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Interactive Comment

Interactive comment on "Impacts of fire emissions and transport pathways on the interannual variation of CO in the tropical upper troposphere" *by* L. Huang et al.

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General comments:

This paper uses rotated EOF/PCA technique to study the dominant factors that control the interannual variability of CO. Then, the SVD method is applied between CO and those controlling factors to explore the coupled relations. This is a detailed analysis of CO interannual variability from both factor analysis and characterized transport pathways – probably too detailed that many (probably good) results are buried among trivial/obvious/already published results. Papers similar to this get published all the time, so I cannot in good conscious argue against publication, but I can beg the authors to spend time improving the writing and emphasizing the really important results. It should be realized that it is absolutely unnecessary to list all figures; a few pouching pieces that emphasize the key points is always better.

My recommendation: It is appropriate for publication after considering the following comments.

One of the concerns would be that many of results in this paper are convincing but not quite surprising since they are either too obvious or have already been shown in previous studies. For example, ENSO is generally the controlling factor for most of the atmospheric variability, which affects the convection and therefore the vertical transport of CO. Therefore, there surely exists the coupled relation between CO and SST (Fig. 6d) and then CO and convection (Fig. 8d). On the other hand, quantification of the coupled impacts makes this piece scientifically useful in our understanding of CO variability. The other highlight of the analysis is the impact of different ENSO types on the interannual variability of CO, which on the other hand compensate our understanding of EP/CP El Niño besides pure dynamic or thermodynamic analysis.

Factors that control CO variability are mainly the emission and the convectively dominated transport process, which is a very complex system that could be affected by various climate conditions hinted by SST, ENSO, etc. Being clear about this, many figures and contexts in this paper could be rearranged and shrunk in more organized structure to tell the stories clearer. For example, Fig. 4 and 5 could be merged and rearranged in order of tropics (overall), South America (emission dominant), Central Africa (transport dominant), and SE Asia (both emission and transport dominant). Similarly, section 4 and Fig. 6-11 could be rearranged in order of CO vs. emission, CO vs. IWC (transport), then CO vs. SST (climate induced change in transport) to demonstrate the coupled relation.

One major complaint about this paper is the redundant figures (and then the redundant

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discussions). For example, Fig. 1-3 could be rearranged into one figure with ONE eigenvalue spectrum followed by three pairs of EOF+PC. The eigenvalue panels unnecessarily repeat in each of the three figures. Also, Fig. 6a, 8a, 10a unnecessarily repeat Fig. 1c because in SVD the homogeneous correlation map only reveals the structure in a field associated with variations in its own expansion coefficient time series (PC). That's why there are strong similarities among these four panels. In SVD analysis, the heterogeneous (singular vector) patterns are more interesting because they reveal more directly the relationship between two (coupled) fields – this is what SVD analysis is all about. Therefore, the singular vector patterns (panel d in Fig. 6-11) should be emphasized more to demonstrate the coupled relation, whereas panel c in Fig. 6-11 are not that interesting (suggest removing). In fact, Fig. 6c & 7c can be found in many SST studies.

It would be more interesting to quantify the coupled relation between CO and SST (and therefore CO and IWC). In some sense I think this part of analysis is more useful than the EOF analysis in section 1.

It also needs to be realized that the signs of eigenvectors are chosen randomly by the calculation, so the similarity of 6a, 8a, 10a to 1c will be more obvious if the authors could plot them with the same sign convention (e.g., always make TWP the positive response). The same applies for Fig. 7d and Fig. 9d. This will help the authors identify the most interesting/important patterns and avoid the laundry list of all figures.

Another major complaint is about the color tables used throughout this paper. Color table usually is trivial and not worth mentioning, but for factor analysis that compares patterns the appropriate use of color table is crucial. In Fig. 1-3 and 12-13 the green color cross zero, which makes positive/negative variance barely distinguishable. In Fig. 6-11 color table has blue (cold) as negative and green (cold) as positive, which is quite misleading. Also the greater gradient in warm compared to weaker gradient in cold is not helping in distinguish patterns. So I strongly suggest using the SAME color table that is symmetric about zero with SAME gradients extend to both positive and negative

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ends, therefore all the EOF patterns could be compared more fairly. In this way, the resemblance in Fig. 1c, 6a, 8a, 10a could be more obvious; also the “dipole” feature in Fig. 6c will be buried (currently the dipole is caused mostly by misuse of color table).

Specific questions:

1. Will the un-rotated EOF yield similar results as rotated EOF? Despite the many advantages of rotating eigenvectors, the expansion coefficients in the rotated basis might not be orthogonal (assume using orthogonal method do the rotation), which blurs the factor contributions; 2. Is it reasonable to put all first components of CO vs. emission, CO vs. IWC, and CO vs. SST together, and then all the second components together? This might separate the contribution impacts; 3. It took ∼2 months for CO to be transported from emission surface to 147 hPa, so Fig. 4-5 might be neater if all emissions lines are shifted 2 months ahead; also the time axis are suggested to be changed to show only year labels with 12 ticks; 4. The 04-05 El Niño and 05-06 La Niña panels in Fig. 12, 13, 14 are unnecessary. In fact, all La Niña panels are not that interesting, suggest removing; 5. When it comes to transport pathways, a clearer classification would be to identify whether the source (emission) regions overlay on deep convective region. The advection just redistributes the CO-rich air horizontally; the significant transport vertically is only through deep convection.

Minor technical comments:

1. Spell out IAV as “interannual variation” throughout the paper; 2. Page 25572 line 9: which → that; 3. Page 25573 line 14-16: I bet you also averaged CloudSat CWC; 4. Page 25573: mention the method to rotate eigenvectors, orthogonal, or oblique, and give out proper citation of that method; 5. Page 25574 line 7-9: rephrase this part to make it clearer. For example, “Project the expansion coefficient time series of the left/right field to the input data that forms the left/right field doing the SVD yield the homogeneous correlation map; whereas project the left/right to the right/left yield the heterogeneous map that describes the coupled relation between two fields”; 6.

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Page 25576: change the order of the whole paragraph by illustrating the tropics and then 3 continents that represent dominant emission, transport, and both emission & transport, respectively; 7. Page 25577: what's the point of mentioning dipole, tri-pole? Besides, the dipole in Fig. 6c is merely caused by misleading color table; 8. Page 25577 line 15: "...index. However..." → "index, with largest shown at Niño 4 of 0.53 with UT CO and 0.87 with SST"; 9. Section 4 reads very redundant and trivial; suggest re-organize this section in a neater way.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/13/C8961/2013/acpd-13-C8961-2013-supplement.pdf

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