calculating the mean of the GPHA values associated with the strongest meridional winds (southward and northward) along the ridge line (see Zhang et al., 2002, for the ridge line). For EMAC-ATTILA, we further required the maximum wind speed to be located at a grid point with GPHA of at least 100 m to avoid noise from unrealistically low values. Using this technique, we determined approximate anomaly thresholds of 280 m and 295 m for ERA-Interim and EMAC-ATTILA data, respectively. The value of 280 m for ERA-Interim is in good agreement with the threshold of 270 m used by Barret et al. (2016)." Additionally, for EMAC-ATTILA we have also checked, that the climatological AMA associated with the threshold of 295 m looks reasonable.

Line 176: The authors compare the 14 years trajectories analysis with the 1981 to 2010 one from the CCM. As the 14 trajectories years has been chosen among the more westward and more eastward shift years of the AMA, I was wondering if it is really representative of the climatology of the period. In addition, are the differences between the CCM and the trajectories analysis related mostly to the convective activity or may be related to the transport behaviour of air masses during the non-considered years?

Reply: A year to year comparison is not possible as the CCM is free-running (see respective text). With respect to the choice of the 14 years: as the East/West years show some differences but the main paths are similar and the discrepancies between the source region contributions are rather small, we assume that the full climatology would not look different. Further, we also point out that the main points of the paper are robust. The difference between CCM and TRJ are likely attributable to two factors: a changed background dynamic and the effect of parametrized convection. A clear separation is not possible from our data and additional simulations and analyses would be needed to distinguish the convective impact (see Summary and Conclusion).

Line 213: I would suggest choosing a different wording than "re-circulation", which recall more the horizontal recirculating patter in the AMA rather than the vertical displacement.

Reply: Actually, what is meant here is a mixture between both: horizontal circulation within the AMA and vertical upward (downward) movement on the eastern (western) side. The later results in a net upward movement and the full pathway is described as "upward spiraling" by Vogel et al. (2019). Anyhow, the respective sentence has been changed in the revised version.

Caption figure 8: can you rephrase the "will be noted at the crossing point also later in time"? It's not clear what you mean with that.

Reply: If a trajectory reaches the PBL it is noted in the analyses at that crossing position, i.e. the position where it first encountered the PBL, also for time points further back in time. As this procedure already applies to the analysis presented in Fig. 5 (ACPD, Fig. 6 in the revision), we rephrased the wording in the corresponding figure caption: "Once trajectories reach the PBL their pathways are not followed back any further. Instead, they are noted at their first PBL-crossing points also for analyses going back further in time. For example, if a trajectory reaches the PBL already after 3 days, it will be counted at this PBL-crossing position also for the analysis 5 days and 15 days back in time." In the figure caption of Figs. 6/8 (ACPD, Fig. 7/9 in the revised version), we write now: "Once trajectories reach the PBL they are not tracked further and will be noted at the crossing point also further back in time (as in Fig. 6)."

Line 255: Why here you choose 2 km and in the figure 3 km as a threshold for the TP?

Reply: We thank the reviewer for spotting this issue. The analysis have all been performed with respect to the 2 km threshold. The outlines of the TP via the 3 km threshold in Figs. 1 and 2 (ACPD version) were given for orientational purposes only. However, to avoid any confusion, in all figures the TP is shown via 2 km contour now. Further, the contours are now also described in Fig. 1 (Fig. 2 revised version; see also our reply to the comment concerning "Caption Figure 1").

Figure 10 and similar: I had some problems understanding how to read the TOT variable. Is it really a percentage (the % of the total trajectories who start in the AMA) or it is just a way to represent the total number of trajectories by the 1 to 4000 conversion? As it's in the same plot as the regional contribution, I would suggest making a clearer separation of the TOT AMA variable from the other percentages, as it would be otherwise confusing!

Reply: The TOT variable is not actually a percentage. The conversion via the conversion factor needs to be used (for Fig. 10: 1% corresponds to 4000 trajectories). In the ACPD version we provided this separation via the light grey vertical dashed line. We made this line darker and doubled it and we made the separation clearer by adding a different axis to the right side of the plot.

Line 262: Does it imply that the uplift is more intense in the TP and IND region, while the WP is contributing as much only because of the larger spatial extent of the defined region?

Yes, concerning the uplift to the AMA we would say so.

Page 21: this whole section can be summarized in a few sentences!

As the Figs. 15 and 16 of the ACPD version have been combined in one panel, we had to revise the corresponding text of Fig. 16 (Fig. 14 b of the revised version) and made the description more concise.

Discussion and Summary and conclusion:
Those two sections are also excessively verbose and with several repetitions between the two. I would suggest cleaning the text and really focus on the important messages (for example the section 5.2 and 5.3 could be significantly shortened) and avoid stating the same conclusion between sections 5 and 6.
We shortened and/or cleaned up the respective sections. Further, as requested by reviewer #1 and #2 we made the entire manuscript less repetitive. Hence, some parts have been (re)moved from/to the discussion/summary.

Technical comments:

Line 3: "analyses".
Reply: Spelling corrected. Thank you!

Line 3: in the same line there is the use of English and American notation. Please correct!
Reply: We are sorry, but we do not see where AE and BE are mixed. However, we exchanged "we analyze" with "we investigate".

Line 29: In the Asian summer monsoon (ASM) regions, the heating. . . .
Reply: The wording has been changed to: "In the Asian summer monsoon (ASM) region, the heating ...".

Caption Figure 1: Better specify here how the TP contours are chosen rather than on Figure 2.
Reply: An explanation regarding the TP contour is now added. Further, the contours have been modified (see your comment with respect to Line 255).

Line 230: put a comma between "indicated above" and "the trajectories start to fill"
Reply: Done.

Line 390: the comma after the "help to discern" can be removed.
Reply: Done.

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