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Interactive comment on “The response of the equatorial tropospheric ozone to the Madden–Julian Oscillation in TES satellite observations and CAM-chem model simulation” by W. Sun et al.

Anonymous Referee #2

Received and published: 16 July 2014

This is an interesting paper on MJO variability in tropospheric ozone determined from TES measurements and a chemical transport model (CAM-Chem, based on GEOS-5 dynamics and MOZART-4 photochemistry). By focusing on low tropical latitudes the authors are able to identify and quantify certain characteristics of MJO variability in tropospheric ozone from TES measurements and their model. Comparison of MJO variability between TES and the model appears good from their analyses and this lends credence to both the TES measurements and the CAM-Chem model. The model appears to be getting the dynamics and chemistry generally correct for the MJO

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in tropospheric ozone. The authors find that the variability of tropospheric ozone due to the MJO may locally be up to about 50% of total variability which is substantial. The model suggests that advection dominates convection in driving the MJO variability of tropospheric ozone. A reason for this is that the model indicates that lightning NO_x greatly increases vertical and horizontal ozone gradients, thus increasing the advection ozone anomalies. The paper is suitable for ACP and is an important science contribution to the measurement and understanding of the MJO in tropospheric ozone (and OH to some extent with the model). I recommend publication but after some better clarification of the analyses and a few other points which are listed below:

* Page 16088, lines 6-11: These sentences imply that TES measurements are better for evaluating MJO variability in tropospheric ozone than other satellite measurements, but this may not be true for several reasons: (1) poor horizontal coverage – TES measures only along nadir orbital positions (i.e., ~14.6 longitudinal measurements per day on average), (2) lack of cloudy scene measurements – TES IR cannot detect tropospheric ozone in the presence of clouds (unlike MLS) which are prevalent in the tropics, and (3) poor vertical resolution – TES has broad vertical resolution for retrieving ozone profiles (section 2.1 notes that this is about 6 km on average for 30-900 hPa). Because of broad vertical resolution TES cannot well separate ozone in the troposphere from ozone in the stratosphere in vicinity of the tropopause (TES is worse at doing this than residual techniques such as from OMI/MLS). The sentences should be either deleted or re-worded regarding these points.

* Page 16094, line 1: The analyses use the EOF methodologies of Wheeler and Hendon [2004] for the RMM indices and the eight defined phases of the MJO. Wheeler and Hendon [2004] applied these techniques to OLR and zonal winds in the troposphere, but it is not clear how their methodologies are actually applied in the present paper for tropospheric ozone. You might describe a bit more regarding this. RMM1 and RMM2 if I am correct are the two MJO indices (with near-zero correlation between them) derived from the two leading EOFs. Wheeler and Hendon [2004] ascribe about 25% total

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variance of intra-seasonal OLR/winds to these two leading components.

* Page 16094, line 5: You might mention that 10S-10N with 10 degree longitude intervals results in 36 grid points for the EOF analysis yielding 36 eigenvalues/EOVs/EOFs (or is this not correct?). Was there a specific reason not to use 15S-15N as by Wheeler and Hendon [2004]? Does including latitudes beyond +/-10 degrees for tropospheric ozone result in too much extraneous signals in the EOF analysis to resolve MJO variability? It is also not clear how the EOF methodologies were actually applied to arrive at Fig.4 for total tropospheric column (TTC) which shows a much larger latitude range from 30S to 30N.

* Page 16098, lines 10-14: You note that Tian et al. [2007] did not find substantial MJO variability in total column ozone in the tropics even though there is a sizable MJO signal in tropical TTC in your current work. (You mention this discrepancy even in the Abstract.) Could the reason for this discrepancy be that Tian et al. [2007] extended the EOF analyses to latitudes +/-40 degrees (i.e., too much cross-talk between too many grid point time series in the EOF analysis?), or could it be caused by stratospheric ozone variability from equatorial Rossby waves, mixed Rossby-gravity waves, Kelvin waves, etc.? These stratospheric disturbances will induce some amount of intra-seasonal variations in stratospheric column ozone which will complicate detection of the MJO in tropospheric ozone from the total column ozone measurements. By using TES measurements of tropospheric ozone you largely bypass these problems involving stratospheric ozone variability when compared to Tian et al. [2007]. Also, shouldn't line #13 refer to Fig. 5 with observed TES measurements rather than Fig. 6?

* The MJO analysis in your paper only includes EOF results – plotting actual time series of TTC in the Indian Ocean/western Pacific region (i.e., region of peak MJO) is simple to understand and would illustrate directly the peak-to-peak variability associated with the MJO (possibly 5 DU or larger?). Is it possible to include a figure of non-filtered and/or band-pass filtered time series of TES TTC and model TTC simulations for this region? This new figure might be placed in the manuscript before or after the Fig.4

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discussion.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 16085, 2014.

ACPD

14, C4991–C4994, 2014

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