Atmos. Chem. Phys. Discuss., 10, C470–C472, 2010
www.atmos-chem-phys-discuss.net/10/C470/2010/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #3

Received and published: 8 March 2010

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 ($\sim$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 resulting 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.

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.

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.

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.

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

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.

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.

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 strato-

spheric 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 2859, 2010.