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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Transport pathways from the Asian monsoon anticyclone to the stratosphere" *by* H. Garny and W. J. Randel

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We thank all Reviewers for their detailed comments and suggestions to improve our manuscript.

Two major changes to the manuscript in response to the review comments are detailed below. All additional changes and point-by-point answers to the review comments are listed below, and a manuscript version with all changes highlighted can be found as supplement.

a) Extension of calculations to include trajectory initialization throughout August

In response to comments by all three reviewers we extended the initial dates for the trajectories throughout August. Accordingly, Figures 7, 8, 10, 11, 12, 13, 14, 15, 16 and 17 as well as Table 1 was updated with values now averaged over the whole JJA season. Overall, all results shown in the paper are very similar when averaging over the three month June-July-August to the former results including only June-July. The percentages of trajectories transported to different regions change by a few percentage points at most. Thus, the conclusions of this study are not affected.

However, we do find some seasonality in the transport destinations, in particular in the amount of downward propagating trajectories. The downward transport is in particular strong in July, while it is much weaker in June (and intermediate in August). This seasonality can be explained by the downward velocities (diabatic heating rates) in the western part of the anticyclone, which are particularly strong during July. We added a sentence on the variability of downward transport to Sec. 4.2.3. We decided not to include more discussion on the seasonality of transport pathways, since robust results on the seasonality would require calculations for more years.

b) New section on uncertainties

In response to several comments by all three reviewers, we included a new section that discusses the uncertainties in our calculations. To address them, we performed a number of sensitivity studies, both with respect to the set-up of trajectory initialization (location / number) and with respect to the high bias in ERA-Interim heating rates. It is found that the results are very robust against the trajectory initial set-up, and that overall conclusions are not critically depended on uncertainties in vertical velocities (as far as they could be tested).

Reviewer 1

The work by Garny and Randel shows a Lagrangian analysis of the effect of the Asian summer monsoon on the UTLS region. The authors initialize trajectories in the region of the anticyclone using a combination of latitude and PV as condition for the monsoon region. They run 60 day forward calculations to study the evolution of the air mass transport pathways. Importantly they use both, kinematic and diabatic vertical velocities for the vertical motion. The authors show that trajectories are more confined at Theta = 380 K compared to 360 K. The calculations results in a budget of air parcels, which have been transported from the monsoon to other atmospheric regions. Based on these budgets the authors state that transport from the Asian monsoon via the trop- ical UT strongly affects the tropical LS. This pathway also contributes to the northern hemispheric lower stratosphere as well as direct mixing but to a lesser extent. However, I'm surprised, that the authors provide no comparison e.g. with the Lagrangian cold point. This could be easily compared with observations and would give more ob- servational evidence to their conclusions. Overall the paper is very clear, well written and provides a new look on the budget of the Asian monsoon impact on the UTLS region. Particularly the use of diabatic and kinematic vertical motions makes their re- sults robust. I therefore recommend the manuscript for publication after considering the following comments.

Main comments:

1) Since the authors use 60 day trajectories: To get a better link to observations for verification the authors could add a plot showing the water vapour sat- uration mixing ratio based on the Lagrangian cold point. This should be consistent with the observed effect of the Asian monsoon on the isentropic water vapour distribution (Randel and Jensen, 2013). The authors should be able to separate in their 3D-real experiment tropical cold point from monsoon cold point air masses (p.25995, l.10 ff.).

Thank you for this suggestion. We feel that this is an interesting and worthwhile analysis, but it is sufficiently complex to deserve detailed analysis beyond the scope of our present study. We choose to leave it for a follow-on study, so it will have enough space.

2) They only run forward calculations initialized in the first half of the monsoon season. Therefore their fractions in Fig.13 and 15 are not valid for the whole monsoon time. Particularly during the late monsoon phase and monsoon breakup the fractions and impact on the LS can be expected to be much higher. Did the authors look at this? If not they should at least discuss this in the conclusions.

See above.

p.25985, l.18: Is the trajectory tool referenced in literature or is the tool used in this study a newly developed tool?

The simple trajectory model used here is not referenced elsewhere. We made this clear now in the text.

p.25985, l.20: What is the motivation to do the 2D isentropic calculation?

The idea was to study isentropic mixing first, in particular to answer the question how confined the anticyclone is at a certain isentropic level, and whether there is substantial isentropic mixing to the extratropics. The motivation is given in the Introduction (former P25984, line 10-15).

p.25987, l.20ff.: Why do trajectories, which are initialized in August in the monsoon sector, not represent the monsoon season? This seems to be inconsistent with isentropic trajectories, which cover August.

Our thoughts were that the 60-day trajectories initialized in August will extend well into October, when the monsoon circulation has faded. However, we re-thought this issue, and trajectory calculations are extend now (see above).

p.25992,l.13 and Fig.12: How was the tropopause crossing point determined?

Information is added on the definition of tropopause crossings, and the sensitivity of conclusions on this definition at the beginning of Section 4.2 (see also comments by Reviewer 2).

Fig.17: Please indicate diabatic or kinetic case in the caption.

Done.

Reviewer 2

This paper constitutes a model study on transport pathways in the Asian monsoon anticyclone based on trajectory calculations. The focus of the analysis in on the potential temperature regime in the range 300-450 K. One major result of the paper is a quantification of the relative strengths of different transport pathways and, in particular, a quantification of transport into the stratosphere. The model experiments are appropriately designed; for example, choosing a range of injection heights based on OLR (section 5 of the paper) is a very positive aspect of the analysis. The topic of the paper attracts much current interest and its results will certainly be of interest to the readership of ACP. The paper is well written and well structured.

However, there are a number of issues in the paper, where I think the discussion should be refined or extended. The results of the trajectory study depend of course on the as- sumptions made and the choice of initial conditions. The trajectories are initialized in the core of the anticyclone and (three-dimensional) trajectories are only started until 31 July. Why is this time period not extended to later in the monsoon season? (Perhaps with shorter trajectories). On some occasions, there should also more clearly be dis- tinguished whether the transport into the stratosphere means transport into the tropical pipe or transport into the lowermost stratosphere in the extra-tropics. An important issue is also the definition of the northern boundary

of the tropics - it is assumed to be located at 45°. I suggest to move this value to a more southern latitude (see also below). The paper presents an analysis for three-dimensional trajectories with both kinematic and diabatic vertical velocities. This is certainly a strength of the paper. However, I would suggest that the authors are a bit more ambitious with assessing the positive and negative aspects of the kinematic and diabatic world. Rather than just juxtaposing the results for both worlds. Of course this is sometimes difficult to do. Also I would suggest to include some more information on the diabatic world, one example is to present the results shown in Fig. 6 also with potential temperature as the vertical coordinate. In summary, while I expect the paper to change in view of these comments (and likely the comments by other reviewers) I am convinced that it will develop into an excellent contribution to ACP.

Detailed comments

Trajectories
The present study relies on ERA-interim reanalysis products, which overestimate of the rate of tropical upwelling in the lower stratosphere (e.g., Dee et al., 2011; Schoeberl et al., 2012; Ploeger et al., 2012). ERA-interim is certainly a good choice and one of the best reanalysis products available. However, this weak point of ERA-interim should be discussed in more detail in the paper. Could it have an impact on the conclusions about upward transport and transit times reported in the paper?

True, the fact that upwelling is overestimated in ERA-Interim very likely biases our results towards an overestimation of transport into the (tropical) stratosphere. We included the references on the general overestimation of upwelling in the lower stratosphere in the new Section on uncertainties. Furthermore, we performed a sensitivity on the possible impact of too high heating rates on our results.

Further, the trajectories are initialized in the core of the anticyclone defined by low values of PV. The same value of PV is used throughout the period from 1 June to 31 August, which might be justified given the recent work by Ploeger et al. (manuscript submitted to ACPD). But the results regarding the fate of the trajectories should depend on the initial position. For example is it more likely for trajectories in the core of the anticyclone to reach the stratosphere than for those at the edge of the anticyclone? Is it more likely for trajectories at the edge of the anticyclone to be exported to mid-latitudes? Are the PV criteria for 360 and 380 K selecting about the same fraction of the anticyclone? What is the criterion for choosing these PV values? I suggest that the sensitivity of the results of the study on the chosen initial position should be discussed in some detail.

Point 1: Sensitivity to initialization position: We tested the sensitivity to the location of trajectory initialization, and the results are found to be very robust (see new Section 6 of the paper). Overall, conclusions on the relative importance of the different pathways are not sensitive even when using this broad region of initialization. This indicates that the initial position of the trajectories can determine the transport pathways for a time period of 10-20 days, but the longer-term transport over 60 days is not critically depended on the initial positions. This is likely because the time scale of horizontal transport is smaller than the one for vertical transport, i.e. as stated in the reviewer's comment below, the trajectories will circle the anticyclone a couple of times while ascending/descending.

Point 2: PV values chosen: The PV values chosen are based on the results by Garny and Randel 2013 (JGR), where we showed that the 0.3 PV area is a good measure of anticyclone strength, and represents the anticyclone core region well. The value of 3 PVU at 380 K was chosen as it represents a similar area, i.e. a similar amount of trajectories are released (about 1000-2000 per day).

Regarding the discussion of the eastern and western part of the anticyclone, I think that a more "lagrangian view" is necessary; i.e., trajectories will likely sample both parts of the monsoon so a strict separation between behavior in the eastern and western part

is not meaningful (see also below).

True, and as stated above the initial position is not critical for the overall conclusions of the study. Therefore, we reformulated this sentence accordingly.

Considered time period

In the most realistic case considered here, for the three-dimensional calculations trajectories are only started for the time period 1 June to 31 July. The monsoon period, however, is longer. The relative weight of the different transport pathways is likely to change over the entire monsoon period. The authors argue that initialization in August is not meaningful as such trajectories would not represent the monsoon season. But why would a trajectory that is initialized in August circulates the monsoon anticyclone and is then exported from the anticyclone in (say) early September not provide information on transport pathways during the monsoon season? In this case, for example, information on the important late period of the monsoon with very likely a weaker transport barrier and thus more export of air from the anticyclone? Possibly, trajectories initialized in August or September do not have to be calculated forward in time for the same length of time as trajectories initialized earlier. In any event, I suggest being more clear about the considered time period when stating the results on the pathways as the importance of particular pathways might change in the course of a season. Also there could be some more discussion on the implications of investigating only the situation for one year; for example, in a strongly ESNO affected year, the results could be different.

We extended the initialization period for trajectories to include August (see above).

Definition of tropics and mid-latitudes

In the paper transport pathways between different regions of the atmosphere are discussed. Of course, an important point here is the definition of these regions. In the definitions used here, the "tropics" extend to 45° N. This is very far north for the northern boundary of the tropics tropics. For example, this definition places Boulder in the tropics (of course sometimes and depending on season there could be tropical air above Boulder). I suggest changing this definition to something more conservative (perhaps 30° N?); in any case the sensitivity of the results presented here on the definition of the tropics should be discussed. Of course, a fixed latitude (whatever the choice) can only be an approximate estimate for the demarcation between tropics and mid-latitudes, which will certainly vary, for example with Rossby wave activity and season.

True, naming the region equatorward of 45°N "Tropics" does seem a little confusing. Our motivation of this choice is to clearly separate air that is located in the anticyclone, or the (sub-) tropical belt from air that is transported across the subtropical barrier. Of course the choice of 45°N is somewhat arbitrary. However, given the location of the subtropical jet (Fig. 1, 15) and the extent of the anticyclone (e.g. as seen by the distribution of trajectories / CO in Fig. 3, 5), we find that 45°N is a reasonable choice. If 30°N was used, a considerable fraction of trajectories that are circulating in the anticyclone would be counted as "transported to the extratropics". Note also that the "extratropical lower stratosphere" is defined as locations above the tropopause, thus given the slope of the tropopause in this region, it will in fact be the "stronger" criteria than the 45°N boundary (see Fig. 15). Note that in the study by Kunz et al (2015), which use PV-based diagnostics of the tropical – extratropical boundary, this boundary lies close to 45°N at 360 K. A sentence is included at the beginning of Section 4.2 to justify the choice of latitudinal boundaries.

Discussion of related studies

In the introduction, the HCN tape recorder signal is explained by the "seasonality" of the Asian Monsoon. Then one would expect a regular tape recorder pattern, which repeats each season (like the water vapor tape recorder). However the HCN tape recorder exhibits an irregular tape recorder signal; attempts have been made to explain the irregularity with the irregularity of HCN sources (Pumphrey et al., 2008; Pommrich et al., 2010). This observation does not exclude a role of the Asian Monsoon in transport of HCN to the stratosphere, but the argument cannot solely rely on the seasonality of the

monsoon.

In reality there is an annual signal in HCN in the tropical lower stratosphere linked to the Asian monsoon (as shown in Park et al, JGR, 2013, their Fig. 14), but the overall signal in the tropics is dominated by interannual changes in HCN sources tied to biomass burning during ENSO events (with maxima during 2004-05 and 2006-07). In order to keep our discussions concise, we weakened the statement by replacing "results" with "likely contributes".

In the discussion (p. 25998) the paper makes the point that the presented results are not in contradiction to the statements in a recent study (Kunz et al., 2015) on exchange between the tropics and extra-tropics. However in the present study, the extra-tropics are defined as poleward of 45°N, whereas Kunz et al. (2015) defined a PV based boundary between the extra-tropics and the tropics (as a function of potential temperature). Thus, in the context of the Kunz et al. (2015) study the 45°N boundary is somewhat arbitrary, at least much of the air considered as "tropical" in the present manuscript would not be counted as "tropical" in Kunz et al. (2015). More importantly perhaps, Kunz et al. (2015) made the point that (at least between 380-420 K) in summer (low) PV streamers constitute an important pathway of transport between the tropics and the extra-tropics. It is thus not obvious in how far the results of Kunz et al. (2015) and the present study are in good agreement. I suggest that in the discussion more emphasis is placed on the accurate vertical and horizontal range as well as summer months that are compared. If there remains a difference to the findings of Kunz et al. (2015) it would be good to more clearly state those.

After reconsidering the comparison with Kunz et al (2015), we realized that the results are rather difficult to compare, and thus the statement that the studies are not in "contradiction" might rather mean that the results are not comparable. First of all, the only comparison that could be made are between the "exchange trajectories" in Kunz et al (their Fig. 9) and our isentropic calculation (Section 3). While we do not think that the different definition of the tropical barrier is a large issue (the PV-based boundary in Kunz et al. at 360 K is close to 45°N, see Figs.9a, 1b in Kunz et al.), the problem with the comparability arises from the different set up of the calculations: Kunz investigates how many "exchange trajectories" associated with PV streamers both equator- and poleward can be found at each isentropic level and season, i.e. they can conclude on where and when streamer activity is relatively strongest or weakest. Our set up only allows to make statements about the relative importance of different pathways of air that originates in the core region of the anticyclone. I.e. the fact that frequent streamer activity is found in the vicinity of the Asian Monsoon anticyclone by Kunz does neither contradict nor agree with our result in that only a few percent of air masses from within the anticyclone is transported to the northern extratropics. Moreover, Kunz et al states that the direction of transport is mostly equatorward (which we won't see), and the trajectories that take part in the transport are not located in the anticyclone core at 360 and 380 K (their Fig. 9a,b), while our trajectories are initially all located in the anticyclone core. We highlighted in the text that the results are not directly comparable, and state only that due to the above mentioned points, the studies are not necessarily in contradiction.

Further, in a recent paper, Vogel et al. (2014) state that the "subsequent long-range transport (8–14 days) [. . .] to the lowermost stratosphere in northern Europe is driven by eastward transport of tropospheric air from the Asian monsoon anticyclone caused by an eddy shedding event". Thus, I do not think that it is correct suggest here that Vo- gel et al. (2014) found that "air is mixed directly from the upper tropospheric anticyclone to the northern extra-tropical lower stratosphere (p. 25998, l. 18-20)".

Unfortunately the formulation we used was mistakable: with "air is mixed directly .." we meant to refer to the fact that the transport occurs quasi-isentropically, as opposed to transport via the tropical stratosphere (that we found to be the more important pathway compared to isentropic mixing). We changed to formulation accordingly.

In the introduction, the study by Orbe et al. (2015) is mentioned; I agree that the findings of this study are relevant for the present paper. I suggest a bit more discussion

of the Orbe et al. (2015) results. More than the equilibrated air-mass fraction, the discussion of the pulse of a conserved tracer (Fig. 5 in Orbe et al., 2015) should be relevant for the trajectory modeling presented here with initial positions in the Asian monsoon circulation in a particular monsoon season.

Thank you for this suggestion, and we included a comparison to the transport pathway shown in Orbe at al. in the discussion. However, comparison to the pulse experiment in Orbe et al is limited, as in Orbe et al transport from the Asian PBL to the lower stratosphere is considered, while we calculate transport (and thus transit times) only from the upper troposphere (360 K) upward.

Finally, a recent paper might be of interest to the discussion in the manuscript. Tissier and Legras (submitted to ACPD, 2015) have investigated the transport between cloud top heights and the 380 K level and insofar are focusing on part of the altitude region (and season) investigated here. The results of Tissier and Legras for the monsoon season seem to be not in contradiction with those reported in the present manuscript, but some discussion might be helpful. For example, the transit times from the top of the convection to the 380 K level for summer (Figs. 4 and 6 in Tissier and Legras) could be compared with the results in presented in Section 5.

Thank you for pointing us to this new study. Both the relevant cloud top heights of 355-365 K and transit times of 20-30 days are approximately in line with our study, and we included the comparison in the Discussion section.

Nabro eruption
The paper now contains a short discussion on the lessons learned from the eruption of the Nabro volcano (Bourassa et al., 2012; Fromm et al., 2014) and I know that in my first review I suggested a discussion of this issue in the paper. However, I am not convinced that this brief discussion demonstrates that the results presented in the manuscript are in approximate agreement with Bourassa et al. (2012). Bourassa et al. (2012) discuss the injection of the Nabro plume at below 14 km or below 355 K, i.e.

below the 360 K level focused on here. And a significant fraction of trajectories initialized at 360 K descends according to the analysis in the paper. Further, the assumed one month conversion time of SO2 to sulphate (not for complete conversion, but for the onset of particle formation being detectable) is rather long. Are there any references for backing up the use of this value? Further, is there evidence from the trajectories considered here that the air in the upper troposphere below 14 km is sufficiently confined in the Asian Monsoon region over a month during upward transport? Beyond the injection of volcanic sulfur at altitudes below 14 km there is also evidence for the Nabro eruption having reached altitudes above 360 K (e.g., Penning de Vries et al., 2014) and it would also be interesting to investigate the transport pathways of this material in the stratosphere after injection. I am not sure what to recommend here. I think either the discussion of the Nabro event should be extended addressing the issues raised above. Alternatively, the authors might prefer to switch back to the first iteration of the paper here and drop the Nabro discussion.

We think that the short discussion of the Nabro eruption is appropriate to include in this paper. The text acknowledges that the main injection likely occurred over 15-17 km (based on Clarisse et al 2014), and the subsequent OSIRIS satellite aerosol observations are consistent with slow upward transport in the monsoon circulation. We have modified the "one month" SO2 to sulphate conversion time scale to "two to four weeks", which is consistent with the stratospheric aerosol plume observed by OSIRIS (18-24 days after the initial eruption). We have included the reference to Penning de Vries et al, 2014. While their results are convincing regarding rapid conversion of SO2 to sulphate aerosol on small scales, the OSIRIS satellite observations still suggest a time scale of 2-4 weeks after the eruption for the emergence of large-scale sulphate aerosol in the lower stratosphere.

Minor issues

- p 25982, l 23: I suggest to distinguish here between "deep stratosphere" and "lowermost extra-topical stratosphere" (as it is done above).

As the calculation in Section 4, the trajectories initialized in low OLR regions at all levels are mostly transported to the tropical stratosphere and "direct" transport to the extratropical LS is of minor importance. We decided to add this information in Section 5 instead of the Abstract to keep it short.

- p. 25983, l 2: "circulation" or "convection"?

While the transport indeed is likely to be driven by convection we decided to keep the word "circulation" here to keep the statement more general (and the transport associated with convection can also be seen as a small-scale circulation).

- p. 25983, l 5: The polar vortex is rather different in size in the northern and southern hemisphere.

True, and we added "northern" (the statement is based on the comparison in Garny and Randel 2014).

- p. 25983, l 11: replace "a HCN" by "an HCN"

Done.

- p 25983, l 18: Is it really "in summer"? I'd argue that it takes time for the monsoon circulation to transport the young air to the lower stratosphere. So the impact of the monsoon will be stronger later in summer than in early summer.

We feel that in is not necessary to distinguish between early and late summer in this more general statement.

- p. 25984, l 6: What is meant by "this transport"? The literature cited above (l. 3, 4) deals with transport from the extra-tropics into the tropics, but air from the interior of the anticyclone will not really participate in transport from the extra-tropics into the tropics.

We exchanged "this transport" with "mixing to the extratropics". While the cited studies might refer to mixing to the other direction, "mixing" by definition means air mass exchange in both directions. Thus, there is a possibility of mixing from the tropics to the extratropics, and anticyclone air might take part in this mixing. This is one of the questions the paper aims to answer.

- p. 25984, l 23: Is the anticyclone really strongest at 360 K? Depends on what is meant by strongest. Not the transport barrier which is stronger at greater heights as you show below. Suggest clarifying and adding a reference.

True, the term "strongest" is not very specific. We meant to refer to the fact that the wind jets maximize close to this level (see Fig.1), and we changed the sentence accordingly.

- p. 25985, l 18: Is there a citation for the employed trajectory model?

No, and the information is added that the trajectory model was implemented for the purpose of this study.

- p. 25986, l 2: What is the resolution in potential temperature in the important range 360-400 K?

The resolution is 10 K, and we added this information to the text.

- p. 25986, l 20: Similar results will be expected for other "normal" years, while ENSO affected years (for example) could have different transport patterns.

True, and we added "..years without unusual conditions" to make this point clear.

- p. 25988, l 3, 4: Have you checked whether (or how well) the trajectories con- serve PV?

No - air masses advected from the anticyclone will likely mix with higher PV air, and thus PV will not be conserved (on a grid-box scale).

- p. 25988, l 16: How well do these PV criteria restrict the initial positions of the

trajectories to the same anticyclonic core region at both 360 K and 380 K? In the polar vortex case, scaled PV (Lait, 1994) has been used.

The PV limit at 380 K has been chosen to represent a similar area, and thus a similar amount of trajectories released as on 360 K. This information is added in the Methods Section.

- p. 25988, l 18: add "at 360 K and at 380 K"

Done.

- p. 25989, l 3: you say "slightly more" here but there are twice as many trajectories at 380 K than at 360 K that reach the northern extra-tropics (8% versus 3%). Reformulate.

True, and we deleted "slightly".

- p 25989, l 27: Why are there no three-dimensional trajectories launched at the 380 K level? Arguably this might be more important for the three-dimensional case than for the (less realistic) isentropic case.

As we are interested in the 3-D calculations in how many trajectories are transported to the stratosphere, an initial height at 380 K, which is close to the tropopause, does not make much sense. Furthermore, we cannot expect convection to transport trajectories as high as 380 K. The information on the confinement of the anticyclone at the 380 K (i.e. the isentropic calculations) is nonetheless relevant for the 3D calculations that start at 360 K: trajectories that are transported upward will pass the 380 K level, and therefore it is relevant how confined they are in this region (see Discussion p. 25998, line 14-16, and Section 5).

- p 25990, l 26: where does the downward transport occur? In the monsoon region or in the tropics in general?

This question is answered in Section 4.2.3: The downward transport occurs in the western part of the anticyclone.

- p 25991, l 9-11: This argument implies that there are trajectories in the western part of the anticyclone and in the eastern part of the anticyclone. But is this true? Is it not the case that the trajectories cycle in the monsoon are covering both the eastern and western part of the anticyclone and are thus sampling both upward and downward velocities?

True, and we reformulated the statement accordingly (see above).

- p 25991, l 18: I do not think that "slightly faster" is the correct wording here; the transit times compared here differ by a factor of two.

True, and we deleted "slightly".

- p. 25992, l 3: How exactly is cross tropopause transport defined?

At each time step, each particle is tested whether or not it lies above the instantaneous local tropopause. We realize that we count trajectories that travel back and forth across the tropopause, but as we are interested in the time integral flux (after up to 60 days) we do not think this is an issue here. Indeed, a test in which we counted "stratospheric" trajectories only as those that remain in the stratosphere for at least 5 days showed that the conclusions are not affected by this different definition of trajectories. We did the same test for the locations of tropopause crossings shown in Fig. 12, and decided to show the distribution for trajectories that are 5 days in the troposphere prior to the crossing, and 5 days in the stratosphere afterward. This does not change the pattern significantly, but sharpens the distribution. We clarified how cross-tropopause transport is defined at the beginning of Section 4.2

- p. 25992, l 15-19: regarding upward and downward transport - does it occur in the monsoon circulation region or outside of this region?

Most of the upward and downward transport indeed occurs within the monsoon region. For upward transport, this is explained in Section 4.2.1 and Fig. 13 and for downward transport see Section 4.2.3 and Fig. 14.

How long have the trajectories to be located in the stratosphere to count as having "entered" the stratosphere?

See above: no such criterion is used (except for the distribution in Fig. 12).

- p 25992, l 8. How are the "tropics" defined?

The definitions of the regions are given in Table 1. We added a sentence on the motivation for the choice of boundaries at the beginning of Section 4.2.

- p 25992, l 29: I do not think that "mixing" is the right word here. In principle, a trajectory does not mix. (Similarly p. 25982, l 13, p 25993, l 14, p 25995, l 3, p. 25998, l 18).

True, the definition of mixing can be tricky. We didn't mean to imply that the air masses that are represented by the trajectory mix, but rather that the trajectory is part of an air mass, that got mixed (here defined as air mass exchange). Thus, we use "mix" where transport occurs which is not in line with mean air advection. Of course we do not show this explicitly, so we changed the term "mixing" to "transport" in most cases, and added "likely by isentropic mixing" in some cases.

- p. 25993, line 12: "travel directly": in a certain time period.

Here, "directly" refers to the location rather than time period. To make this more clear we reformulated the expression to: "from within the upper tropospheric anticyclone region to.."

- p. 25993, lines 20-25: If I understand correctly, the 10% and the 3% in line 25 should add up to the number given in line 20 (12%), but this is not the case.

As stated in the next sentence, 1% are transported downward, thus with 10+3% transported into the region, and 1% transported out of the region, 12% remain there.

- p. 25994, l 1: Not sure what exactly is the point here: What is meant with "to below"? To the troposphere? And if 12% remain "there", what do the other 88% do?

With below, the region below the extratropical LS is meant, i.e. the troposphere (see definition Table 1), and we added "to the troposphere below" to make this point clear. Again the numbers are percentages of all trajectories released, and in the budget of the extratropical LS we only regard those that are transported to this region (i.e. 12%). All other trajectories are transported to other regions, as listed in Table 1.

- p 25995, l 3: Is not the bottom part of the Asian monsoon circulation part of the "tropical upper troposphere"?

The tropical upper troposphere as defined here (see Table 1) is the region in the upper troposphere (250 hPa to Tropopause) which is not the AC region, i.e. it represents the same vertical region but different a different horizontal region. We realize that the box in Fig. 15 is misleading and changed it accordingly.

So is the point here that this smaller fraction of trajectories is detached from the monsoon anticyclone in the upper troposphere and is subsequently transported upward – in the tropics, but outside the monsoon?

Correct.

- p 25995, l 5: replace "amount" by "number"

Done.

- p 25997, l 15, 16: citations for the patterns of CO and ozone?

We added a reference.

- p 25997, l 17, 18: I believe that "not resolved" is too strong. I would argue that some of this transport is resolved, but that the part missing is difficult to quantify. But of course this is the judgment of the authors.

True, the mean effect of convective updrafts is imprinted in the resolved winds, and as the authors state it is difficult to quantify how well this resolved upwelling represents what really happens. However, single convective updrafts are certainly not resolved,

and thus we decided to leave the text as is.

- p. 25997, l 21-24: This sentence is confusing, the first part of the sentence seems to contradict the second part.

True, and we reformulated the sentence accordingly.

- p 25997, l 26: You could add some further citations here for "eddy shedding" (for example Hsu and Plumb, 2001).

We added the reference to Popovich and Plumb (2001) here, as this paper focuses on observations mentioned in this context, while Hsu and Plumb focuses on conceptual model results.

- p 25998, l 2: according to this definition the northern extra-tropics are located poleward of 45°N, isn't this to far north for a boundary between tropics and extra- tropics?

See response to main comment above.

- p. 25998, l 10: add "than on the 360 K level (x%)" if this is the point here. Also, you mean "on" the level, not "when in initialized at the . . . level" – is this correct?

Correct and done.

- p. 25998, l 25-29: If the divergence is located at approximately the correct alti- tudes in the reanalysis products, then also the deep convective transport should be relatively well represented in the reanalysis. (See comment above). Also, I think there are reports in the literature of a somewhat successful reproduction of trace gas profiles in the upper troposphere based on such reanalysis data.

True, this is a hint that the mean convective transport is represented well in the reanalysis.

- p 26000, l 3: Is the fact that diabatic transport delivers a larger fraction of the trajectories to the stratosphere significant? By repeating this point here in the summary, the authors seem to imply that it is. So can we consider this as an advantage of diabatic transport?

The differences between kinematic and diabatic transport calculations are highlighted here as they are a measure of the uncertainty, and therewith important. We moved this discussion to the new Section 6.

- p 26000, l 7: which "region" are you referring to? Unclear.

As stated in the text we refer to the "region of the anticyclone".

- p 26000, l 9: The fact that diabatic heating rates are different between analyzes was also made by Randel and Jensen (2013).

Reference added.

- p 26000, l 10: Citation for this ERA feature?

We shifted the Wright citation to the end of the sentence to make clear that this is a results of this paper.

- p 26000, l 10, 11: It is up to the authors, but I do not think it is a nice way to end the paper with such a negative statement. The issue of different reanalysis products (an important one, I agree) could be discussed earlier in the paper.

True, and the main discussion of uncertainties is shifted to Section 6, and only a short paragraph summarizing this discussion is added to the last Section.

- Fig 1: could you add a line for zero heating level?

We prepared a version of this Figure adding the line of zero total heating and schematic information on up-and downwelling regions. However, we decided that the Figure is too crowded and the information added in this way not necessarily worth showing (as geographical regions of up- downwelling are more evident from Fig. 9, and profiles from Fig. 16).

- I think it would be useful to show Fig. 6 also with potential temperature as vertical coordinate.

In general, we agree that this would be an interesting addition. However, in the light that we have a large number of Figures already, and that this Figure would not add very much relevant information (the distributions in Theta coordinates are shown in Fig. 8), we decided against including such a Figure.

- Fig. 12: How sensitive is this analysis to short term trajectory fluctuations around the tropopause? Could this lead to artifacts in tropopause crossing patterns?

See comment above: We tested this sensitivity and decided to limit the distribution to trajectories that remain in the stratosphere for at least 5 days (and were in the troposphere 5 days prior to the crossing). This limitation does not change the location of the maxima of the distribution, but leads to a sharper distribution.

- Fig 16: dashed line is mentioned but is not clearly visible as a dashed line.

Fixed.

- Acknowledgments: perhaps you want to mention ECMWF here for ERA-I reanal- ysis data.

Thank you for reminding us to include ECMWF.

Reviewer 4

The paper of Garny and Randel provides a very interesting quantification of the budget of transport from the Asian monsoon anticyclone to the lower stratosphere. The analysis, based on lagrangian calculations driven by atmospheric reanalysis, indicates that air parcels trapped inside the Asian anticyclone follow a direct parthway to the lowermost stratosphere. An interesting point of the paper is the use of both diabatic heating and explicit vertical velocity to evaluate 3D transport. The authors put their conclusion in a wider perspective, integrating similar studies based on trajectories already published and hence provide a nice additional information. The paper is well written and structured, sufficiently coincise although some figures may be grouped:17 of them may be a too large number. I consider that this paper could be a good contribution to ACP, but authors could consider to address several issues. Most of them concern doubts on the methodology that could weaken the robustness of the results. The paper is built in a reanalysis world and uncertainties on the trajectory calculation may affect the results. In general I consider necessary to revise and improve that as described below to consolidate the outcomes.

1./ A single cluster is composed of 1000-2000 trajectories. The number of air parcel seems small compared to similar approaches. Lagrangian calculations are computational cheap and the same analysis could have been done with a much larger number of parcels. Moreover, authors provides statistics of small fractions (2-3 from the UT AC to ExTropic LS) meaning on average few tenths of single parcels. Is this significant ? A reasonable option could be to check the results with a larger num- ber of parcels. Moreover this may reduce errors due to the long trajectory runtime (60 days).

In response to this comment, we performed a sensitivity run with 10x as many trajectories, and found that results are nearly identical (even the timeseries of fraction of trajectories for each individual starting day) – see above and new Section 6 of the paper.

2./ The results are based on a single month (July 2006). Despite the fact that 2006 may be considered an "average" year for Monsoon conditions, it would be necessary - at least - to extend the analysis to the whole summer season (June to September) to account for intraseasonal variability. This would imply additional simulations but, as said before, this won't be too heavy from a computational point of view.

We extended the initialization period for trajectories to include August, i.e. capture now the whole period June - August (see above).

3./ The cluster is limited to 15°N. This is clearly visible in figure 2, top left panel and 4 where initial distribution is cut south of 15°N while being still inside the anticyclone. This is annoying and may have a negative bias on the estimate on the equatorward transport. Authors should consider to re-run the trajectories extending the domain southward.

The trouble with the choice of the equatorward boundary of the anticyclone based on low PV (as used here) is in the separation of low PV air from within the anticyclone to "background" low PV air in the tropics. The choice of 15°N is of course somewhat arbitrary and neglects anticyclonic air in some cases. However, if using a boundary more southward, in other cases air would be included that is clearly not within the anticyclone. To test the sensitivity how relevant the initial positions of the trajectories are for their final distribution we can regard the sensitivity runs in Section 5, where the initial positions are based on low OLR values (in the region 0-110 E and 0-45 N, see Section 2). The mean initial distribution is shown in Fig. 1 of this response (and can be compared to Fig. 3 in the paper). While trajectories are started in the mean more southward in the OLR case, the end result of transport to the stratosphere is very similar (see new Section 6 of the paper).

Additional points:

4./ The authors estimate the fluxes in terms of fractions of number of parcels. This may be misleading since mass (or conversely volume) is not conserved with this type of lagrangian calculation where no density changes and diffusion are applied. I understand that do this would imply the use of a different model but a careful consideration of these aspects should be included in the paper.

True, given a trajectory represents a certain fixed mass, it will represent a different volume at other locations in the atmosphere. The statement that can be made based on this definition is how many air parcels (i.e. mass) from the upper tropospheric anticyclone travel to other defined regions of the atmosphere, i.e. the relative importance of different pathways. Of course one cannot conclude on how much these transported air parcels contribute to the mass (or volume) in the destination region – but as this point is not the topic of the paper, we don't think this is an issue.

5./ I have some doubts on to me the need to include isentropic calculations with respect to the overall conclusions of the paper. Authors bases on the isentropic approach the comparison with results obtained with MERRA and Era-interim re-analyses. This to desantangle the possible role of vertical velocities. Nevertheless, the comparison is mostly portrayed. Resolution of the meteorological fields is also different (!) and this is just considered as a factor to explain why results are different without a through discussion. If a comparison should be included here it would be important to investigate how Asian anticyclone is seen in two reanalyses, if and how winds differ. A possible option is to drop this part.

We agree with the reviewer in that the comparison of isentropic trajectory calculations between ERA-Interim and MERRA do not contribute to the conclusions of the paper, and decided to drop the MERRA results (and reduced former Fig. 4 to the mean latitudinal distribution, that is now added to Fig. 3). However, we keep the results on isentropic trajectories based on ERA-Interim. These results reveal how confined the anticyclone is on an isentropic level, which is important to understand the three-dimensional transport (i.e. time scales of horizontal confinement versus vertical transport, see Discussion Section).

6./ The paper draws its conclusion in an "ERA-interim" world as said above but it would be important to discuss uncertainties and differences in the main variables that could differ with respect to reality and have large uncertainties. More specifically it would be important to further detail on: - How realistic tropopause height is with respect to observations (see Pan et al., 10.1002/2013JD020558). A sensitivity test with an higher

TP height (if a bias is found) would be probably interesting. - Possible impact of the uncertainty on heating rates (Wright and Fueglistaler, 2013) on the results. As above, a sensitivity test with biases on the vertical motion would be an ideal solution.

We extended the discussion of uncertainties and biases in the ERA-Interim reanalysis (in particular in heating rates) in the new Section 6 of the paper. We are not sure how the study by Pan et al relates to tropopause biases in ERA-Interim; perhaps the reviewer is thinking of Pan and Munchak (10.1029/2010JD015462), where biases in tropopause heights from GFS and GEOS5 are discussed. In any case, we feel a detailed analysis of biases in ERA-Interim tropopause heights is beyond the scope of this paper We took up the suggestion of sensitivity tests with respect to heating rates. While it is difficult to estimate the biases (or rather uncertainty, as the "truth" is not known) on the full 3-D field of heating rates, we did a simple test by reducing the heating rates by some percentage number (50%). Results are discussed in the new Section 6.

As last minor point, I agree with reviewer 2 that the discussion of the volcanic ashes transport mechanism is marginal for this paper that is fully consistent. I suggest not to insist on it.

We still believe that the short discussion of the Nabro volcanic eruption is a useful inclusion for this paper.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C11007/2016/acpd-15-C11007-2016-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 25981, 2015.

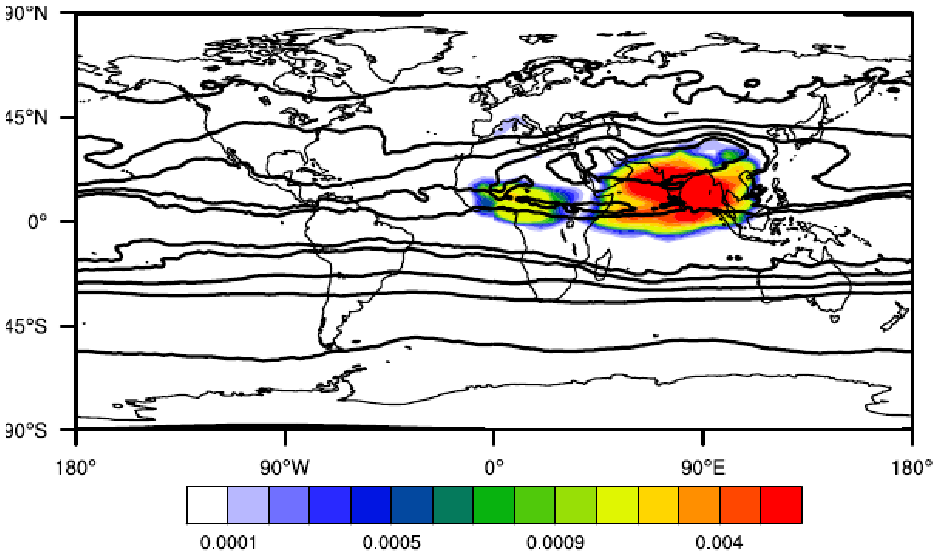

Fig. 1. Mean initial distribution of trajectories released within regions of low OLR.