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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

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Engida and Folkins present an analysis of the correlation between trace gases and temperatures to obtain information on upwelling in the tropical tropopause layer. Specifically, they analyse timeseries of CO and N2O at 68hPa, and temperatures at 100hPa. I've read the manuscript several times, and I think I understand the method of the paper. However, the interpretation of the results remains unclear to me. I miss a clear description of what one can expect to see - basically, information from one level can propagate to another layer through advection, or through what is essentially "air mass displacement". There seems to be a brief discussion on page 19575, lines 20ff; but I don't think that this discussion captures the essence of the problem. If I understand the paper correctly, the authors try to extract information about the diabatic

upwelling, and how this is influenced by tropical large-scale waves. Indeed, the title postulates that the paper presents evidence for the role of breaking tropical waves (the Rossby-waves, as specified in the abstract). Certainly, variations in diabatic upwelling result in variations of the speed of the advective signal propoagation. But, it would appear to me that the search for optimal lags as function of frequency of the perturbation does not give the desired information in the case where the perturbation period is shorter than the mean transit time. In fact, I cannot see how the authors relate the 3-day lag found related to sub-seasonal variability in upwelling, when the mean transit time is of order 100 days. It appears to me that the authors primarily detect isentropic displacement related to dynamical variability, not changes in the residual upwelling. These effects are easily seen in temperature data alone, and are not overly spectacular per se. The discussion provided in the paper as to what one can see in tracer variations on fixed pressure pressure levels is too thin. All three tracers have different characteristics, and pointing to them having vertical gradients is not enough. Sources and sinks of the tracers need to be considered - for example, the Fueglistaler and Haynes (2005) paper cited in this study shows that one must expect dehydration regions to vary in space - how meaningful is then a correlation of a tropical average at 68hPa against temperature at a given position? Similar arguments can be made for CO and N2O. I agree with the authors that these tracers all contain interesting information, but the paper needs to be much clearer what kind of information, and what kind of information the chosen method is able to extract.

In my opinion, this paper needs a major revision both to address my concerns on how to interpret the results, and to address the issues raised by the other reviewers (which I share) specifically also concerning the statistical robustness.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 19571, 2012.