Review of "Impacts of fire emissions and transport pathways on the interannual variation of CO in the tropical upper troposphere" by L. Huang, R. Fu, and J. H. Jiang

Recommendation:

The paper requires major revision.

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.

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.

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.

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

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?

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.

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.

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.

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

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?

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 1sf REOF is an expected result. The important thing about REOFs is that they reveal the where the important regions are.

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.

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.

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.

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.

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.

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.

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.

Technical Comments:

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

Page 25569, line 3: Change 'influence' to 'influences'

Page 25570, line 1: Change 'to improve' to 'for improving'

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

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

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

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

Page 25573, line 19: Delete the comma

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

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.

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

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