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Comment

Interactive comment on “Impacts of 2006 Indonesian fires on tropical upper tropospheric carbon monoxide and ozone” by L. Zhang et al.

Anonymous Referee #1

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General Comments

This paper investigates the impact of the Indonesian fires and dynamical changes associated with the 2006 El Nino on CO and ozone in the upper troposphere. The study compares satellite observations of CO and ozone from the Microwave Limb Sounder (MLS) with simulations using the GEOS-Chem model, enabling the variation of fire emissions and meteorological fields (GEOS-4 and 5, 2005 and 2006) in a series of simulations.

2006 was a moderate El Nino according to traditional ENSO classification, but resulted in an exceptionally large perturbation to tropospheric CO and ozone over the tropical Western Pacific, thus making the topic worthy of investigation and appropriate material for ACP. The body of this manuscript does a fair job of citing earlier work investigating

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the impacts of El Nino on tropospheric ozone and CO in the region, demonstrating that the authors have done their reading; however, the present work does not demonstrate new and innovative approaches. Although the present work adds confirmation to the earlier work, it doesn't reach any major new conclusions beyond what is found in the earlier publications.

The largest contributions that this paper makes are:

- 1) An emphasis on upper tropospheric (UT) ozone and CO. Earlier papers like Chandra et al. (2009) dealt with column ozone, while Nassar et al. (2009) investigated both the lower troposphere (LT) and UT, but more effort was dedicated to understanding the LT where impacts of surface emissions are more direct.
- 2) The exploitation of different meteorological fields (GEOS-4 and GEOS-5) to investigate the impact of convection on UT ozone and CO.
- 3) Confirming that fire emissions from Australia did not make a large impact on CO or ozone in the region of interest.

If this study were to be published 3-4 years ago, it would be useful, but right now, it does little to expand the boundaries of our understanding on the subject. There are still gaps in our understanding of tropospheric chemistry over Indonesia during El Nino that could be explored and it is up to the authors to decide which direction to take to address these gaps and contribute to our understanding. Some suggestions that come to mind include:

- 1) Adding a comparison of the 2009 El Nino, for which there is not much in the literature yet.
- 2) Addressing the impact of injection heights from the Indonesian biomass burning to see if this is a factor in the observation-model mismatches.
- 3) Better quantifying the role of NO_x (from lightning, fire emissions, soil, etc.) or other ozone precursors in explaining the larger differences seen between model and MLS

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ozone in comparison to CO (for example Figures 6 and 8).

The authors do make a reasonable and fairly detailed effort to interpret the differences between the GEOS-4 vs. GEOS-5 simulations, or the GFEDv2 vs. seasonal biomass simulation; however, they do not make much of an effort to understand the differences between the simulations and the MLS observations. They frequently state that the model “reproduces” features of MLS ozone or CO, which I do not consider to be a justifiable statement. Figures 6-9 show large differences between the model and MLS observations. Although these differences are discussed somewhat in section 4, this is not done very thoroughly and the interpretation is based entirely on previous studies. Chemical transport models have been around long enough that they are clearly able to simulate the gross features of ozone or CO, but investigating the slight differences between the model and observations, and explaining why they occur is important to help the models improve.

With regard to other aspects of the paper overall, it is well-written and the scientific methods are clearly outlined and presented with fluent language. The figures are of sufficient quality and the use of symbols, units, abbreviations etc. is consistent with norms in the field.

Considering all of these factors, I do not recommend the paper for publication in its current form, but would reconsider it after major revisions are undertaken so that their findings add something new and significant to our understanding of the impact of El Nino on tropospheric ozone and CO over Indonesia.

Specific Comments

Title: the paper actually investigates the impact of fires and dynamics but only fires are mentioned in the title.

Page 19358, line 7: To me, the term “reproduce” implies that they are identical, which they are not, so something like “have similarities with” would be more appropriate. This

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distinction is important.

Page 19360 lines 4-7: Attribution of CO and ozone enhancements to fire or dynamics is not made in Logan et al. (2008), but is made in the modeling paper Nassar et al. (2009) with some of the same authors. Some sentences in the present manuscript come directly from the abstract of Nassar et al. (2009), which is fine, but the citation should be corrected.

Page 19361, line 17: Field and Shen (2008, JGR, 113, G04024) showed that the likelihood of extreme burning events like 1997 and 2006 is not linearly related to precipitation but rather it abruptly increases when precipitation drops below a threshold values. This citation should be included with this important fact noted.

Page 19361, line 22: The fact that GFEDv2 was used rather than GFEDv3 is acceptable since v3 has only been available for a few months, but since v3 is an improved version, its existence should be acknowledged. The authors need to take at least a quick look at v3 and a statement discussing whether or not the findings are expected to be consistent using v3. This can either be discussed here or in the model section.

Figure 1: Why was the WCI region (blue rectangle) defined to exclude some emissions from Northern Sumatra and Borneo, but include Java with near-zero emissions?

Page 19362, lines 17-20: "Previous version biases . . . have been ameliorated . . . compared with previous versions". This sounds like a circular argument and should be rephrased.

Figure 5: Would it not have made more sense to compare GEOS-4 and GEOS-5 Radon-222 distributions for 2006 since that is the primary year of interest in this paper?

Page 19366, line 18: Please state what time frame the seasonal biomass climatology represents and if any significant El Ninos were included. The interannual variability used to derive the climatology will matter and the reader should not be required to go to Duncan et al. (2003a) for these key points.

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Page 19367, line 2 and Figure 6: GEOS-Chem does not reproduce the MLS observations. There are some nice similarities, but some noticeable differences too and explaining these differences is necessary for a complete understanding of the El Nino impacts. The models are expected to get the large scale gross features correct. If they did not, we would have some real problems, but if the models are not held to a higher standard than they are here, they will not improve and nothing is learned. For example, how about a quantitative assessment of the extent to which the model gets the seasonal cycle correct?

Page 19367, lines 6-20: The authors correctly identify these shortcomings of the model and observations, but it is not verified that these factors are actually responsible for the CO and ozone differences in a quantitative sense.

Page 19368, line 18: Rather than (or in addition to) Luo et al. (2009, ACPD), the peer-reviewed and accepted paper Luo et al. (2010, Remote Sensing of Environment 114, 2853-2862) should be cited. It is good to see that the small contribution from the Australian fires is quantified here, confirming that they did not have a large role in the present study.

Figure 9: An alternate approach would be to have one panel for each level (and species) showing the MLS and GEOS-Chem results together. This would make it easier to determine the extent of differences between the model and observations, as alluded to on page 19369.

Page 19371-19372: This was one section of the paper which I found difficult to follow. I think a table summarizing the values would help the reader a lot.

Page 19372: The authors should take a look at Folkins et al. (2006, JGR, 111, D23304), which could provide further insights to their interpretation. It is a comparison of GEOS-3 and GEOS-4 but is still relevant.

Page 19373: The impact of lightning NO_x on ozone is acknowledged here based on

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past studies by noting that its effects are included in the impacts of dynamics, however, no attempt is made in the present work to quantitatively separate the impacts of lightning from the rest of the dynamics. This is good example of a situation where rather than expanding the boundaries of our current understanding, this work stops short and we are left with no new information.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 19357, 2011.

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