Atmos. Chem. Phys. Discuss., 9, S758–S759, 2009
www.atmos-chem-phys-discuss.net/9/S758/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of the 2006 El Niño on tropospheric ozone and carbon monoxide: implications for dynamics and biomass burning" *by* S. Chandra et al.

Anonymous Referee #1

Received and published: 12 March 2009

This is a worthwhile investigation describing enhanced values of carbon monoxide and ozone being present over Indonesia during the 2006 El Niño. The authors use both satellite information and modeling results to quantify the enhancement relative to the same time of the year in 2005 and conclude from the analysis of their modeling results that the enhancement found over this region is from both an increase in biomass burning and transport. The satellite observations they discuss are from the tropospheric column ozone (TCO) calculated by subtracting stratospheric ozone derived from MLS from total column ozone measured by OMI. They conclude that findings are similar to and consistent with the previous of Logan et al. (2008) who conducted a similar analysis using tropospheric ozone amounts derived from TES measurements. From

this perspective, it is reassuring that TES and OMI/MLS tropospheric ozone provide comparable results.

Overall, the paper provides additional information about the relationship between inter-annual regional pollution enhancement and ENSO. They have performed similar studies using different methodology to calculate TCO values for the 1997 El Niño. They also look at the long-term relationship between the difference in TCO values in the eastern and western Pacific and one of the conventional indices relating to ENSO (i.e., SST 3.4). This analysis is similar to one done by Fishman et al. (2005, JGR), but they did the long-term relationship for different regions in the tropics.

One of the aspects that the authors employ that had not been done by Logan et al. was the use of the GMI (CTM) model to interpret their findings that led to the conclusion that the enhanced ozone levels were due to a combination of both increased emissions from burning and long-range transport. Since this is the most unique aspect of the paper, the authors should expand on what the distribution of ozone looks like in the upper troposphere and lower stratosphere. They say that they use MLS data to construct TCO values, but it would be beneficial for them to expand upon what such a model is calculating for the ozone distribution in this important region of the atmosphere and discuss whether or not it is consistent with what MLS is observing. How does one define the tropopause using these model calculations and using an instrument such as MLS, which is pushing its capabilities trying to extract information in the upper troposphere? Can the synergy of models and measurements be used to improve both or is there perhaps another set of data that can be used to provide a higher quality dataset from each? As the paper is written now, it is a worthwhile publication, but it can expand the frontiers of knowledge to a much greater degree if it takes the next steps as put forth above.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2735, 2009.

ACPD

9, S758–S759, 2009