Atmos. Chem. Phys. Discuss., 15, C9200–C9202, 2015
www.atmos-chem-phys-discuss.net/15/C9200/2015/

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

Anonymous Referee #4

Received and published: 12 November 2015

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 % for air parcels 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 number of parcels. Moreover this may reduce errors due to the long trajectory runtime (60 days).

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.

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.

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.

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.

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.

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.

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