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Interactive comment on "Sub-seasonal Variability in the Boundary Layer Sources for Transport into the Tropopause Layer in the Asian Monsoon Region" *by* Bin Chen et al.

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Review of "sub-seasonal variability..." by Chen et al., 2017

General comment:

This paper deals with the origin of BL air masses that cross the tropopause during the Asian Summer Monsoon with a focus on its sub-seasonal variabilty. It is based on the analysis of 13 years of Lagrangian modelling. The subject of this paper is relevant to ACP and the results could bring some interesting and new information. Nevertheless, the paper is not yet fully convincing and the authors should adress some major issues before publication. These issues detailed below concern both the methodolgy and the results. Concerning the methodology, the choices made for the Lagrangian modeling should be discussed: use of kinematic versus diabatic trajectories on top of the con-

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Discussion paper

vective outflow; uncertainties related to the use of ERA-Interim reanalysis. Relative to this particular point, the large discrepancies between the present study and Chen et al. (2012) should be adressed. Concerning the results, the authors use many statistical tools such as wavelet or EOF decomposition but the methodology is not detailed enough, their added value is not straightforward and their analysis is confusing. Therefore, the presentation and the analyses of the statistical results should be improved or some parts disgarded as not conclusive.

Detailed comments:

Methodology:

1/ Lagrangian modeling: The 3D Lagrangien particle transport and dispersion computation presents some limitations that are not discussed: - The use of ECMWF ERA-Interim reanalysis may be responsible for systematic biases. In many other studies different sets of reanalyses are used and the results are compared to determine the uncertainties and strenghten the conclusions. For instance Bergman et al. (2013) compare results of trajectory calculations from ECMWF, NCEP/GFS and MERRA. They show that the analysis used do not change their conclusions. The authors of the present study have already used NCEP/GFS reanalyses to deal with the same problematic for the 2001-2009 period (Chen et al., 2012). It should therefore be possible to discuss the differences based on this previous study for the common period (2001-2009). - For vertical transport above the altitude of convective outflow, the trajectories are computed with vertical velocities from the reanalyses. A number of studies dealing with the same subject are based on vertical transport computed from radiative heating rates (Tissier and Legras, 2015, Garny and Randel, 2016). For instance, Garny and Randel show that kinematic calculations are responsible for larger vertical dispersion of the trajectories and that "diabatic calculations result in more trajectories traveling to higher levels that lie well above the tropopause". It could be interesting to discuss the impact of your choice to use kinematic trajectories on your results and conclusions. - The OLR are used to "represent the strenght of convection..." It is not clear wether they are used to trigger convection in the model or simply to characterize convection as an external source of information. In the second case it is not obvious that the model triggers convection in good coincidence with convective activity derived from the OLR. Are there references for such a coincidence concerning FLEXPART? Otherwise is it posssible to give such evidence?

2/ ASM anticyclone boundaries: - The study is dedicated to "transport from the BL to the TL over ASM anticyclone region" and "BL-to-TL trajectories are selected and rigorously defined as those trajectories .... that then cross the tropopause within the ASM anticyclone". Nevertheless, the boundaries of the anticyclone are not defined rigorously nor used quantitatively. The computations are made with the anticyclone region defined as the 0-50°N and 20-160°E box instead. The longitudianl boundaries are more or less correct but the latitudinal boundaries are much too large. The AMA is centered around 25°N and extends roughly south to 15°N and north to 40°N. A 0°N south boundary probably leads to an overestimation of the imapct of southern regions such as the southern part of IN, BB, SC and more importantly from the WP box. Furthermore, the AMA is obviously not a rectangle box but an ellipsoid with boundaries undergoing a strong sub-seasonal variability as recognised in the manuscript p3l28-p4l1-6 (see also Popovic and Plumb et al., 2002, Ploeger et al., 2015). Therefore, the subseasonal variability of the origin of air masses crossing the tropopause within the AMA may vary also with the AMA variability itself. Recent studies have tried to determine the dynamical boundaries of the AMA. For instance Ploeger et al. (2015) have developped a sophisticated PV-Based criterion to follow the AMA dynamic and Barret et al. (2016) a simplified criterion based on geopotential height anomalies. Some test on limited periods (one season for instance) could be made with dynamical AMA boundaries to evaluate the impact of the AMA dynamics on the results. If the authors choose a rectangular box, they should not state "within the ASM anticyclone" but "within the region that encompasses the ASM anticyclone". In which case the southern boundary of the domain has to be revised to at least 15°N.

3/ Pollution transport: - The authors state many times that a tracer-independent Lagrangian modeling is needed to characterize the origin of the air masses affecting the UTLS composition and that pollution tracer studies are weighting the results towards polluted regions. The UTLS composition is affected by the transformation of species (gases and aerosols) emitted by anthropogenic, fire or natural sources which are region dependent. Therefore, tracer-independent Lagrangian modeling is not the best method to characterize regions that affect the most the UTLS composition. The afore mentioned statement should be modified accordingly. Another example is p14l13-14 : "the low level... led to inappropriate conclusions". This statement is exagerated and also needs to be modified. Conclusions of CO-based studies are appropriate to determine where CO and associated pollutants come from and therefore which regions are significantly contributing to modifications of the composition such as enhancements of CO, O3 or aerosols in the AMA. TB is not one of them due to the lower level of pollutants compared to N India and the southern Himalayan slopes. Of course, uplifted air masses from the TB or WP or BB modify the UTLS composition because they are wetter or O3-depleted. Inappropriate should be replaced by complementary.

4/ Statistics analysis methods: The reader needs more detailed explanations of the statistical methodology. The "BL sources anomalies . . . substracting May-June-July means from the total field...". Does this means that the May-June-July are substracted from the April 15 to August 31 ? Such a difference is not an "anomaly" but a kind of seasonal mean. Anomalies are normally defined as oultliers from means. Furthermore, the mean is not really seasonal and the choice of May-June-July is not easy to understand because the monsoon generally starts in June and ends in September. The authors use EOFs and their PCs but more details, references should be given about the calculation and signification of EOFs and Pcs. What is the meaning of high and low Pcs? What is the difference between a high and a low PC relative to the BL to TL transport?

Results:

1/ Comparison with Chen et al. (2012): - p10l1-2: It is stated that "the WP . . . is less important than in our earlier study (Chen et al. (2012)". It is the less that can be said! Fig. 2 from the present paper and Fig. 3 from Chen et al. 2012 show completly different structures. The maxima are located over northern equatorial Indian Ocean (the most important maximum), BB, South China Sea and WP for Chen et al. (2012). Here they are located over India and over the TP. Equatorial Indian Ocean has disappeared and the density over WP is 2 to 3 times less important than over IN and TP! These discrepancies should be described and discussed more thoroughfully in the present study. Indeed, the hypotheses given are rather light to explain such discrepancies: - the studied domain is smaller here (ASM region) than in Chen et al. (2012): here 0-50°N and 20-160°E and in Chen et al. (2012) 0-60°N and 0-160°E. The region north of 50°N has probably little impact on TST or BL-to-TL transport. The part west of 20°E encompasses Africa and could have some importance in the maxima over the equatorial Indian Ocean. Nevertheless, during the ASM transport is rather from Asia to Africa via the Tropical Easterly Jet, and no maxima is present over India and the TP in Chen et al. (2012). - Chen et al. (2012) have used NCEP/GFS analyses while the present study is based on ECMWF ERA-Interim reanalyses. As suggested above, a comparison should be made between (i) the wind fields from both datasets (ii) the trajectory distributions even though it should not change much from the difference between Chen et al. (2012) and the present study. Nevertheless, according to Bergman et al. (2013) the use of different analyses do not change radically the results of Lagrangian simulations. For instance, using ECMWF/NCEP and MERRA, the contribution of fresh air masses within the AMA varies from 35 to 38

2/ Use of statitiscal tools: - p10l15-26 and Fig1a/b: The CV and variance maps do not bring much information. The first one, weighted by the inverse of the density from Fig. 2 is close to its negative picture. The second one closely follows the density map of Fig 2. The text relative to these plots could be shortened and the plots could be removed. - From the analysis of the wavelet spectra (Fig. 7), the authors determine marked 10-20 and 30-60 days peak for the variability of the anomaly for the TP. The

90 days peak that probably correponds to the seasonal variability is clear but instead of marked peak, Fig. 7 rather shows a broad continuum from 10 to 60 days. For the IN region, the 30 days peak is much clearer. - The analysis of the spatial patterns of the sub-seasonal variability with the EOF is rather confusing. The analysis of the EOF focuses on the three leading EOF that explain less than 40- In 3.3.2, the authors aim at characterizing "the relationship between sub-seasonal variability and atmospheric composition" through composite analysis (p16-18). The aim is interesting but I find this part of the paper particularly confusing and I think it should be largely improved (starting by a better explanation of the method as required above). - The authors "hypothesized" that the difference between conditions corresponding to high and low PCs explain the controling mechanism of transport. The basis of this relationship is unclear to me and should be developped further. - PC1 and PC3 high – low wind composites are characterized by North easterlies and PC 2 by Westerlies. What is the meaning of these patterns in terms of monsoon weather variability and what is the link with convection ? In what sense are the composite explaining the BL-to-TL transport sub-seasonal variability are they for instance linked to enhanced or decreased transport? Why? - high CAPE and low OLR are indicative of deep convection activity. Nevertheless, the composite maps of Fig. 9, 10 or 11 do not display a clear coincidence of both. The CAPE pattern presents a very localised band of high values over the BB along the Indian east coast and the lowest OLR are over a large domain of the BB. Could the authors discuss this point?

3/ Interannual variability: The interannual variability displayed in Fig. 3a is rather strong with some years showing some values largely on top of the others. The authors comment about this plot is "This annual variability is presumably associated with... strength of the ASM". The absence of discussion is rather frustrating for the reader and Fig. 3a could be removed or discussed further even if the paper does not deal with interrannual variability.

4/ Comparison with Bergman et al. (2013): Concerning the regional contributions, comparisons with Bergman et al. (2013) should be made deeper. The IN contributions found here during the monsoon (20-40

Details: - p8l12: sentence is not clear. - p9: "this distribution pattern does not match those of previous studies that used CO as a chemical tracer". Concerning studies using CTM's (such as Park et al. 2009, Yan and Bian, 2015) they are not displaying the same distributions and quantitative comparisons are not possible. Nevertheless, there is a qualitative agreement because these studies clearly show the predominance of South Asian relative to East Asian emissions to fill the AMA with CO. Particularly the highly polluted Indo-Gangetic Plain is highlighted here as a region largely participating to BL-to-TL transport. The most important qualitative difference between CTMs studies and the present one is of course TB with high convective activity and low pollution emissions. - p10l22: Figs 1a and 1b - p11l25: the northward shift of sources and their retreat from pre to post-monsoon is also clearly demonstrated in Barret et al. (2016) from Eulerian chemistry transport simulations and satellite observations of CO distributions once again showing that both approaches are qualitatively agreeing. - p13l23: "as the TP region" - p13l27: "possibly due to . . . variability in SST". This can easily be verified with SST from ECMWF reanalyses for instance. - p15l13-14: this sentence is not really informative and could be removed.

Additional Refs: Barret et al. (2016), Upper-tropospheric CO and O3 budget during the Asian summer monsoon, Atmos. Chem. Phys., 16, 9129-9147, doi:10.5194/acp-16-9129-2016. Ploeger et al. (2015), A potential vorticity-based determination of the transport barrier in the Asian summer monsoon anticyclone, ACP, 15, 13145-13159, 2015, doi:10.5194/acp-15-13145-2015. Popovic and Plumb (2001), Eddy shedding from the upper-tropospheric Asian monsoon anticyclone, J. Atmos. Sci., 58, 93-104, doi: 10.1175/1520-0469(2001)058. Tissier and Legras (2016), Convective sources of trajectories traversing the tropicaltropopauselayer, Atmos. Chem. Phys., 16, 3383-3398, doi:10.5194/acp-16-3383-2016.

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