Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2017-216-RC1, 2017

Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #1

Received and published: 13 April 2017

General comments:

This paper is not publishable in its present form. The first half of the analysis (Secs. 3.1.1 to 3.2.2, pages 9-14) represent a positive contribution. This analysis focuses on climatological aspects of air transport from the boundary layer into the Asian Summer Monsoon anticyclone, providing valuable insights into robust characteristics of this transport. The analysis in the second half (Secs. 3.2.3 to 3.3.2, pages 14-18), however, is too weak to publish. In particular,

- Regarding Sec. 3.3.2: The conclusions drawn from this analysis are not consistent with the figures shown. I see no 'distinct' peak in variability at 10-20 day periods (as claimed by the authors). There is a hint of a peak near 32 days for India, but otherwise

there are no distinct peaks at 30-60 day periods either. The largest peaks correspond to 64-128 day periods – considering that you use 3 months of data (each year), those peaks might indicate nothing more than the seasonal cycle. In creating the multi-year time series, did you combine a series of summers, for example, placing May 1 2001 immediately after July 31 2000? That practice is problematic and should be avoided.

- Regarding Sec. 3.3.1: For EOF analysis to be effective and the associated spatial patterns to be meaningful, the case for dominant modes should be stronger. Explained variances of 10-15% for each of the first three modes is small and the separation between these modes (and higher modes as well) might not be statistically significant (See North et al 1982, Monthly Weather Review, 'Sampling errors in the estimation of Empirical Orthogonal Functions'; there is also a review in Journal of Climate, Monahan et al 2009; 'Empirical Orthogonal Functions: The Medium is the Message'). That this section does little more than describe the spatial patterns and temporal variability of the first three EOFs makes it particularly important that these EOFs be dynamically meaningful and independent.

- Regarding Sec. 3.3.2: This analysis does little more than confirm that EOFs 1, 2, 3 represent similar modes of variability and that none stand alone as being dynamically relevant. This section is mostly descriptive and does not actually contribute to conclusions drawn at the end of the section [e.g., that the anti-cyclone is a bubble].

Specific Comments:

Page 4, Line 8: '. . . due to the lack of surface emissions, the contributions from certain sources regions, such as the Tibetan Plateau and the southern slope of the Himalayas, are likely seriously underestimated'. Do you mean 'lack of surface emission data'? If there was a lack of emissions (as stated) how could sources be underestimated (i.e., no value underestimates zero)?

P 6 L 4-15: Please specify the initial parcel spacing (i.e., how many degrees/kilometers between parcels) and how often parcels are initiated. In particular, be sure to discuss

how the model set up affects the results. For example, does the model setup maintain a uniform distribution of BL parcels throughout the entire analysis period?

P 6 L 24-27: Please discuss how the distribution of convection parameterized by FLEXPART compares with OLR.

P 7 (Sec 2.3): The question above (P 6 L 4-15) regarding the distribution of BL sources could be addresses in this section. It is not clear from your discussion that parcel distribution in the boundary layer is uniform. If that distribution is not uniform (throughout the analysis period), then the BL-TL statistics are not reliable measures of transport.

P 8 L 2-12: Provide a reference for the EOF analysis used and a brief description of the information that it allows you to extract.

P 8 L 7-9: Please describe (more precisely) how the low variance regions were filtered from the data. Why not use space-time averaging to reduce noise variance? How large of an impact does noise have if it is not filtered out?

P 8 L 9-10: What is meant by 'total field'? E.g., were monthly anomalies calculated by subtracting monthly means from the overall time mean?

P 8 L 21-26: Provide references for the wavelet methods used. Secs. 3.1.1 and 3.1.2: These two sections along with Figs. 1 and 2 contain a lot of redundant information. These two sections should be condensed into a short discussion of a single figure – Fig. 2 would be a good choice.

P 11 L 8-9: The text mentions an increase of mass transport from May 1 to July 31 – Fig. 3 shows mass transport from June 1 to August 31 (i.e. May 1 to May 31 is not shown). Please correct this discrepancy. A related question: is the time of transport related to the time of tropopause crossing? (Do the times along the x-axis of Fig. 3 represent the time of tropopause crossing?) Or some other reference.

P 12 L 22-23: You state that the air mass uplift from India (in Fig. 6a) increases dramatically before June 15 then decreases slowly afterward. To me, it looks like the

descent is just as dramatic as the ascent (absolute values of the slopes before and after June 15 are similar).

P 12 L 25-27: The meaning of the sentence 'Therefore, compared . . .' is too obscure.

P 13 Paragraph 1: This paragraph (regarding contribution by the Tibetan Plateau to BL-TL transport) is too unclear, unnecessarily speculative, and contains apparent discrepancies. For example:

- You state that 'the large contribution from the TP source in early May could be due to the strong sensible heating' and that the decrease in June 'presumably coincides with the decrease in sensible heating in this region'. The seasonal timing of sensible heating (e.g., from reanalysis data) should be cited here.

- You also refer to the seasonal timing of convection. You should refer explicitly to the OLR data set you use.

- You claim that the 'rain belt' reaches the Tibetan Plateau in mid-July and suppresses convection. My understanding is that Summertime rain in mountainous regions is associated with convection. Please clarify why the rain belt suppresses convection.

P 13 L 20 – P 14 L 2: Could the West Pacific contribution be related to tropical cyclone activity – again, the OLR data set can be helpful here.

P 14, last paragraph of Section 3.2.2: It would be helpful to clarify (state explicitly) that your study examines total mass transport whereas chemical tracer studies examine the mass transport weighted by chemical concentrations. This paragraph only alludes to that fact.

Technical details:

P 2, L 7 (and other lists of references): Remove the comma before the reference year; e.g., change 'Bannister et al., 2004' to 'Bannister et al. 2004'. Add a space between ';' and next reference; e.g., change 'Park et al., 2009;Randel' to 'Park et al 2009; Randel'.

P 4, L 3: Undefined acronym (ASH) used. P 4, L 10, 15, 17: The word ‘therefore’ is over used. P 8, L 12: Delete ‘when’. P 10 L 22: Should ‘Figs 2a and 2b’ be ‘Figs 1a and 1b’? P 11 L 22 (also L 27, P 12 L 1): No need to place ‘hotspot’ in quotes. If you want to define what a hotspot is – then place the first occurrence in quotes (and only the first occurrence) followed by the definition. Fig. 8 Caption: Undefined acronym (ASH) used. P 16 L 5: Change ‘EOFs’ to ‘EOF3’ P 18 L 10: Change ‘This region is overlaps . . .’ to ‘This region overlaps’

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