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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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General

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 assumptions 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 distinguished 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?

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.

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).

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.

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.

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 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.

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 Vogel 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)'.

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.

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.

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 SO_2 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.

Minor issues

- p 25982, l 23: I suggest to distinguish here between ‘deep stratosphere’ and ‘lowermost extra-topical stratosphere’ (as it is done above).
- p. 25983, l 2: ‘circulation’ or ‘convection’?
- p. 25983, l 5: The polar vortex is rather different in size in the northern and

southern hemisphere.

- p. 25983, l 11: replace ’a HCN’ by ’an HCN’
- 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.
- 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.
- 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.
- p. 25985, l 18: Is there a citation for the employed trajectory model?
- p. 25986, l 2: What is the resolution in potential temperature in the important range 360-400 K?
- p. 25986, l 20: Similar results will be expected for other ‘normal’ years, while ENSO affected years (for example) could have different transport patterns.
- p. 25988, l 3, 4: Have you checked whether (or how well) the trajectories conserve PV?
- 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.
- p. 25988, l 18: add ‘at 360 K and at 380 K’

- 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.
- 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.
- p 25990, l 26: where does the downward transport occur? In the monsoon region or in the tropics in general?
- 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?
- 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.
- p. 25992, l 3: How exactly is cross tropopause transport defined?
- p. 25992, l 15-19: regarding upward and downward transport – does it occur in the monsoon circulation region or outside of this region? How long have the trajectories to be located in the stratosphere to count as having ‘entered’ the stratosphere?
- p 25992, l 8. How are the ‘tropics’ defined?
- 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).

- p. 25993, line 12: ‘travel directly’: in a certain time period.
- 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.
- 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?
- p 25995, l 3: Is not the bottom part of the Asian monsoon circulation part of the ‘tropical upper troposphere’? 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?
- p 25995, l 5: replace ‘amount’ by ‘number’
- p 25997, l 15, 16: citations for the patterns of CO and ozone?
- 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.
- p. 25997, l 21-24: This sentence is confusing, the first part of the sentence seems to contradict the second part.
- p 25997, l 26: You could add some further citations here for ‘eddy shedding’ (for example Hsu and Plumb, 2001).
- 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?

- 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?
- p. 25998, l 25-29: If the divergence is located at approximately the correct altitudes 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.
- 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?
- p 26000, l 7: which ‘region’ are you referring to? Unclear.
- p 26000, l 9: The fact that diabatic heating rates are different between analyzes was also made by Randel and Jensen (2013).
- p 26000, l 10: Citation for this ERA feature?
- 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.
- Fig 1: could you add a line for zero heating level?
- I think it would be useful to show Fig. 6 also with potential temperature as vertical coordinate.
- Fig. 12: How sensitive is this analysis to short term trajectory fluctuations around the tropopause? Could this lead to artifacts in tropopause crossing patterns?

- Fig 16: dashed line is mentioned but is not clearly visible as a dashed line.
- Acknowledgments: perhaps you want to mention ECMWF here for ERA-I reanalysis data.

References

Bourassa, A. E., Robock, A., Randel, W. J., Deshler, T., Rieger, L. A., Lloyd, N. D., Llewellyn, E. J. T., and Degenstein, D. A.: Large Volcanic Aerosol Load in the Stratosphere Linked to Asian Monsoon Transport, Science, 337, 78–81, doi:10.1126/science.1219371, 2012.

Dee, D. P., Uppala, S. M., Simmons, A. J., Berrisford, P., Poli, P., Kobayashi, S., Andrae, U., Balmaseda, M. A., Balsamo, G., Bauer, P., Bechtold, P., Beljaars, A. C. M., van de Berg, L., Bidlot, J., Bormann, N., Delsol, C., Dragani, R., Fuentes, M., Geer, A. J., Haimberger, L., Healy, S. B., Hersbach, H., Holm, E. V., Isaksen, L., Kallberg, P., Koehler, M., Matricardi, M., McNally, A. P., Monge-Sanz, B. M., Morcrette, J.-J., Park, B.-K., Peubey, C., de Rosnay, P., Tavolato, C., Thepaut, J.-N., and Vitart, F.: The ERA-Interim reanalysis: configuration and performance of the data assimilation system, Q. J. R. Meteorol. Soc., 137, 553–597, doi:10.1002/qj.828, 2011.

Fromm, M., Kablick, G., Nedoluha, G., Carboni, E., Grainger, R., Campbell, J., and Lewis, J.: Correcting the record of volcanic stratospheric aerosol impact: Nabro and Sarychev Peak, J. Geophys. Res., 119, 10,343–10,364, doi:10.1002/2014JD021507, http://dx.doi.org/10.1002/2014JD021507, 2014.

Hsu, C. J. and Plumb, R. A.: Nonaxisymmetric Thermally Driven Circulations and Upper-Tropospheric Monsoon Dynamics, J. Atmos. Sci., 57, 1255–1276, 2001.

Kunz, A., Sprenger, M., and Wernli, H.: Climatology of potential vorticity streamers and associated isentropic transport pathways across PV gradient barriers, Journal of Geophysical Research: Atmospheres, 120, 3802–3821, doi:10.1002/2014JD022615, http://dx.doi.org/10.1002/2014JD022615, 2014JD022615, 2015.

Lait, L. R.: An alternative form for potential vorticity, J. Atmos. Sci., 51, 1754–1759, 1994.

Orbe, C., Waugh, D. W., and Newman, P. A.: Air-mass origin in the tropical lower stratosphere: The influence of Asian boundary layer air, Geophys. Res. Lett., 42, 2015GL063937, doi:10.1002/2015GL063937, http://dx.doi.org/10.1002/2015GL063937, 2015.

Penning de Vries, M. J. M., Dörner, S., Pukite, J., Hörmann, C., Fromm, M. D., and Wagner, T.: Characterisation of a stratospheric sulfate plume from the Nabro volcano using a combination of passive satellite measurements in nadir and limb geometry, Atmos. Chem. Phys., 14, 8149–8163, doi:10.5194/acp-14-8149-2014, http://www.atmos-chem-phys.net/14/8149/2014/, 2014.

Ploeger, F., Konopka, P., Müller, R., Fueglistaler, S., Schmidt, T., Manners, J. C., Grooß, J.-U., Günther, G., Forster, P. M., and Riese, M.: Horizontal transport affecting trace gas seasonality in the Tropical Tropopause Layer (TTL), J. Geophys. Res., 117, D09303, doi:10.1029/2011JD017267, 2012.

Pommrich, R., Müller, R., Grooß, J.-U., Günther, G., Konopka, P., Riese, M., Heil, A., Schultz, M., Pumphrey, H.-C., and Walker, K. A.: What causes the irregular cycle of the atmospheric tape recorder signal in HCN?, Geophys. Res. Lett., 37, L16805, doi:10.1029/2010GL044056, 2010.

Pumphrey, H. C., Boone, C., Walker, K. A., Bernath, P., and Livesey, N. J.: Tropical tape recorder observed in HCN, Geophys. Res. Lett., 35, L05801, doi:10.1029/2007GL032137, 2008.

Randel, W. and Jensen, E.: Physical processes in the tropical tropopause layer and their role in a changing climate, Nature Geoscience, 6, 169–176, doi:10.1038/ngeo1733, 2013.

Schoeberl, M. R., Dessler, A. E., and Wang, T.: Simulation of stratospheric water vapor and trends using three reanalyses, Atmos. Chem. Phys., 12, 6475–6487, doi:10.5194/acp-12-6475-2012, 2012.

Vogel, B., Günther, G., Müller, R., Grooß, J.-U., Hoor, P., Krämer, M., Müller, S., Zahn, A., and Riese, M.: Fast transport from Southeast Asia boundary layer sources to northern Europe: rapid uplift in typhoons and eastward eddy shedding of the Asian monsoon anticyclone, Atmos. Chem. Phys., 14, 12 745–12 762, doi:10.5194/acp-14-12745-2014, 2014.

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