Reply to Referee #2:

We would like to thank referee #2 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).

General Comments: Major concerns

The paper contains an interesting analysis regarding a pertinent scientific issue – the analysis is a worthy attempt to better understand the interannual variations of CO concentrations in the upper troposphere. However, much of the discussion of the analysis is misleading and important aspects of the analysis methods are left out. The authors should more clearly state which aspects of the analysis are meaningful which are essentially speculation. In addition, too much attention is given to unimportant details of figures. Sections 4 and 5 are particularly problematic. The authors should focus on the most important points and how the figures support those points. See 'Specific Comments' below for details.

Reply: Thanks for these helpful comments. We have made major revisions to the manuscript according to all reviewers' comments. These revisions mainly include: (1) add more discussion of the analysis methods, including more details of the pathway identification method developed in our previous study; (2) revise the figures and discussion to highlight the meaningful and important results, such as deleting the homogeneous correlation maps and only kept the heterogeneous correlation map and paying more attention to the coupled relationship discussion in the SVD analysis; (3) reorganize some sections, such as Sections 4 and 5, to remove redundant/trivial information and focus on the most important points, according to reviewer's comments below.

General Comments: Minor concerns

There is an overuse of acronyms. Since there are so many, I suggest retaining the most established acronyms (e.g., EOF, SST, ENSO) and those that stand for complex names of data sets (e.g., MODIS) and eliminating the rest (e.g., UT, LT, IAV, CWC, CPR) – or at least use them (a lot) less often.

Reply: In the revised manuscript, we have spelled out "IAV" as "interannual variation" throughout the paper. We deleted "CPR" since it was only used once. We also paid attention to the use of other acronyms (e.g., UT, LT, CWC) to make sure they were used appropriately.

Specific Comments:

Abstract, page 25568: The abstract is poorly written and does not reflect the most important results of the paper. The last two sentences (lines 12-17) are particularly confusing.

Reply: We have rewritten the Abstract in the manuscript as follows:

"This study investigates the impacts of fire emission, convection, various climate conditions and transport pathways on the interannual variation of carbon monoxide (CO) in the tropical upper troposphere (UT), by evaluating the correlation between these fields using multi-satellite observations and principle component analysis, and the transport pathway auto-identification method developed in our previous study. The Rotated Empirical Orthogonal Function (REOF) and Singular Value Decomposition (SVD) methods are used to identify the dominant modes of CO interannual variation in the tropical UT and to study the coupled relationship between UT CO and its governing factors. Both REOF and SVD results confirm that Indonesia is the most significant land region that affects the interannual variation of CO in the tropical UT, and El Niño-Southern Oscillation (ENSO) is the dominant climate condition that affects the relationships between surface CO emission, convection and UT CO. Results also show that convection and fire emission have different importance on the interannual variation of UT CO over the three tropical continents. We find that different types of ENSO have different impacts on CO transport from the surface to the tropical UT, by affecting surface CO emission over Indonesia and South America and convection over the west-central Pacific. Transport pathway analysis suggests that the average CO transported by the "local convection" pathway (ΔCO_{local}) accounts for the differences of UT CO among different ENSO types over the tropical continents during biomass burning season. ΔCO_{local} is generally higher over Indonesia-Australia and lower over South America during El Niño years than during La Niña years. The other pathway ("advection within the lower troposphere followed by convective vertical transport") occurs more frequently over the west-central Pacific during El Niño years than during La Niña years, which may account for the UT CO differences over this region between different ENSO types."

Abstract, page 25568: Use the word 'rotated' when referring to the EOF analysis (and use 'REOF' instead of 'EOF').

Reply: Revised as suggested.

Page 25568, lines 1-2: You should briefly describe the important aspects of deriving daily values from monthly. For example, did you assume noisy variations based on daily statistics? Did you use another daily quantity that is related to fires?

Reply: We have revised "The approach we used to derive daily emission of CO from GFED3 monthly emission is described in Mu et al. (2011)" as follows:

"In the transport pathway auto-identification method (Huang et al., 2012), daily along-track co-located satellite observations and CO emissions are used. Thus, we need to derive daily emission of CO from GFED3 monthly emission and the approach we used is described in Mu et al. (2011)."

Page 25573, lines 11-12: The sentence 'This is consistent with previous findings by directly evaluating CO anomaly field' is nonsensical; it needs some elaboration.

Reply: We have deleted this sentence in the revised "Abstract" to avoid confusion.

Page 25573, Paragraph 1 of Sec. 2.2: You should mention that the advantages of rotated EOFs (when using varimax) come at the expense of losing temporal orthogonality. Thus the variations associated with each REOF are not necessarily independent of the rest.

Reply: We have added more discussion as follows:

"Despite the advantages of rotated EOFs, it should be noted that an orthogonal rotation will find a new orthogonal basis, but the principal components may not be uncorrelated in the new basis, i.e., the temporal variations associated with each REOF mode are not necessarily independent of the rest."

Page 25574, lines 13-14: Linear relationships do not fully quantify functional relationships (i.e., they correspond to only the first term of an infinite number of terms in a Taylor expansion). Since there is missing information in a linear correlation, the ability to quantify relative importance is compromised. You should mention this caveat regarding your analysis.

Reply: We agree that simple linear correlation analysis cannot fully quantify functional relationships, and we have added discussion in the revised manuscript as follows:

"Although simple linear correlation analysis cannot fully quantify functional relationships, they could provide an initiative estimation of the relative importance of each variable through the magnitude of correlation coefficient."

Page 25575, line 3: You should briefly describe what North's rule of thumb is.

Reply: We have revised as follows:

"The REOF analysis is applied to the monthly anomalies directly. The first 10 EOF eigenvalues along with their uncertainties are shown in Figure 1a. We calculated the uncertainty of each eigenvalue based on a rule of thumb (North et al., 1982):

$$\Delta\lambda_k \approx \sqrt{\frac{2}{n}}\lambda_k$$

where λ_k is the kth eigenvalue, and n is the number of independent samples."

Page 25575, line 5-6: Since you rotated the EOF's, the PC's are no longer orthogonal. How does that affect the explained variance?

Reply: Previously, We did the traditional EOF (unrotated) analysis, the three leading EOF modes explain 37.6%, 18.6% and 8.3% (totally 64.5%) of the total variance, respectively. We have added the following discussion in the revised manuscript:

"After the rotation, the three leading REOF modes explain 28.4%, 13.9% and 12% (totally 54.3%) of the total variance, respectively. Thus, the order of variances explained by the three leading modes did not change, only the magnitudes have a little change. In either way, the three leading modes together explains >50% of the total variance."

Page 25575, line 15: Regarding 'this mode accounts for up to 96% of the variance in the regions of largest amplitude'. You should mention this increase of local correlation is what rotation of EOFs is designed to do – 96% explained variance in the 1st REOF is an expected result. The important thing about REOFs is that they reveal where the important regions are.

Reply: Thanks for this comment. We have added: "This is an expected result, since one advantage of REOFs is that they could yield localized or simple structures, and thus highlight the important regions."

Page 25575, lines 16-20: Statistical significance, regardless of the level of confidence, does not measure how close the relationship is. The explained variance measures how close the relationship is and statistical significance measures how confident we are in the result. The significant correlations in Table 1 typically explain 5-10% of the variance (correlations of 0.25-0.32) and none explain more that 15% (correlation 0.388). These do not indicate particularly strong relationships – but regardless of the value, please do not use statistical significance as a measure of a close relationship.

Reply: Sorry for this confusion. Actually we did not use statistical significance as a measure of a close relationship, to avoid misunderstanding, we have revised as follows:

"PC1 is significantly correlated (at 90% confidence level, the significant correlation is defined at this level hereinafter) with the ENSO indices (e.g., Niño4, SOI, as shown in Table 1), after considering the effective degrees of freedom associated with autocorrelation (hereinafter, autocorrelation is considered when needed). No other climate indices are significantly correlated with PC1. This suggests that the first REOF mode of UT CO interannual variation is closely related to ENSO."

Page 25576, line ~26: Perhaps you should mention the potential importance of the negative correlation with IWC over SE Asia. Could it be that fires don't burn when it's raining? The correlation analysis used for Fig. 4 and 5 has problems. There might be too many different factors involved for this over simplified

analysis. In addition to the negative correlation between CO and IWC in Fig. 5a, not that CO leads convection for the large peak on Fig. 4a. You should shy from drawing strong conclusion in the face of glaring contradictions.

Reply: We agree that we need to be cautious with the interpretation of the correlation analysis. The negative correlation between UT CO and IWC over SE Asia is reasonable because CO emission in this region is mainly related to the intense drought-induced fires (e.g., Liu et al., 2013; Livesey et al., 2013) when deep convection is depressed. Although this correlation analysis is simplified, it could provide us a general idea of which factor is more important over different continental regions and the results are consistent with our later composite analysis in Section 5.

Page 25577-80, Sec. 4: The main point seems to be that interannual variations of upper tropospheric CO are strongly related to ENSO. That point can be made very succinctly; however, this section is filled with seemingly pointless details that only confuse the discussion. This is particularly true of paragraphs 3 (page 25578; lines 3-15), 5 (page 25579; lines 1-13), and 7 (page 25579 line 24 – page 25580 line 4). Please rewrite.

Reply: The REOF analysis already demonstrated that the interannual variations of UT CO are closely related to ENSO. The SVD analysis (Sec. 4) aims to study the coupled relationships between CO and its governing factors, and the impacts of different climate conditions on their coupling. Following other reviewer's comments, we have 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.

Pages 25580-25584, Sec. 5: This section is too disorganized and unfocused and the conclusions regarding 'two types of El Nino' are likely to be meaningless due to insufficient statistical samples. It would be best to completely revise this section by first identifying 2-3 main points that can be reliably defended and discussing the relevant analysis in the context of those points. If that cannot be accomplished effectively, then it would be best to eliminate the section.

Reply: This section is an important component of this study since it investigates the impacts of different ENSO types on CO transport from the surface to the UT, through both composite analysis and transport pathway analysis. The different types of ENSO are already defined by many previous studies and is not the focus of this study. In our revised manuscript, we have rearranged the discussions to highlight the new findings of this study, i.e., the distinct impacts of different ENSO types on CO transport and UT CO changes associated with different pathways.

Page 25580, beginning of Sec. 5: Please acknowledge the fact that you have a very small sample of El Ninos and La Ninas, and have no statistical basis from which to conclude that there are two distinct types of El Nino evident in Fig. 12. In fact, one could easily look at the 3 El Nino maps in Fig. 12 and conclude there are 3 types or 3 variations of 1 type. It is well known that ENSO is more than a 1-dimensional phenomenon – for example, the prediction model of Penland and Sardeshmukh (J. Climate 1995) uses 15 EOFs of tropical Pacific SSTs to forecast their evolution. Please to not infer that two categories of El Nino capture all of the variability.

Reply: Thanks for this helpful comment. As we mentioned in previous reply, ENSO types are not a focus of this study. To avoid confusion, we have deleted Fig. 12 and the discussion of SST anomalies associated with different ENSO types, and used a table to list the ENSO cases analyzed in this study.

Page 25582, discussion of pathway analysis: More description of the method is needed. It appears that this analysis might be quite clever, but the discussion is too obscure to be sure. You should discuss briefly what distinguishes the two pathway types and what the uncertainties are – then refer to Huang et al (2012) for documentation of this description. A discussion similar to that of the last paragraph of Sec. 3 in Huang et al 2012 would be appropriate.

Reply: We have add more description of this method in Sec. 2.2 to briefly explain the mechanism and advantages of this method. The revised context is as follows:

"This method identifies the "local convection" pathway when an increase of CO in the UT is detected simultaneously with co-located non-zero surface CO emission and deep convection, and identifies the "LT advection → convection" pathway when an increase of CO in the UT is detected simultaneously only with co-located deep convection (i.e., the co-located surface CO emission is zero). This method streamlines the identification of two CO transport pathways by combining instantaneous along-track observations of CO in the UT from the Aura MLS, convective clouds from the CloudSat radar, and CO emissions derived from the MODIS fire counts data. Thus, it is very useful and efficient for the study of CO transport from the surface to the UT. Further details about this method and its application can be found in Huang et al. (2012)."

Page 25582, lines 17-19: Please describe how the percentage increase of CO associated with each pathway is calculated. For example, is there a metric that determines the percentage of transport accounted for by each mechanism? Is the time derivative of the CO concentration regressed against such a metric? [somehow, I don't think that this is what was done, but it gives an example of what constitutes an explanation] Perhaps an equation or 2 is warranted.

Reply: We have added an equation to help clarify the definition of "relative frequency of each transport pathway". The revised context is as follows:

"First, we calculate the relative frequency of each transport pathway, which is defined as:

$$freq_i = \frac{N_i}{N_{total}} \quad (2)$$

where $freq_i$ is the relative frequency of the *i*th pathway within a 4° latitude × 8° longitude grid box, N_i is the number of CO increase cases associated with the *i*th pathway and N_{total} is the total number of CO increase cases within the same grid box."

Technical Comments:

***Page 25568, line 10**: 'over Indonesia and is related'*

Reply: The "Abstract" is rewritten. Please refer to our reply above.

***Page 25570, line 3**: Change 'influence' to 'influences'*

Reply: Done.

***Page 25569, line 1**: Change 'to improve' to 'for improving'*

Reply: Done.

***Page 25570, line 21**: Change 'former refers to as that CO is' to 'former refers to CO that is'*

Reply: Revised to "former refers to that CO is…, whereas the latter refers to that CO…"

***Page 25572, lines 23-24**: Change 'The gridded data has … and includes …' to 'The gridded data have … and include …'*

Reply: Done.

***Page 25572, line 19**: Change 'monthly anomaly of each variable is' to 'monthly anomalies of each variable are'*

Reply: Done.

***Page 25573, line 18**: Change 'The Empirical Orthogonal Function' to 'Empirical Orthogonal Function'*

Reply: Done.

***Page 25573, line 19**: Delete the comma*

Reply: Done.

***Page 25573, line 22**: Change '80th' to '80s'*

Reply: Done.

***Page 25575, lines 17 and 18**: 'Hereinafter' requires explicit context. For example, 'hereinafter, a 90% confidence level defines statistical significance'. Please provide a context for the two uses.*

Reply: Revised as: "PC1 is significantly correlated (at 90% confidence level, the significant correlation is defined at this level hereinafter) with the ENSO indices (e.g., Niño4, SOI, as shown in Table 1), after considering the effective degrees of freedom associated with autocorrelation (hereinafter, autocorrelation is considered when needed)."

***Page 25576, line 12**: Change 'period' to 'periods'*

Reply: Done.

***Page 25577, line 4**: Change 'these' to 'those'*

Reply: Done.