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

by M. Nützel et al.

October 17, 2022

We thank the referees for taking time to reevaluate our revised paper. In particular, we also thank the editor for taking the time to read our manuscript and provide detailed and helpful suggestions/comments to improve the paper. Both, the previous reviews and the current editor comments are very much appreciated. In the following we address each comment of the editor (*black italics*) by stating our reply (blue). In addition we append a manuscript version which highlights the changes between the revised version and the current manuscript version, i.e. the version after the second revision.

# Reply to editor comments

Below we will address all comments of the editor and we will state corresponding changes in the manuscript. Again, we would like to thank the editor for taking the time to comment on our revised manuscript.

*Editor decision for paper*

*acp-2022-143*

***Climatology and variability of air mass transport from the boundary layer to the Asian monsoon anticyclone***

*by M. Nützel et al.*

*I thank the authors for submitting their revised version. While reviewer 2 is happy with the revisions and accepts the paper for publication, reviewer 1 – a very experienced colleague – contacted me offline and indicated that "the authors largely rejected my comments and did not do much, if anything, that I suggested. I can't recall having a review treated this way before. I see two options: 1) to ask them to take my comments seriously and revise the paper, or 2) to publish as is. I am OK with option 2, although I think it leaves the paper much weaker than it needs to be. I will leave the decision up to you." While I understand that redoing the entire study with ERA5 would be an enormous task and beyond what can be done during revisions, the remark that the reviewer felt his/her comments to be largely ignored is problematic. I therefore had a closer look at the revised version, having in mind the general comment 6 from reviewer 1 about the conciseness of the writing. Although you shortened certain parts, the text is not yet fully reader friendly. I found several parts of the text unclear or distracting. Below my comments and suggestions.*
We greatly appreciate the editor's thoughtful comments and suggestions, which we will address below. We have revisited the point from Reviewer 1 with respect to the sensitivity of using ERA-Interim vs ERA5 to calculate the trajectories. Although we have considered to re-do the calculation for one season using ERA5, the decision in the end was to discuss the expected changes if using ERA5, based on some new diagnostics performed by co-author Laura Pan's group. These diagnostics go beyond showing differences in trajectory model studies driven by

ERA-Interim vs ERA5. The vertical wind products and the trajectory results are evaluated using two observation-based diagnostics. This discussion is now included in revised Section 5.2, cited here:

> "The representation of convective transport in the trajectory analyses forms the leading uncertainty in our results. This uncertainty can be addressed with two related questions: 1) how well is convective transport represented in trajectory analysis, which use the resolved winds of analysis products? 2) What is the sensitivity of the calculations to the analysis products used? In particular, what is the influence of the relatively coarse spatial and temporal resolution of the ERA-Interim data employed in this study (here 1.5° and 6 hourly) on the presented results versus that of the newer generation reanalysis ERA5 (Hersbach et al., 2020) at high horizontal resolution (∼0.25°), provided in hourly intervals?
>
> These questions are examined in a recent work by Smith et al. (2021), in which convective transport time scales were quantitatively characterized using transit time distributions (TTDs), analogous to the age spectra, or distributions of the age of air, in stratospheric transport studies (e.g. Hall and Plumb, 1994). The work uses a set of diagnostics to quantify the representation of convective transport in trajectory calculations, specifically, by comparing TTDs from trajectory model results with the chemical lifetime-based TTDs derived from airborne in situ measurements over the convection dominated Western Pacific. Four sets of wind products from commonly used operational analyses and re-analyses are examined in this study, including ERA-Interim and ERA5. The results of the study indicate that the trajectory-based TTD from ERA5 has comparable mode and mean to that of the chemical-lifetime based TTD. The ERA-Interim based TTD on the other hand, shows considerably slower transport, although showing qualitatively similar distribution in transport origins at the boundary layer. Using the TTD diagnostic, the ERA-Interim based calculation misses approximately 30% of the convective transport (Smith et al., 2021, Table 2).
>
> Based on this diagnosis, we expect that if the higher spatial and

> temporal resolution products from ERA5 were used, the result of this study would show enhanced convective transport which should lead to a higher percentage of back-trajectories that reach the top of the PBL within the season. This assessment is also in agreement with the presented EMAC-ATTILA data, which contain the effect of parametrized convection and show an higher fraction of young (<90 days) air masses in the AMA than the TRJ data (Fig. 14). Further the EMAC-ATTILA data also support key characteristics of the transport pathways and the increasing contribution of the TP to AMA air masses over the course of the monsoon season. For the distribution of PBL source regions, although we expect changes in detail, the overall conclusions in the large-scale perspective are not expected to change. The latter is also supported by Legras and Bucci (2020), who show similar source regions based on ERA5 (and ERA-Interim data) with an entirely different modelling approach (i.e. a combination of reanalysis and observational data)."

The statement regarding the PBL source regions is also supported by a more recent paper, Pan et al., in revision (JGR, minor revision), where the time scales and contributing boundary layer of Asian Monsoon transport over the Western Pacific are calculated using a trajectory model, which is driven by ERA5 for one season. This - yet to be published - new result serves as an update to the published 39-year climatology based on ERA-Interim (Honomichl and Pan, 2020). In this case, the ERA5 result is consistent with the ERA-Interim in the large-scale perspective, although the ERA5 result provides much better details in the distribution of contributing boundary layer. Based on these studies (Smith et al., 2020, and Pan et al., in revision), we expect that a re-do of the study using ERA5 would add significant more insight into the transport process, but it would be "an enormous task and beyond what can be done during revisions", as remarked by the editor. Further, even a single season intercomparison would need additional experiments e.g. as in (Hoffmann et al., 2019) to provide context and would clearly shift the focus of the paper. We hope the discussion in the revised Section 5.2 provides sufficient information for the readers to put the presented results into perspective.

*L4: to me this sentence only makes sense if I insert hyphens "... displacements of the AMA with the PBL-to-AMA-transport". Is this what you intend to say? This term appears many times in the paper. If you prefer a formulation without hyphens then I would suggest "... with the transport from the PBL to the AMA".*

Yes, that is what we wanted to convey. We have checked the entire manuscript and changed the respective phrase to either the first (including hyphens) or second suggestion (no hyphens) of the editor.

*L11: why "above"?*

The sentence was adapted. Please see our reply to your comment L207 concerning "below".

*L15: why not simply "variability of PBL source regions"?*

Changed.

*L34: you might like to add here a reference to the recent paper by Clemens et al. 2022: Clemens, J., F. Ploeger, P. Konopka, R. Portmann, M. Sprenger, and H. Wernli, 2022. Characterization of transport from the Asian summer monsoon anticyclone into the UTLS via shedding of low potential vorticity cutoffs. Atmos. Chem. Phys., 22, 3841–3860.*

The reference was added. Thank you for pointing it out.

*L46: you often use "with respect to" when – in my view – a simpler construction would be much clearer, see also remark above. Here my suggestion would be "highlighted the importance of the Tibetan Plateau for the transport ..." Please ask the native speakers in the team of authors to check the use of "with respect to" throughout the paper.*

Corrected here. We have checked the entire manuscript for "with respect to" and also for "via" and largely replaced the phrases.

*L55: "analysis" should read "analyses"*

Corrected. Thank you.

*L78: not sure whether I understand this question. Do you mean "Are the PBL source regions and the transport pathways affected by / sensitive to inter-annual east-west shifts of the AMA?"*

Yes, this is what is meant. The very simple question would be: If the AMA is located rather to the east or west, do we see any differences in the pathways or/and source regions? We chose the wording "related to" on purpose as the east–west shifts of the AMA might not be what is causing the different contributions in the first place but might rather be themselves a response to changed heating, i.e. we did not want to imply a causal relationship. We changed the wording to "sensitive to" - hoping that this does not suggest a causal relation, while keeping "related to" at a few other instances in the text.

*L82: no need for "In particular"*

Removed. The sentence now starts with: "These Lagrangian CCM results ..."

*L83: this sentence does not work, maybe "Results from the Lagrangian model will serve for a comparison with ..."*

We rephrased the sentence to: "Results from the Lagrangian model will help to assess the sensitivity of the results to the modelling approach as..."

*L97: I wonder whether the results of the study are sensitive to the choice of the starting level – here 150 hPa. This choice is not well motivated. Would you have trajectories at hand to check, whether a starting level of 100 or 200 hPa would lead to different results? At least you should better explain why this starting level is appropriate (and sufficient) to capture the entire transport from the PBL to the AMA.*

We added a paragraph to explain the choice of the starting level of the trajectories:

> "We chose the 150 hPa level to initialize the trajectories as it roughly corresponds to the 360 K from which trajectories tend to further ascend into the stratosphere (Garny and Randel, 2016). Moreover, the 150 hPa level is a level where we find strong anticyclonic circulation based on the maximum and minimum zonal wind speeds in the UT in the Asian monsoon region (see e.g. Fig. 1 of Garny and Randel, 2016). From the analysis shown in (Bergman et al., 2013) for the 100 and 200 hPa level, we expect that our qualitative results are not strongly dependent on the choice of the starting level."

*L149: Is it correct that you use this GPHA threshold criterion only at 150 hPa? If yes, please mention this explicitly.*

For TRJ the threshold is used only at 150 hPa to select the AMA trajectories from all trajectories initialized at 150 hPa. But for LG data as a starting range is used (140-160 hPa) the criterion is applied for this pressure range together with a restriction on the longitude/latitude to filter out trajectories that start within the AMA. We have updated the respective text and explicitly added the sentence: "We emphasize that the GPHA criterion is only applied once at the starting point of the trajectories or air parcels to determine whether they are located within the AMA."

*Section 2.3.1: I find it a bit painful to read this section. Please shorten the text, if I understand correctly, what you explain here is that you do not consider all years from 1979 to 2013, but only 14 years, and you selected them such as to capture the variability in the W-E position of the AMA as expressed by the South Asian High Index. This can be said in a few lines. And please list the 7 years each that were chosen for the west/east position of the AMA.*

Yes, your understanding is correct. We have shortened the respective section. For further information on the SAHI (as requested by referee 2) and the list of the selected summer seasons we refer the reader to the Appendix: "The selected summer seasons are listed in the Appendix A2, where also a description of the modified SAHI and of the selection process is presented."

*I don't think that Fig. 2 is needed in this paper. Vertical motion at 150 hPa is not very relevant for the transport from the PBL to this level.*

We have checked the differences also at 175 hPa (and also at 200 hPa) and the differences look (relatively) similar to the differences at 150 hPa. Previously, we showed this figure to motivate the choice of the 14 summer seasons. Nevertheless, as suggested we have removed this figure and the motivation for the selection is now entirely by referring to Fig. 14 of Nützel et al. (2016).

*L198: Just write "First, we investigate the climatological ..."*

Changed as suggested. To increase the readability of the paper, we checked the entire manuscript and tried to shorten/adapt introductory clauses, where appropriate.

*L204: I think this is a very important point: you write here that you only consider trajectories that reach the PBL top within 90 days. How many of the AMA backward trajectories started at 150 hPa fulfill this criterion? I think it* *is important to mention this percentage. If it is substantially lower than 80%, then it might make sense to show Fig. 3 only for the trajectories that also fulfill the PBL criterion. Currently it is a bit strange that so many trajectories are started at 150 hPa over the Arabian Peninsula (Fig. 3), but this region does not appear at all when looking at the 200-hPa crossings (Fig. 4a). This should be discussed, and maybe the reason is that the Arabian Peninsula trajectories don't reach the PBL within 90 days(?).*

We agree that it is helpful to early state the fraction of AMA trajectories that reach the PBL. Hence we added a sentence in Sect. 3.1: "For the analysis of the transport pathways, we will only consider trajectories that start within the AMA and reach the PBL within 90 days, whereas in the analyses of the PBL sources we also quantify the fraction of trajectories starting within the AMA that do not reach the PBL within 90 days (roughly 15%, see Sect. 3.1.2)." Although it can be assumend from the large fraction that reaches the PBL within 90 days (∼85%), we have explicitly checked that Fig. 3 does not change substantially if only PBL crossing trajectories are considered.

The trajectory starts at 150 hPa are simply related to the occurence of the AMA in the respective region. As the AMA spans also to the Arabian peninsula (see e.g. Fig. 4 in Nützel et al., 2016), the start of trajectories at 150 hPa in this region are correct. Further, a trajectory started at 150 hPa over the Arabian Peninsula, does not have to vertically ascend to that level from the Arabian peninsula. The trajectories are indeed transported upward over the south-eastern side of the AMA as can be seen in our Fig. 4, which is in agreement with the findings of (Bergman et al., 2013). We think that additional clarification of that issue will be given in our replies to your comments L209 and L218.

*L207: I don't understand "or below"*

As the AMA starts only at a certain height level we wanted to be clear that the upward transport is on the south-eastern side of the AMA where it exist and below the south-eastern part of the AMA in the height region, where the AMA does not exist. However, we can understand that this distinction is confusing, so we rephrased to try to make our statement clearer: "With increasing height, the upward transport of air masses focuses on (the region below) the south-eastern part of the AMA."

*L209: I apologize but I am lost here. What you show in Fig. 4 are trajectories that fulfill at 150 hPa your AMA criterion and that cross the PBL height* *within 90 days (backward in time). I don't understand what you mean by "not necessarily the full three-dimensional pathways", do you mean by this, e.g., trajectories that don't go back to the PBL or trajectories that rise up only to 200 but not 150 hPa? And I am totally at lost with understanding why you show Fig. 5. Why is Fig. 5a so totally different from Fig. 4a? Also, the distinction between trj1 and trj2 does not seem very relevant to me. Things should be clear if you write that Fig. 4 shows the last upward crossing of the XX hPa level. (I assume that if a trajectory crosses a certain pressure level more than once, you only retain one crossing?).*

There is still a misunderstanding and it seems that we have not been clear enough with our explanation. With the "full 3-d pathways" we do not mean a different subset of the trajectories, i.e. we still analyze trajectories that start in the AMA at 150 hPa and reach the PBL within 90 days. And yes, only the final crossing points are retained in Figs. 4, 8 and 11. To avoid any misunderstanding we replaced the phrase "full pathways" (or similar) and we noted in the discussion of Fig. 4 that only the final crossing point is registered.

Figs. 5a and 5b were actually meant to clarify why the analyses e.g. in Figs. 4 can not be used to infer that trajectories are only located in these regions on their way to the 150 hPa level in the AMA. Fig. 4 and Fig. 5 a are different as they depict the upward and downward crossings of trajectories at ∼200 hPa. As on a climatological basis on the east side of the AMA upward winds are present and on the west side downward winds (e.g. Nützel et al., 2016, their Fig. 10), upward crossings as they are diagnosed in Fig. 4 are most likely to be detected in this region. Hence, although trajectories might be located also at different horizontal positions (e.g. in the western part of the AMA, as seen in the starts of the TRJ or the density distributions Fig. 6 etc.) they will only be noted on the eastern side in analysis of Fig. 4 as this is the region where they are transported upwards. The "snapshots" of the location of the trajectories 1, 2.5 , 5 and 15 days prior to their starting date (Fig. 6) do not exhibit this "flaw" and hence trajectories that circle within the AMA and are located on the western side at the time of the snapshot are noted as well. In contrast, Fig. 5a reverses the analysis in Fig. 4 and looks where trajectories experience downward movement and hence here (in agreement with the location of downward movement on the western side of the AMA) the western side of the AMA shows up in this analysis. The hypothetical trajectories trj1 and trj2 are different as trj1 experiences upward and downward motion close to the 200 hPa level, whereas trj2 simply continues to further rise after crossing the 200 hPa level: the final crossing points of the 200 hPa level of both trajectories are registered in Fig. 4 - and actually all PBL-crossing trajectories are registered once in Fig. 4 as they somehow have to cross the 200 hPa level on their way from the PBL to the 150 hPa level. As only trj1 experiences the upward/downward transport around the 200 hPa level, only trj1 is noted in Fig. 5a which displays the regions of downward transport of trajectories. To facilitate the understanding, we adjusted the figure captions of Figs. 4 and 5a, so the terminology "upward" and "downward" crossing are easier to spot. Further, we adjusted the discussion of Fig. 5a (now Fig. 4a).

*L218: Here you write "To get a better picture of the full transport pathways ...", which is now confusing after Fig. 5. Do you now continue with the trajectories shown in Fig. 4, or does "full transport" mean that you include here other trajectories as well? I wonder whether the results in Figs. 4 and 6 are fully consistent. Fig. 4a shows no 200-hPa crossings west of 60°E, whereas Fig. 6d shows many trajectories west of 60°E at altitudes from 9- 15 km. Please discuss this discrepancy or my misunderstanding when comparing the two figures. And as noted by one of the reviewers, it would be most helpful to have a pressure axis in Fig. 6 (e.g., to make a good comparison with Fig. 4). This would be much more reader-friendly than the barometric height formula and the complicated text in L219.*

The subset of trajectories does not change between Figs. 4, 5a and 6. The wording has been adjusted to avoid any confusion. We hope that our reply to your comment L209, clarifies that there is no inconsistency, as the difference between Figs. 4 and 6 is entirely caused by the underlying analysis method. Fig. 4 depicts the locations of the final upward crossings of trajectories through a specific surface - hence only regions and time steps where trajectories experience upward motion are noted. In contrast Fig. 6 does not make such a restriction, but simply shows a snapshot of the trajectories on their pathway to the 150 hPa level. We included the pressure axis in the respective plots to facilitate the intercomparison. We did not do that before as the figures (in the multi-panel) get smaller and as the units of the displayed quantities are given with respect to the log-pressure height. Further, we adapted the figure captions and tried to shorten the explanation in L219.

*L238: where can the reader see the "upward circling"? I don't doubt that this interpretation is correct, but I don't see it in the results shown. My understanding of upward circling is that the trajectories follow a circular path in*

*the horizontal from the PBL to the AMA at 150 hPa. But the panels in Fig. 4 allow all sorts of interpretations of how air parcels, e.g., from the Northern Philippines move from the PBL to 150 hPa. This ascent could also be rather vertical according to Fig. 4, so what does "circling" mean?*
Maybe this is a misunderstanding. The upward circling was meant to occur only after the trajectories have been transported to a certain height/pressure level: so there is first vertical transport and then recirculation within the AMA (with downward/upward transport on the western/eastern side of the AMA). This is supported by our analyses (Figs. 4, 5a and 6) and is in agreement with the findings of Vogel et al. (2019) and Legras and Bucci (2020) (and to some extent with the study by Bergman et al. (2013)). Whether the net circulation is upward or not, we cannot deduce with our analysis and hence when it comes to our study, we rephrased the term "upward circling".

*L239: typo in "refines"*
Corrected.

*L256: very complicated "on individual dates with respect to the initialization date". I assume that Fig. 9 is done in the same way as Fig. 6?*
Yes, Fig. 9 shows the corresponding differences of Fig. 6 for west minus east years. We agree that it was difficult to follow, hence we revised the respective sentence: "To capture the differences of the trajectory pathways between years with a rather western and rather eastern position of the AMA, Fig. 9 shows the corresponding composite differences (west minus east) of the analyses in Fig. 6."

*L259: "remain stable" is not clear enough, what you mean is that the PBL sources seem to be very similar in years with an eastern position of the AMA vs. years with a western position of the AMA.*
Yes, concerning the pathways that is what we meant. Hence we rephrased to: "Overall, there are no qualitative differences in the transport pathways between years with a rather eastward and years with a rather westward location of the AMA."

*L267: Oh, now you quantify for the first time the trajectories that do not cross the PBL within 90 days (see my comment above)! I don't find it ideal that now, in Fig. 10, you consider all trajectories started from the AMA, whereas Figs. 4-9 only considered those that crossed the PBL. Therefore, the percentages* *in Fig. 10 do not correspond to percentages of trajectories shown, e.g., in Fig. 4. It would be more reader-friendly, if the noX trajectories were mentioned earlier in the paper (before Fig. 4) and from then on, only PBL-crossing trajectories were considered.*

In response to your comment on L204, we have added a clarification that for the transport pathways only PBL-crossing trajectories are analysed in Sect. 3.1. We hope that this clarification helps the reader to follow the manuscript more easily. We agree that the percentages (if one would integrate e.g. Fig. 4 at 0.85*ps) would not match with the percentage given in Fig. 10. However, we think that the information on the noX trajectories is valuable - as we would also guess from your comment L204 - and decided that it should not be removed from the plots. Hence, we also though about reversing the appearance/discussion of the plots. That would mean to first discuss Fig. 10 and then Figs. 4-9 etc. However, accordingly also Figs. 11-13 and 14 would need to be switched, leading to the problem that the reader would have to switch between different "bases" (AMA vs AMA and PBL-crossing) again. Thus, instead of removing the noX trajectories or reversing the appearance, we worked on that issue by being more precise which subset is being analysed, e.g. by updating Sect. 3.1. and by explicitly mentioning the noX trajectories in the discussion of the respective analyses.

*L273-284: I suggest omitting this analysis, because it is already clear from Figs. 8b and 9 that the east-west position of the AMA does not matter for the PBL source regions.*

We agree that this is the case for the mean, however the interannual variation and also additional information, e.g. concerning the total number of trajectories and the fraction of the noX trajectories can not be inferred from Figs. 8 and 9. Hence we made the compromise to keep the figure while we substantially shortened this paragraph.

*L301: "transport from the TP into the AMA occurs vertically" – how does this correspond to the "upward circling" mentioned before?*

First we hope that it is now clearer, that the recirculation is meant to take place only after a first "vertical uplift" (see our reply to your comment L238). Still, we thank the editor for spotting this unclear statement. What is exactly meant is, that in June air masses, which are transported vertically above the TP eventually encounter the STJ (typically at levels below 150 hPa) and get advected out of the monsoon region - hence they cannot contribute to the AMA air masses at 150 hPa. In August this is different as air masses transported from the TP can either ascend vertically up to 150 hPa or get entrained into the AMA circulation at some level and from there circle to the 150 hPa level (of course not all air masses from the TP but these are the ones that get noted in our analyses). Hence we rephrased to: "In August the AMA is located above the TP and air masses from the TP can directly feed into the core of the AMA."

*L304: no need to motivate here again the need to look at intraseasonal variability, as you already discussed this in the previous subsection!*
The motivation was deleted here and the motivation in Sect. 3.1. was revised.

*L309 and Figure 14: again, it is not ideal / not necessary that the noX trajectories are included.*
Please consider our comments to your comments concerning L204 and L267.

*L339: To me a supplement is a separate document, but you include Fig. B2 in an appendix, which is part of the main paper. Please decide about your strategy and terminology.*
This is also our understanding. We would have split the documents in the end. But as the supplement is not long we thought it is more convenient for the review process to have all data in one document. Consequently, we decided to keep the figures in an Appendix called "Supporting figures".

*L363-366: I suggest omitting this short paragraph, because the reader does not really understand how you varied the PBL identification, and it is a bit arbitrary to test this sensitivity for LG-D but not for TRJ.*
As suggested we deleted the paragraph.

*Discussion of Fig. 17: I am a bit confused why now hemispheric results are shown and the discussion includes the North American monsoon. Why not confine the analysis and discussion to the main theme of the paper?*
We want to show where TP trajectories are located in the UT in June vs. August at various pressure levels to show the possibly stronger confinement/dispersion of TP trajectories. Hence, we need to make the analysis globally and such an analysis is only possible using the LG data and can not be done with the existing TRJ data. As can be inferred from Fig. 17 in August compared to June the TP trajectories are more likely to be located in the Asian monsoon region (stronger

confinement), whereas in June compared to August, the trajectories are dispersed more strongly and located downstream of the Asian monsoon region. This result corroborates the results from the TRJ data about a key process, which causes the different contributions of the TP in June vs. August. As the difference of the probability densities shows a local minimum in the North American monsoon region, we simply stated this in the text to explain this feature.

*L427-435: Why introducing here a discussion about the MHI? I think it is one conclusion that the west-east position of the AMA does not influence the upward transport substantially, so why then adding an excursion about MHI and SAHI?*

You are right, the east-west position has no substantial impact on the pathways or PBL sources. The reason to include here the discussion on SAHI and MHI is to motivate that it is likely that there are no dependencies/differences of the PBL sources etc. if stratifying/compositing against/with the MHI. We think, that so far the connection between MHI and SAHI has not been analyzed.

*L454: I would not dare to make such a statement. All data sets used so far for studying the transport into the AMA are far away from convection-resolving simulations. I think we need such simulations to really assess the impact of deep convection.*

We fully agree and we thank the editor for spotting this unclear statement. Hence, we went through the manuscript and rephrased to only address parametrized convection with such statements. The sentence at hand was actually removed during the revision of Sect. 5.2.

*L500: "However, we found an upward circling already considerably below 150 hPa for approximately half of the PBL crossing trajectories." I don't understand where this result has been shown in this study.*

Concerning the use of the phrase "upward circling", we refer to our reply on your comment L238. Further, in the discussion of Fig. 5a (now Fig. 4a), which shows the regions of downward crossing, we note that approximately 50% of the PBL-crossing trajectories are noted in the respective analysis. From this we infer that roughly 50% of the trajectories recirculate within the AMA considerably below the starting level.

*L501 "The attribution of PBL source regions, however, is less clear" – what do you mean by less clear? Do you mean are more sensitive to the model / approach used? Or do you mean that a large set of source regions contributes?*
We rephrased the sentence and also added a colon, to indicate that the explanation follows. "The attribution of the PBL source regions, however, is less clear as it is more sensitive to the modelling approach: In TRJ, ..."

*Finally, I would like to briefly comment on your replies to the general comments of reviewer 1. While I can follow your argumentation about the difference between PBL source distributions and precipitation – indeed, you "only" look at upward transport that reaches 150 hPa – I thought that you might be able to do some sensitivity tests with your TRJ approach using hours ERA5 data. You mention that doing this for the entire study would be a huge effort. I fully agree. But already backward trajectories from 150 hPa for a single JJA season with hourly ERA5, 6-hourly ERA5 and 6-hourly ERA-Interim would be tremendously insightful. I cannot estimate how difficult it is for you to do such an analysis, and therefore I leave it up to you whether you include it or not in the final version of your paper. And about the complex link of the source maps and precipitation: could it make sense to discuss this in your discussion section? I find it interesting that with a starting level of 150 hPa, one obviously "misses" a lot of the vertical transport in the monsoon region associated with intense precipitation.*
We agree that including a discussion concerning the differences between precipitation maps and source maps makes sense. We have added a paragraph about this issue in the discussion Sect. 5.1, which is based on our previous reply to reviewer 1. This paragraph explains the seeming inconsistency between precipitation maps and source regions of AMA air masses. Actually, we debated about including such a discussion in the last version (first revision, 13 July) of our paper, however, previously we decided otherwise to shorten the manuscript.

The sensitivity of our results with respect to ERA-Interim has been addressed in the revised Section 5.2. Please, see also our reply to your first comment. According to the method of quantifying convective transport in Table 2 of Smith et al. (2021), ERA-Interim missed $\sim$30% of convective transport in the trajectory model experiment over the Western Pacific. A single season sensitivity test is done in the work of Pan et al., (JGR in minor revision) for a very similar problem (see Honomichl and Pan, 2020, where the boundary layer and transit time of air mass transported from the AMA to Western Pacific are quantified using ERA-I driven kinematic back trajectory). The result shows similar spatial pattern of PBL encounter in the large scale but with more details along the monsoon trough.

# References

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