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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Upwelling into the lower stratosphere forced by breaking tropical waves: evidence from chemical tracers" *by* Z. Engida and I. Folkins

Anonymous Referee #2

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Overall:

This paper describes an analysis of coherence between MLS tracer data (H_2O, CO and N_2O) at 68 hPa and temperatures at 100 hPa. The aim of this study is to make clear mechanisms to explain variability in the tracers in the tropical lower stratosphere. Methods of this study mostly consist of statistical calculation. However, there is no information about statistical significance and uncertainty. Additionally, many results do not show enough evidence to make clear the mechanisms that explain variability in the tracers (especially CO and N_2O), thus many conclusions of these results are matters of speculation.

I think major revision of the current study is required.

Major comments:

1) I think D'Amelio et al. (2009) suggests N_2O and CO have a strong relationship "if biomass burning is a significant contributor." Additionally D'Amelio et al. (2009) compares N_2O with CO only in the Eastern and Central Amazon in dry season, and poor or negative correlations between CO and N_2O were also observed. Thus it's possible that there are poor or negative correlations between CO and N_2O, and first 600 days of N_2O data should not be excluded.

2) Vertical distribution of the tracers is needed to show local upwelling controls the tracer mixing ratio clearly. Without the vertical distribution, it is difficult to make clear that variation in vertical advection is controlled whether by changes in the upwelling or by changes in vertical gradient of the tracer mixing ratio. I think the tracer data at both 68 hPa and 82 hPa should be used.

3) If local variability of the vertical flow controls the tracer mixing ratio at 68 hPa, the temperatures at 70 hPa also have high coherence with the tracers at 68 hPa. Thus, coherence between the tracers at 68 hPa and the temperatures at 70 hPa should be shown.

4) Information about statistical significance and uncertainty is needed for these coherence analyses especially for $\overline{\tau}$ in Fig. 11. I think scatter plot does not converge enough except for multiyear H_2O.

Minor comments:

1) Description of the paper overview, especially for section 4 is too specific and wordy. Most of that should not be described in the paper overview.

2) I could not understand why there are negative values in power spectra in Fig. 6, and

there is no minimum at around 0.5 year in Fig. 6a and 1 year in Fig. 6b.

3) If both fluctuations of H_2O and CO in subseasonal time scale are controlled by Rossby wave activity associated with the MJO, why are their $\overline{\kappa}$ maps not similar (Figs. 8a and 8b)? Additionally, why do their predominant peaks not match in Fig. 7?

Specific comments:

page 19582, line 17: I think equation 1c is correctly,

"$\gamma_{T,H_2O}(\tau) = \frac{1}{N-1}\sum_{t=1}^{N}(T(t))(H_2O(t+\tau))$".

page 19582, line 19: Fig. 4e? I think Fig.4b indicates CO time series.

page 19594, line 27: 100 hPa "temperature" anomalies.

page 19595, line 11: "exchange" is overlapped here.

page 19611: Caption of Fig. 9 should be written properly.

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