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Atmospheric Chemistry and Physics
Discussions

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 #2

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It should be said that the manuscript seems to be a resubmission of a prevously paper published in ACPD in 2014 http://www.atmos-chem-phys.net/14/12725/2014/acp-14-12725-2014.html that was not further published in ACP. Two versions do not show substantial differences and no indications are given in how the pertinent remarks of the reviewers in 2014 were addressed.

The 2015 paper from Fadnavis et al addresses the role of transport on the UTLS concentrations of PAN based on a 10-years CCMI simulation. The topic is certainly inter-

esting and there is a huge amount of information on different issues that, however, is not treated sufficiently in deep; this is a crucial weakness since the focus of the paper is smeared out and the analysis should be improved. Below a series of major and other issues that should be addressed.

- This paper shares a non marginal fraction of information with a previously published paper from the same authors (http://www.atmos-chem-phys.net/14/12725/2014/acp-14-12725-2014.html). Especially concerning the comparison with satellite and aircraft observations. The role of deep convection on PAN distribution (that is obvious) is mentioned in the companion paper and it should be essential to clearly state what is really new.

- The comparison with satellite data is misleading. As mentioned by the other reviewers the model n covers a different period with respect to the MIPAS database and that attribution of differences and biases is not sufficiently addressed and remains speculative. Authors claim that this simulation should be considered as a mean climatology but two points should be clarified: The simulation is driven by 1995-2004 SSTs and year 2000 emissions and hence is representative of a specific period for climatic variability and chemical regimes. The simulation is 10-years long and it should be clearly demonstrated that a relatively short period could be sufficient to include the necessary variability

- The comparison with aircraft data has several weaknesses as well. A point-to-point correlation is to my opinion inappropriate while comparing sporadic observations (as the in-situ ones) that can be biased by specific flight strategy and a coarse simulation that should be representative of mean large scale conditions. It is necessary to define appropriate diagnostics (as done for instance in several CCMVAL analysis) to extrapolate from the aircraft database some synthetic and representative data to be compared to a CCM run (i.e. mean profiles, tracer-tracer correlations, latitudinal transects).

- Since the main focus of the paper is the role of transport and hence an evaluation of the model dynamics or at least of Monsoon(s) circulation should be done. This can be done evaluating the mean Monsoon annual cycles, the extent of Monsoonal circulation at the ground, the distribution of deep convection. The same applies for the Asian Anticyclone discussed in P15104 (see detailed points) Moreover, it should be discussed how vertical velocities in a large scale model can be used to infer rapid uplif in deep convective regions.

- Most of the discussion refers to the role of deep convection in different regions. The fact that authors treat PAN is a secondary issue since this is considered as a proxy for NOx tropospheric emissions. So, it would be desirable to coupe that to PBL tracers (as done in many studies) to desantangle the role of emissions from different regions. Moreover, the discussion remains often speculative and sometimes misleading , as for example in the case of overshooting convection that is often onvoked in the text. I agree that this may eventually play a role in the LS chemical budget but I have some doubts that this could be seen in satellite data (due to their coverage / vertical resolution) and it is certainly impossible to consider it with a T42 CCM.

- The role of horizontal vs vertical transport in UTLS is certainly fundamental and is a focus of a vast literature. Here no real information is added and, again, the simulation strategy cannot really help. It would have been important (as mentioned above) at least to make use of different geographical PBL tracers.

- The sensitivity analysis to lightning production is certainly interesting but is somewhat disconnected to the main core of the paper and appears to be still in a rough form. As mentioned earlier it would be necessary to conduct other types of sensitivity studies.

- The discussions are often lenghty with unnecessary details on well-known items and english form may be accurately revised throughout the paper

- In addition to that I fully share the remarks of the 2 reviewers of the first version of the paper (that to my opinion are still valid here) and the evaluation of reviewer 1.

Other issues

p15098L8 The production of PAN in deep convective regions is not so striking. The high values at 16 km height may be better discussed. Model coarse resolution may play a role here. It is also not clear how to address the role of tropopause folding. The last sentence of the paragraph is not clear to me.

P15099L4 What is a coherent location ? Are Aircraft data aggregated in some ways ?

P15099 section 3.1 The database of PAN seems to be limited to few points. In addition to the main issue raised before, it seems unappropriate here to calculate any correlation or statistical significance test.

P15102L21 I would not use WAM (West African) to identify land convection in Africa south of the Equator

P15103L3 It is not straightforward that strong vertical winds are responsible for a strong transport in the UTLS. The question of the wind intensity (not mentioned in the paper) and vertical resolution is crucial here.

P15104L3 to 16 The whole discussion is confusing and not sufficiently robust. The evaluation of the Asian Anticyclone would need a per-se evaluation. I cannot see a region of mixing and the discussion on the role of tropical heating in generating the “Gill-type” dipole is uncomplete and useless here.

I consider that there are too many crucial revisions to foresee a possible publication in ACP.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 15087, 2015.