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Interactive comment on "Tropospheric ozone variability in the tropics from ENSO to MJO and shorter timescales" *by* J. R. Ziemke et al.

Anonymous Referee #2

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1 Overview

The authors apply spectral analysis techniques to time series of tropical tropospheric columns of ozone (TCO) derived from the OMI/MLS satellite instruments, as well calculated within a CTM driven by the MERRA reanalysis, and a CCM driven by monthly mean SSTs. Variability in tropical TCO is examined for the first time in a consistent manner across a wide range of time scales, from ENSO through MJO and shorter. The authors demonstrate that techniques like this can be powerful tools for evaluating model performance and interpreting the processes driving variability in atmospheric composition. I have some concerns about the specific definition of the ODI, and I disagree that the coherence test definitively implies causality (one of their main conclu-

sions). However, I support its publication, pending some necessary clarifications and a strengthening of the arguments and/or softening of the language.

2 General Comments

1. I am skeptical of the suitability of the ODI metric for evaluating a model's ability to reproduce tropical TCO across short time scales for two reasons. First, I am concerned in its definition as a dipole over a large distance. Most of the atmospheric processes that drive variability in equatorial tropospheric ozone, such as the MJO-driven convection anomalies, act as waves propagating along the axis of this dipole. Given the spatiotemporal scale of the ENSO system, the dipole approach with 3-month smoothing as done in Ziemke et al. (2010) was certainly appropriate. However, for daily temporal variability, differences taken across such a large distance will almost certain lead to spectral aliasing and complicate interpretation. Second, the authors show that the Atlantic has the greatest relative variability associated with non-ENSO processes, however the ODI completely ignores this region. Does the phase coherence with OLR, the models, and OMI/MLS hold in the Atlantic as well? I think the authors need to provide a stronger justification and ideally an evaluation of the suitability of using ODI as a metric for a model's ability to reproduce all-tropical ozone variability on short time scales. To me, the simpler and more suitable approach would be as was done in Fig. 6 for the Indian Ocean, applied separately to the eastern and western Pacific, as well as other regions.

2. A main conclusion of this paper argues that the coherence/phase-coherence of the OLR and TCO time series shows that convection anomalies drive the majority of daily variability in TCO. I personally agree with the statement that convection drives the majority of variability in tropical TCO. However, mere coherence of the

signals does not imply causation. One obvious source of correlation between OLR and TCO is that tropical ozone is an absorber of longwave radiation itself, albeit a smaller effect than convection-driven changes in clouds. Furthermore, this work ignores discussion of the potential relative role that variability in ozone precursors may play on short-term variability in ozone. Perhaps one reason for the better performance of the CTM over the CCM is a more realistic interpretation of day-to-day variability in emission precursors? In the absence of additional evidence (does one wave lead the other?) or sensitivity simulations to demonstrate the physical causality between convection and TCO, I think that the authors need to soften language throughout the text that argues that the coherence test shows causality (e.g., p6374 L7, p6381 L26-28, p6383 L7-9) to statistically more conservative terminology.

3 Specific Comments

- p6374 L2 – “Ni no” should be “Niño”
- p6374 L5-11 – The Duncan et al. 2003 reference currently cited is an emissions inventory; it was probably meant to be 10.1029/2002JD003195. The authors might consider including references to studies using the TES instrument to examine ENSO/tropospheric ozone connections such as
  - Nassar et al., 2009 (10.1029/2009JD011760)
  - Neu et al., 2014 (10.1038/ngeo2138)

  and the following other relevant model studies
  - Valks et al., 2002 (10.1029/2002JD002894)
  - Murray et al., 2013 (10.1002/jgrd.50857)

  - Sekiya and Sudo, 2014 (10.1002/2013JD020838)
- p6377 L6-8 – Should give references for this claim, e.g.,
  - Lin et al., 2006 (10.1175/JCLI3735.1)
  - Hung et al., 2010 (10.1175/JCLI-D-12-00541.1)

  If any papers specifically evaluate the MJO in GEOS-CCM or the MERRA reanalysis, those would be especially relevant.
- p6377 L17 – “analyses” should be “reanalyses”
- p6378 L3 – Remove “deemed”
- p6378 L10 – Missing space between sentences.
- p6378 L10-12 – I assume “consonant" should be “consistent"? It is concerning to me that a selection criteria for the TCO product used here is its higher fidelity with the OLR, which is then later used to argue a causal relationship between convection and ozone. Since this acknowledges an uncertainty that affects the conclusions, I think it is important to include a quantification of what is meant by “most consistent", and a discussion later about how uncertainties in the TCO product (and OLR for that matter) may affect interpretation.
- p6378 L17-19 – Why wasn’t the MERRA tropopause pressure used for the OMI/MLS TCO to be consistent with the GMI simulations? How do the NCEP tropical tropopause pressures differ from MERRA? Please also state what tropopause pressures were used to calculate the TCO columns in the CTM and CCM.
- p6379 L7-11 – What does the CCM use for non-biomass burning precursors, are they consistent with GMI? The authors should provide a brief mention of which

tropical ozone precursors are being allowed to vary with time (e.g., lightning NOx, biogenic VOCs), and on what temporal scales, since it has direct implications for interpreting the relative role of convection versus other potential sources of tropical ozone variability in the models.

- p6379 L7-8 – Please give reference for the SST product being used in the CCM. If an SST product with daily variability such as the MERRA assimilated product or the daily SSTs from Reynolds et al. (2007) had been used, would the CCM have performed better on shorter time scales?
- p6379 L12 – Recommend narrowing scope of section title to "tropical tropospheric ozone"
- p6379 L16-19 – This description requires more detail in order to be reproducible. I assume TCO was de-seasonalized as in Appendix 2, which should be referenced here. How is the OLR time series de-seasonalized? Which Nino34 product was used; assumably it is at daily temporal resolution? The Nino34 index is sometimes provided as absolute SSTs instead of anomalies; if the former, was that also de-seasonalized? What is the justification for using a lag of 1-day in the Nino34 term? What is the value of β, and what specific method was used for determining it? I assume the "No ENSO" lines in Fig. 1 are the variability in the residual, ε(t)?
- p6379 26-27 – Can you clarify the justification for this claim? Figs. 5 and. 6 seem to indicate that the majority of the spectral energy is across ISO and ISO-to-ENSO time scales, not time scales shorter than ISO.
- p6380 L22 – replace "1.0" with "one" or "zero" with "0.0"
- Sections 4 and 5 could be merged.

- p6381 L6-7 – Please clarify what is meant by "and appears to produce variability with appropriate magnitudes for shorter timescales". My interpretation of the figure is that the CCM greater underestimates variability at shorter time scales, and this sentence seems to contradict L23-24.
- p6381 L25-26 – Please correct (OLR and convection are anti-correlated) and make clearer how the OLR proxy relates to convection. Low OLR corresponds with colder cloud tops, and therefore are a proxy for higher cloud top heights and increased convective activity (e.g., Chelliah and Arkin, 1992).
- p6382 – No need to redefine CCM, CTM and MERRA.
- p6383 L5-7 – This sentence seems at odds with Fig. 1, where ISO is at most half of the magnitude of observed non-ENSO daily variability.
- p6383 L24-25 – Please provided references for the statement "The inability of the CCM to generate shorter time scales is a known problem with GCMs/CCMs", and clarify what time scales are meant – ozone? convection?
- p6382-6383 – The final section would be greatly improved in scientific value with a brief discussion of the results in the context of previous work and implications for future studies. E.g., Is the conclusion that convection is the ultimate driver consistent with the earlier modeling studies focusing on emissions? Are the results consistent with the Sun et al. (2014) value cited earlier in the text? Why might the CCM so poorly reproduce variability in convection? Can we use ODI to constrain convection in CCMs?
- p6385 L16 – Erroneous period after "6"
- p6385 L20 – "was" should be "were"
- Figure 2 – Some guidance for the labels of the time axis would benefit readers, especially non-native English speakers.

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