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Comment

## ***Interactive comment on “A new ENSO index derived from satellite measurements of column ozone” by J. R. Ziemke et al.***

**J. R. Ziemke et al.**

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Referee #3 Comments on Title: A new ENSO index derived from satellite measurements of column ozone Author(s): J. R. Ziemke et al. MS No.: acp-2009-852

### General Comments

This paper provides a novel ENSO index based on east-west variability in tropical tropospheric column ozone driven primarily by a planetary-scale shift in SST and convective transport. The calculation of this index uses total column ozone (troposphere plus stratosphere) and removes the stratospheric component under the assumption of minor zonal variations (1-2 DU) of tropical stratospheric column. This assumption is tested and shown to be reliable for the timescales of interest in the paper. The result-

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ing ozone ENSO index (OEI) is calculated by differencing the tropospheric column in two broad regions of the eastern and western Pacific. The result is interesting both from a scientific point of view to show that such a robust correlation exists as well as a diagnostic test that can be used in modeling long term changes in tropospheric ozone. The paper is well written and I recommend publication after minor revisions.

Thanks for your comments on the paper. We have incorporated them into the revised manuscript.

### Specific Comments

1. The assumption is that the main cause of variation in TCO is due to shift in deep convection from the western to the eastern Pacific. This appears to be a plausible mechanism, but has there been any modeling work that has shown this to be a causal link? Of course, the causal link isn't necessary to prove a high correlation between OEI and other indices, but I wondered whether anyone has simulated this in a real CCM or CTM.

In the revision we reference the study by Chandra et al. [2002] which indicated from the Harvard GEOS-Chem model that most changes in TCO in the Pacific associated with ENSO are caused by dynamics. Reviewer #1 had a similar comment.

2. In Section 3.1 you say that the variation in tropical SCO is small at a few DU even in daily measurements. These results are shown in Figure 4 for MLS SCO averaged between 15 S and 15 N. What are the variations in MLS ozone based on individual profiles? Is this much larger than that over the broad geographical average? Couldn't there be zonal wave features in the SCO that cancel each other out resulting in low average.

You are correct that individual grid-point SCO time series have larger temporal variability, and wave features in the stratosphere can become dominant beyond about 20N and 20S, especially in winter and spring months. In our study we averaged the SCO

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data between 15S and 15N to be consistent with the OEI spatial averaging.

3. It might be worth explicitly mentioning that the years associated with the CCM run don't necessarily correlate to real-time, since the model isn't directly constrained to real-time observations for those years.

This is mentioned in the revision.

4. It would be helpful to reproduce the Figure 4 with the data from the CCM.

For our study we opted for brevity to include only monthly means from the CCM. The purpose of Figure 4 relates only to CCD measurements – it is meant to show that even in daily measurements there is meaningful information from the CCD method in the tropics.

5. Were the assumptions of SCO zonal variability tested for SBUV/2 as they were for the TOMS and OMI-based data? With 3-years of SBUV/2-based data inserted into the time series it might be important to mention.

Even though total column ozone from SBUV is very accurate, there is little ozone profile information from SBUV below the ozone number density peak (~20-30 hPa in the tropics). SCO from SBUV shows a robust (and erroneous) zonal wave-one pattern in the tropics that has an amplitude about 1/2 that of the actual wave-one pattern in TCO (~25 DU) [Ziemke et al., JGR, 1998]. In spite of these problems the total column measurement is highly precise and at least as good as TOMS for use in this study. We have discussed these points in the revision.

6. Is the “data mining” technique you’re referring to simply looking at the correlation plot and selecting a region “by eye” that has a high correlation? The term “data mining” sounds like something more technical.

Reviewer #1 had a similar comment (see our response for Reviewer #1). We have now deleted referral to “data mining” in the revision since it is really not necessary.

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7. In Figure 11, it would be interesting to plot the OEI together with the stratospheric dipole to see whether there appears to be any temporal correlation. Can the stratospheric dipole be easily extended to a longer time series? It would be nice to correlate the two over the whole time series. Maybe this is beyond the scope of the current work, but could be an important calculation.

In the current manuscript one can compare the MLS SCO dipole time series in Figure 11 with the OEI in previous Figure 10 (or Figure 9) over the same time record from October 2004 – June 2009, but there is no apparent correlation between these time series. Interestingly, there is better evidence of a correlation with the QBO signal in MLS SCO in Figure 6, but the amplitudes in Figure 11 are exceedingly small in comparison ( $\sim 1$  DU on average). The purpose of Figure 11 in our paper was only to estimate errors in tropospheric-ozone inferred OEI under the CCD assumption of zonal invariance of SCO. For the second point, evaluation of an SCO pacific dipole cannot be extended much outside the recent Aura MLS time record beginning in late 2004. Previous SCO measurements such as from UARS HALOE or SAGE/SAGE II, although very accurate, are very sparse in the tropics even for monthly ensembles. Such an analysis is beyond the scope of the current study.

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