

## Response to Reviewer #2 Comments:

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 conclusions). 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 certainly 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.

**RESPONSE:** You are certainly correct that tropospheric ozone in the tropical western and eastern Pacific regions as separated over large distance may not be dynamically linked for short time scales, especially day-to-day changes. As you note, ENSO and MJO links the east and west Pacific via eastward propagation of convection. *Dunkerton and Crum [1995]* also showed from OLR that convection at even shorter 2-15 day periods also propagates from western to eastern Pacific. For any time scale varying from ENSO and MJO to these 2-15 day periods, dynamical connection between the western and eastern Pacific depends on season and year and is not continuous. It is not required that the two regions used to calculate the ODI need necessarily be linked together at any time scale. The usefulness of the ODI is that it is easy to implement and that a model ODI will simulate measured ODI only if the model has both the correct dynamics and photochemistry in the Pacific from ENSO to short periods.

In our study we evaluated only time periods of 10 days or longer in the spectral analysis. (All figures involving spectral analysis such as coherence/phase were plotted only down to 10-day

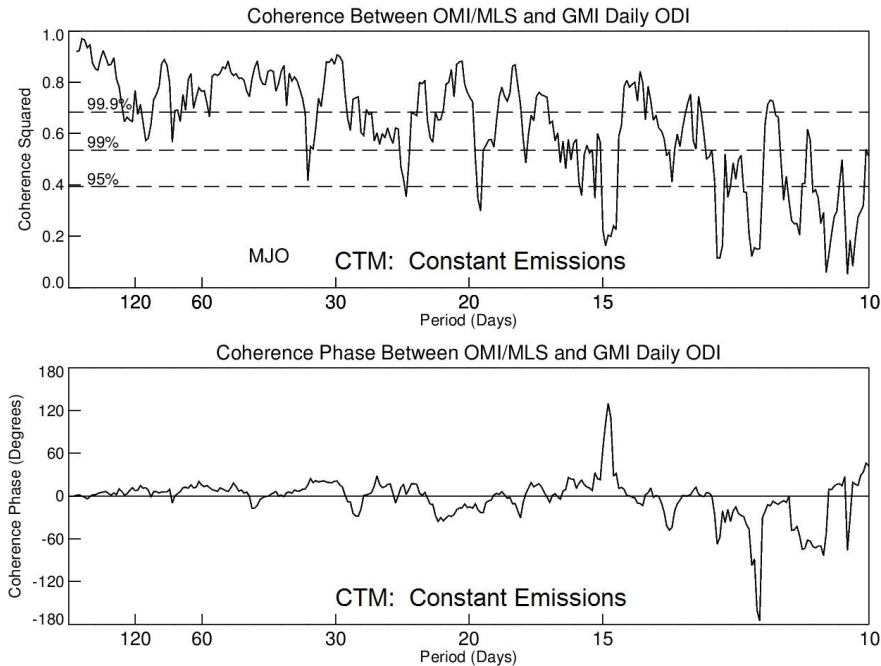
period.) There shouldn't be aliasing affecting the spectral analysis since we use daily time series where the Nyquist fold period is 2 days.

You also have a comment regarding the Atlantic region and the ODI. The ODI is a useful test for a model's performance in the tropical Pacific region but not elsewhere. For the Atlantic region (actually the entire tropics) Figure 1 provides, as a function of longitude, comparison between measurement/model ozone variability from ENSO to short time scale. Measured ozone (solid curves) and model ozone (dotted curves) in Figure 1 are remarkably similar in magnitude and longitudinal change. In the revision we have made the above points more clear. As a note, Figure 1 in the revision is now comprised of two parts with the latter (Figure 1b) corresponding to extreme ENSO events only.

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.

RESPONSE: You are very correct on this point – it was oversight on our behalf to imply such strong statements. Throughout the revision we have applied your suggestions and re-written the text in line with your comments including softening causality inference from the statistics. The revision now also mentions the variability of ozone precursor's potential effect on the variability of tropospheric ozone. We note in the revision that the emissions for the CCM were specifically chosen to closely match those of the CTM in both abundance and change with time.

Again regarding your question about the variability of precursor's effect on ozone, we tested the CTM again but instead used a run where the emissions were held constant with time over the long record. The results from that model run (i.e., ozone time series, spectra) were found to be nearly identical at all timescales to results in the paper where emissions varied with time. As example, shown below is a plot of ODI coherence/phase between OMI/MLS and CTM (i.e., similar to Fig. 3a) for the constant emission model run. This figure is nearly identical to Fig. 3a at all frequencies. The variability of tropospheric ozone from inter-annual to weekly timescales didn't change much at all from the previous CTM run despite holding the emissions constant in time.



(Similar to Figure 3 in paper, but instead for the CTM constant emission run)

### 3 Specific Comments

- p6374 L2 – “Ni no” should be “Niño”

RESPONSE: Done.

- 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)

RESPONSE: The *Duncan et al.* [2003] paper listed in the References was indeed incorrect and has now been replaced with the correct 2003 paper – thanks for recognizing this error. The above are all pertinent references that you have listed and have been included in our revision on ENSO from observations and models.

- 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.

RESPONSE: We have included in the revision these references that you mention regarding the difficulty in simulating the MJO – thanks.

- p6377 L17 – “analyses” should be “reanalyses”

RESPONSE: Done.

- p6378 L3 – Remove “deemed”

RESPONSE: Done.

- p6378 L10 – Missing space between sentences.

RESPONSE: Corrected.

- 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.

RESPONSE: Thanks for pointing this out. Our discussion was not written correctly. A reason for using the *Ziemke et al.* [2006] residual ozone stems from being independent of the MERRA analyses including wind fields used by the CTM. There are known problems with these wind fields in the tropical stratosphere which propagate to errors in tropospheric column ozone. The other OMI/MLS tropospheric ozone residual products evaluated by *Ziemke et al.* [2014], notably the trajectory and data assimilation products both used MERRA/GEOS-5 wind fields. Although we indeed found that the *Ziemke et al.* [2006] product compared better in the tropics overall than the other OMI/MLS residual products with both OLR and the CTM ozone analyses, that was not the real reason for using that data. This paragraph has been re-written in the revision.

- 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.

RESPONSE: Similar to the previous comment, using different tropopause pressures (i.e., MERRA versus NCEP) provides some amount of independence between OMI/MLS TCO and CTM TCO even though both MERRA and NCEP are similar analyses. In the tropics it really doesn’t matter much which analyses or which tropopause pressure definition is used since the tropopause pressure is largely invariant on even short time scales. We note in the revision that both the CCM and CTM used the same WMO lapse-rate definition of tropopause pressure. In

the tropics differences between MERRA and NCEP tropopause pressures are small and essentially do not affect calculated tropospheric column amounts.

- 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 NO<sub>x</sub>, 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.

RESPONSE: Thanks for these useful comments – we discuss these points in the revision. We mention among these other points that all of the global biomass burning and anthropogenic emissions for the CCM were specifically chosen to closely match those of the CTM in both abundance and change with time. We note in the revision that lightning NO<sub>x</sub> is allowed to vary daily. We again reference *Strode et al.* [2012] for details regarding emissions.

- 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?

RESPONSE: We now include reference to *Rayner et al.* [2003] for the monthly SSTs. We note in the paper with several references that there are ongoing issues with GCMs/CCMs in simulating an MJO in particular. Using daily instead of monthly SSTs would likely not improve performance of the CCM in light of these previous studies. We mention all of this in the revision. Improvements to this CCM will require understanding the complex convective processes in the atmosphere and correcting for this in the model (as future work).

- p6379 L12 – Recommend narrowing scope of section title to “tropical tropospheric ozone”

RESPONSE: Done – included “tropical” in section title for clarity even though the entire paper including main title relates 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  $\tau$ , and what specific method was used for determining it? I assume the "No ENSO" lines in Fig. 1 are the variability in the residual,  $\tau(t)$ ?

RESPONSE: Good points – thanks. This paragraph has been re-written for clarity and to correct for typos that somehow escaped us. I.e., the factor should have been 0.18, not 5, and index  $t-1$  in the equation should have been  $t$  (this has also been corrected elsewhere in the paper including the Figure 1 caption).

- 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.

RESPONSE: For the ODI part of the reason is the Pacific differencing and for Figure 6 the region was arbitrarily chosen where the 1-2 month variability (MJO) and ENSO was large.

- p6380 L22 – replace “1.0” with “one” or “zero” with “0.0”

RESPONSE: Done.

- Sections 4 and 5 could be merged.

RESPONSE: Valid argument - these two sections are related, but we would rather like to keep Section 4 and Section 5 separate. In this way Section 4 defines/explains calculation of the ODI and Section 5 separately discusses results using the ODI.

- 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.

RESPONSE: We have re-written this part.

- 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).

RESPONSE: Thanks for seeing that typo. We include more discussion of OLR including references as you suggest in the revised paragraph.

- p6382 – No need to redefine CCM, CTM and MERRA.

RESPONSE: This appears as an arbitrary decision – it is okay to repeat some of the acronym definitions in the Summary if they are not generally common acronyms for many of the potential readers of the article.

- 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.

RESPONSE: Non-ENSO refers to the residual  $\varepsilon(t)$  after extracting via regression the ENSO signal from gridded time series.

- 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?

RESPONSE: We now include references including *Del Genio et al. [2015]* as a recent paper (with other references therein) on the problem of generating an MJO in GCMs/CCMs.

- 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?

RESPONSE: We have now discussed the *Sun et al. [2014]* paper more in the revision, specifically in the Introduction on the topic of MJO. We have expanded the paper throughout on such issues, but we would like to keep those in the main section of the paper and retain a relatively short concluding Summary.

- p6385 L16 – Erroneous period after “6”

RESPONSE: Corrected.

- p6385 L20 – “was” should be “were”

RESPONSE: Sentence has been re-written.

- Figure 2 – Some guidance for the labels of the time axis would benefit readers, especially non-native English speakers.

RESPONSE: The Figure 2 caption now includes the following: The beginning labels “O”, “J05”, “A”, and “J” on the horizontal time axis in (a) and (b) denote October, January 2005, April, and July, respectively (similar labels for subsequent years).