Reply to Referee #1:

We would like to thank referee #1 for detailed comments that helped us to improve the manuscript. We have carefully considered each of the reviewer's comments in our revision. Our responses are provided below (the reviewer's comments are shown inline in italics).

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 perspectives from pure dynamic or thermodynamic analysis.

Reply: We agree that previous studies have shown that ENSO has significant impacts on the interannual variation of CO in the troposphere, and we have reviewed some of these studies in the "Introduction" part. However, most studies focused on either case studies, or ENSO impacts on surface emissions; few studies have focused on ENSO impacts on the coupling between CO and its governing factors (e.g., emission, convection) as well as CO transport from the surface to the upper troposphere (UT). Besides, previous studies mainly focused on El Niño events, the differences of impacts between El Niño and La Niña on CO transport still need to be clarified. Thus, this study aims to answer these questions by using statistical methods and an innovative transport-pathway-identification method developed in our previous study.

Factors that control CO variability are mainly the emission and the convectively dominated transport process. The latter 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.

Reply: This is a very helpful comment. We have redrawn most of the figures in the revised manuscript according to the reviewer's comments. The changes include merging figures, deleting unnecessary figures, rearranging figures and adjusting the color tables.

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

Reply: Thanks for these helpful comments. We have merged previous Figs. 1-3 into one new figure as suggested. We have also removed the homogeneous correlation maps for each SVD analysis and only kept the heterogeneous correlation maps to highlight the coupled relationships between CO and its governing factors (e.g., emission, convection). Besides, we have put the first three SVD modes into one figure for each SVD analysis and revised the according discussions in the context.

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.

Reply: The SVD analyses are used to evaluate the coupled relationship between CO and SST (and also IWC). The REOF and SVD analyses served for different purposes. Specifically, the REOF analysis is used to identify the dominant modes of the interannual variation of CO in the tropical UT, and it shows the regions that have significant contribution to the interannual variation of UT CO and the climate conditions that have important effects on the temporal variation of UT CO. The SVD analysis is used to study the coupled relationship between CO and its governing factors (e.g., emission, convection), and it shows the regions where the coupling is strongest and the climate conditions that dominate the coupling.

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.

Reply: Thanks for this helpful comment. We have revised the figures as suggested.

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 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).

Reply: Thanks for this helpful comment. We have revised all the previous blue-red figures using a new blue-red color table which is symmetric about zero with same gradients extending to both sides.

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;

Reply: No, the traditional (un-rotated) EOF analysis could not separate the first two modes, both modes show a center over the Indonesian region and the principal components (PCs) are significantly correlated with ENSO index, thus the first two modes indicate the same ENSO impacts. We used an orthogonal method ("varimax") to do the rotation, and checked the rotated principal components; they are nearly orthogonal after the rotation (correlation coefficients are close to zero).

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;

Reply: It may be better to do so if we want to directly compare the 1^{st}, 2^{nd} or 3^{rd} mode of each SVD pair. However, in our revised manuscript, we have deleted the two homogeneous correlation maps and only kept the heterogeneous correlation map. In this case, we think it may be more concise to put all the first three modes of each SVD pair in one figure. It also facilitates the discussion of each SVD analysis.

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 (month) ticks;

Reply: We have tried to shift all emission lines 2-month ahead, but it seems more confusing without the use of two different horizontal axes, thus we decide to keep previous arrangement. Besides, we think it may be better to keep the "Jan" and "Jul" label on the time axis to help readers quickly identify the month they are interested in.

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;

Reply: The 04-05 El Niño and 05-06 La Niña are two ENSO cases since the launch of Aura in 2004. Since we want to compare the impacts of different ENSO types on CO transport, it is necessary to include as many ENSO cases as possible to find the similarity of the same ENSO type and the differences between different ENSO types. The reason why we did not consider these two cases in the pathway analysis is because "CloudSat was launch in 2006, here we only have two El Niño years (2006 and 2009) and two La Niña years (2007 and 2010) for the analysis". La Niña is another important phase of ENSO. Previous studies have shown that La Ninas are not simple mirror images of El Ninos and few previous studies have investigated its impacts on CO transport. Thus, we think it is important to compare El Niño with La Niña, instead of considering El Niño only. In this way, we can have a more comprehensive understanding of how different ENSO types affect CO transport to the tropical UT. In our revised manuscript, we have rewritten the abstract to highlight the new findings and clarify the different impacts on CO transport to the UT between different ENSO types.

5. When it comes to transport pathways, a clearer classification would be to identify whether the source (emission) regions overlay on deep convective regions. The advection just redistributes the CO-rich air horizontally; the significant transport vertically is only through deep convection.

Reply: Thanks for this helpful comment. In fact, our transport pathway identification method is based on whether the emission is co-located with convection (Huang et al., 2012). In our revised manuscript, we have added more description of this method to clarify the definition and differences of the two transport pathways that transport CO from the surface to the UT.

Minor technical comments:

1. *Spell out IAV as "interannual variation" throughout the paper;*

Reply: Revised as suggested.

2. *Page 25572 line 9: which → that;*

Reply: Revised as suggested.

3. Page 25573 line 14-16: I bet you also averaged CloudSat CWC;

Reply: No, the CloudSat CWC was only used in the transport pathway analysis, not in EOF and SVD analysis. In our pathway auto-identification method, we use instantaneous along-track observations of CWC to locate the deep convection, thus CloudSat CWC is not averaged.

4. Page 25573: mention the method to rotate eigenvectors, orthogonal, or oblique, and give out proper citation of that method;

Reply: Revised as: "In this study, the REOF analysis was performed using the most well-known and used rotation algorithm "varimax" (Kaiser, 1958), which is an orthogonal method, and the first 10 EOFs were chosen for rotation."

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";

Reply: Revised as "Projecting the principle component of the left/right field to the original data of its own field yields the homogeneous correlation map, whereas projecting the principle component of the left/right field to the original data of the other field yields the heterogeneous map that describes the coupled relationship between the two fields."

6. 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;

Reply: Revised as suggested.

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;

Reply: We have deleted the figures of "homogeneous correlation map" and the context of corresponding discussion in our revised manuscript.

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";

Reply: Revised as “Both PCs have significant correlation with ENSO indices (Niño 3, Niño 4, Niño 3.4 and SOI) and Pacific Decadal Oscillation (PDO) index, with largest correlation 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.
Reply: We have revised Section 4 as suggested.

10. Page 25589 line 24, this reference starts with first name. This makes some confusion because in page 25569 line 4 the reference of “Liu et al., 2007, 2010” should be two papers by two different authors.
Reply: We have revised the citation in the context to “Liu et al., 2007; Liu et al., 2010”.