This paper presents an analysis of the coherence between temperatures at 100 hPa and tracers (H2O, CO and N2O) at 68 hPa, intended to identify mechanisms of variability for the tracers in the tropics. While the cross spectrum methodology seems reasonable, I find weak evidence for the conclusions in almost all of the results. Although most results are based on coherence calculations, there is no discussion of statistical significance. Even the premise of looking for relationships between temperature at 100 hPa and tracers at 68 hPa (almost 3 km above) seems curious to me (why not examine temperatures and tracers on the same level?). Overall I do not find the results in the paper novel or compelling, and I do not suggest publication in anything resembling the current form.

Comments:

1) Regarding exclusion of the 600 days of N2O data: it is unclear to me why one expects close correlations between CO and N2O, as surface sources are not common. Fig. 5 does not show much correlation for any time period, and it seems arbitrary to exclude part of the record because it doesn't behave as you might expect. In any case, there are no significant results in the paper regarding N2O, so these data might just be completely omitted.
2) Why are there negative values for the power spectra in Fig. 6? The coherence squared calculations used throughout the paper need to have rigorous statistical uncertainties evaluated; a well-known aspect of coherence squared calculations based on few degrees of freedom (small bandwidth) are spurious large values.
3) Subseasonal results: The patterns in Figs. 8-9 look noisy and incoherent on large scales. The patterns in Fig. 8a are only slightly reminiscent of 'Rossby lobes', and it is not easy to make sense of their longitudinal location with respect to climatological convection. Why should water vapor reflect this quasi-stationary structure for MJO-like subseasonal variations, which are eastward propagating features? The CO and N2O results in Fig. 8 do not suggest physical relationships.
4) Multi-year time window: The comments above (2) regarding significance are especially relevant for this section, as there are relatively few degrees of freedom for the multi-year window. The patterns in Figs. 9a-b are not similar to the localized dehydration regions identified in Shoeberl and Dessler (2011), and why should these be evident at 68 hPa in any case? The CO and N2O results in Fig. 9 are noisy and incoherent.
5) The results in Fig. 10 do not show the patterns that would be expected for the QBO (primarily zonally symmetric, centered over the equator) or ENSO (Rossby/Kelvin wave structures). This simply does not look correct to me.
6) Weighted time lags: I understand the calculations, but the results do not make sense. Uncertainty estimates need to be incorporated in all of these statistical calculations.

There is excessive hand-waving in this section, with a lot of ‘inferred’ and ‘implied’ discussions; I cannot find any clear robust or novel results.

Minor comments:

1) The title is very curious to me. Where is there any evidence of breaking tropical waves in this work?
2) Page 19576, lines 22-25: temperature tendencies and tracer tendencies should both be related to upwelling, so that one expects in-phase temperatures and tracers from this mechanism.