

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

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Reply to anonymous referee #2

We would like to thank the Editor and the referee for the thoughtful and insightful comments. We have addressed all of the comments. Our responses are itemized below.

The manuscript presents a comparison of MLS observations of the 2006 Indonesian fire outbreak with a number of GEOS-Chem model simulations designed to investigate the roles of biomass burning and ENSO driven dynamical changes in driving atmospheric composition in the region. This topic has been the subject of a number of modeling and observational studies and as such, I was at first skeptical about the value of another. After a thorough reading, I think there are new contributions warrant-

C10192

ing publication in ACP.

In particular, the focus on the UT using MLS is different from the focus of a number of other studies though I think it would help if the authors include more discussion of why the UT perspective is valuable.

Reply: Indeed. We now include additional discussions.

Also, a very important point that is under emphasized is that the attribution of composition changes to dynamics is highly dependent on the underlying model fields used. This is not true only of this study but of many previous ones. The use of two different sets of meteorology fields helps to bracket the uncertainty in this estimate which is valuable. Additionally, the paper is very well written and easy to follow even though a large number of different simulations are presented. I recommend publication following the minor revisions described below. My main concern is the small amount of attention that changes in lightning are given. The model description states that cloud-top height based flash rate calculations are scaled to match regional satellite climatology values. Is this climatology year dependent (2005 and 2006 reflect different flash rate distributions) or is it a multi-year average? My concern is that if a single climatology is used for both the 2005 and 2006 simulations, differences in flash rate may be suppressed due to the regional scaling. I think some more details on how this is done are needed in the model description.

Reply: Yes. We clarified the lightning source description.

Also, I think another simulation should be added - a 2006 study in which 2005 lightning emissions are used to separate the dynamical and lightning impacts. I do not think this should be very complex, though it would require more work, and it would add a new and exciting dimension to separate this work from the large number of previous studies on the topic.

Reply: Point well taken. We now include additional simulations and discussions for

C10193

precisely that purpose.

Also, I think a plot of the difference in lightning emissions from these simulations is needed to place the results in context.

Reply: Indeed. A plot and discussions are now included.

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