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Interactive Comment

Interactive comment on "Transport pathways of peroxyacetyl nitrate in the upper troposphere and lower stratosphere from different monsoon systems during the summer monsoon season" *by* S. Fadnavis et al.

Anonymous Referee #1

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This paper uses the global chemistry-climate model ECHAM5-HAMMOZ, PAN retrievals from MIPAS-E, and aircraft observations during the monsoon season (June-September) to evaluate the transport pathways of PAN, NOx, and HNO3 from various monsoon region to the upper troposphere lower stratosphere (UTLS). The model results suggest that three monsoon regions - the Asian summer monsoon (ASM), the North American Monsoon (NAM), and the West African monsoon (WAM) - contribute to pollution in the South Asian UTLS. The authors also investigate the impact of lightning NOx on the distribution of these species in the UTLS, and find that the PAN in the

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UTLS over the ASM region is primarily anthropogenic rather than from lightning NOx.

This paper is on an important topic and the authors have extensively analyzed their results. The lightning piece of the study is a fairly straight-forward model experiment to quantitatively assess the impact of lightning NOx production on O3 and NOy species in the UTLS. However, the main part of the paper is more confused, and it is not clear to me that it presents any new results or information on the transport of PAN from the boundary layer to the UTLS, which is supposed to be the main topic of the paper.

P15109 L10-15 seems to be the clearest statement of the paper's conclusions, saying that pollution from North America and Europe merges with the ASM plume in the troposphere before being lofted into the UTLS. This is a true statement, but trivially so, as it is practically a restatement of the general circulation of the atmosphere. One of the advantages of a model study is that you can use the model to determine the relative importance of different factors that are difficult to separate in observations of the real atmosphere, as in the lightning NOx study in this paper. For example, if this study had used the model to estimate the relative contribution of pollution from the US, Europe, and Asia to NOy species in the UTLS over the Asian monsoon region, that would be an interesting and publishable result. As it stands, the paper goes to a lot of effort to demonstrate a qualitative result that seems obvious to me.

The MIPAS-E data could also have been used to test if the model circulation is correct, and to determine how it needed to be corrected. For example, Figures 4d and 4e show major qualitative and quantitative differences in the distribution of PAN in the UTLS between the model and the MIPAS-E observations. The model could have been used to investigate if these differences are due to incorrect emissions, chemistry, or transport and thereby provide new information on the transport of PAN. Instead, while the differences between the model and observations are extensively discussed, they are explained as likely a consequence not of errors in the model, but of sampling issues with the MIPAS-E data due to clouds in convective regions, and no effort is made to screen the model results to mimic the MIPAS-E sampling and account for this effect.

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Again, it is not clear to me what we have learned from these model-measurement comparisons that we didn't already know before.

I also don't understand why the model wasn't run for the time period corresponding to the MIPAS-E dataset. The ECHAM5-HAMMOZ simulations were run using monthly mean sea surface temperatures sea ice cover data from the years 1995-2004 with anthropogenic and biomass burning emissions for the year 2000. However, the MIPAS-E data is for 2005-2012, so that there isn't a single year of overlap between the modeled period and the observations. The authors make clear that they know that changes in emission can have a significant impact on NOy species in the UTLS, but never make clear why they didn't set up their model runs to correspond to the observations they intended to use in validation, or why they used observations that they think have substantial sampling biases in their regions of interest.

Finally, the paper frequently gives quantitative estimates of the model bias relative to the aircraft and MIPAS-E data but does not give sufficient information on the location that the authors are referring to. For example, P15106 L8-9 says that PAN is underestimated over North and South America in Figure 6f, without noting that there is a significant model overestimate at 30 N between 8 to 10 km in altitude. There are several other statements in the paper that need to be made more specific before they can be evaluated.

Although I recognize and appreciate the substantial amount of work the authors have put into their study, I do not see how it significantly adds to our knowledge of PAN transport by monsoon convection, and I have substantial concerns about the methodology as noted above. The amount of work needed to address these concerns is more significant that can be expected in a manuscript revision, so I recommend rejecting the manuscript.

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