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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interpretation of Aura satellite observations of CO and aerosol index related to the December 2006 Australia fires" *by* M. Luo et al.

Anonymous Referee #2

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This is a review of manuscript Atmos. Chem. Phys. Discuss., 9, 23665–23693, 2009, "Interpretation of Aura satellite observations of CO and aerosol index related to the December 2006 Australia fires" by Luo et al. I find this manuscript needs substantial revisions as outlined below.

General comments:

1) Although I personally agree with the authors that the particular event is most likely due to pyro-convection, and perhaps even pyroCb, I find they did not present any compelling evidence to this effect. Aren't there some additional ground-based or geostationary satellite observations that can pin this down better? The Dirksen et al. paper

is less than totally convincing. I found the presented trajectory analysis also less than convincing. Relying on only a few Hysplit back trajectories is dubious. I would prefer to see either an ensemble of back trajectories from the vicinity of the TES/MLS observations or a dispersion mode run where one could more clearly see the potential source areas. Furthermore, since the locations of the fires are known, why not show some forward trajectories, as well. If there is more direct evidence for the time of the pyroCb event(s), then more precise forward trajectories could be very useful for confirming the source of the downwind TES and MLS CO observations.

2) My second major complaint with the present manuscript is the use of spatially interpolated maps of the rather sparse daily or bi-daily TES and MLS retrievals in Figures 1 and 2. This is very misleading to the un-initiated reader. I can understand this approach for one or two overview maps to show the global distribution, but not for the detailed analysis of the downwind plume between Australia and South America. Figure 7 shows how nice the specific observations could look if the plots were done more carefully. Although, the CloudSat data is really not very useful in this discussion since it is a null result. As such, Figure 7 could be deleted and the results briefly summarized.

3) Given that one of the major conclusions of this investigation is the need for observations with greater spatial coverage, I do not understand why the authors limited there trace gas study to only those from TES and MLS. Although they clearly provide superior vertical resolution, they lack broad horizontal coverage to identify and track the downwind plume. OMI AI shows the extent of the plume, but what about OMI tropospheric ozone? If only Aura instruments were to be used, then why is CloudSat included? And if CloudSat data was examined, then why not other A-Train observations such as from AIRS and MODIS? Couldn't AIRS CO, although much more limited in vertical resolution, provide nearly temporally co-incident broad spatial coverage? What about AIRS cloud retrievals for cloud-top height? What about Aqua MODIS AOD and cloud products? Surely these would help find any pyro or other convection. Other recent studies have utilized the more comprehensive A-Train observations of fire emission

plumes to good effect. Although including these datasets would lengthen the paper, even a cursory discussion of them could greatly strengthen the arguments. To not even mention the other existing satellite observations is a substantial oversight that must be corrected.

4) As Dirksen [2009], Frietas et al. [2007], and others have already shown, chemical transport models need to be specifically modified to more accurately account for the effects of pyroconvection. Thus, it is no surprise that the as presented GEOS-CHEM simulations failed to reproduce the downwind transport from this pyroCb event. The discussion on page 23677 could be shortened considerably to still make the same point. The findings of this study are very important to point out the short-coming of assuming nearly all (90% or more) of fires release all their emissions into the boundary layer. These and other satellite observations provide evidence for direct, rapid transport of biomass burning emissions into the mid-upper troposphere where they can be rapidly transported downwind. Determining the impact of such transport on global air quality or far flung locations will require extensive modeling and compilation of satellite observations over many years. I believe improving the present study to strengthen these conclusions will provide motivation for such extended analyses.

More detailed comments:

Page 23667, line 14: CO is not regulated everywhere on the planet. Page 23668, line 8: Are lightning started fires termed accidental? Or are these natural fires?

Page 23668, lines 19-20: Comparable appears twice in the same sentence.

Figure 1: I would strongly encourage the authors to include additional plots showing more accurate spatial representations of the TES and MLS retrievals for South Pacific Ocean between Australia and South America. The caption for Figure 1 needs to include mention of spatial interpolation of the data to produce a smooth field. Similar comments follow for Figure 2. For example, Figure 7 nicely presents the CloudSat IWC observations in this region. Similar plots of the TES and MLS CO observations would

be more compelling to me than the false impression of more complete coverage given by the spatially interpolated maps.

Page 23672, lines 15-21: MLS does not provide a full tropospheric CO profile, so how can TES averaging kernels be applied to them to produce a synthetic TES observation? Due to the inherently broad vertical smoothing of TES, isn't a pretty full tropospheric profile required for this convolution? If not, then why are the TES 618 hPa retrievals in Figure 2 downwind of Australia so impacted by the upper tropospheric CO?

Page 23673, lines 1-17: Why is no quantified comparison presented for OMI AI vs. TES CO? The authors state that because of TES' narrow track, the comparisons are few, but because of OMI's wide swath and their location on the same satellite, for every TES observation there should be OMI observations. Although OMI AI is related to the AOD, the previous discussion of large OMI AI values being indicative of biomass burning emissions speaks to an expected high degree of correlation between TES CO and OMI AI, regardless of the altitude of the plume. The same pyroCb plume that sends up the CO should also carry a large number of smoke aerosols that OMI sees. Several previous studies have successfully compared MODIS AOD values to MOPITT and AIRS CO. It would be very interesting to see the correlation of TES CO and OMI AI.

Page 23673, line 21: Only the presented satellite data coverage are sparse.

Page 23674, line 1: The locations of the Australian December 2009 fires should have been noted on one or more of the previous maps (MLS, TES or OMI) for a point of reference. Why are only the 8-day > 50 locations shown? These most likely capture the most intense fires, but fire count alone does not guarantee this. The full MODIS fire location data are available; why not show all fires in the region?

Page 23674, line 3: To what does the "respectively" refer? There are four panels in Figure 6, two for 681 hPa and two for 215 hPa.

Page 23674, line 6: “marke”?

Page 23674, line 16: While I agree that generally speaking, shorter trajectories are more believable, a reference is needed to support this assertion. However, there is a huge caveat that this statement greatly depends on the atmospheric conditions encountered by the trajectories, their altitudes, and the model used to generate them. A one day back trajectory could be unreliable if it encounters a frontal zone, deep convection, etc.

Page 23674, lines 5-12: From Figure 7, it looks much like the back trajectory path shown was in largely clear air. Perhaps a MODIS image or two would be helpful to show this more definitively and indicate no strong convection occurred enroute. However, are these CloudsSat tracks all from one day or do they cover the entire time of the back trajectory? The later should be case to give a more complete picture. If they are all from one day, then this figure is very misleading as to the atmospheric conditions experienced by the parcel along the back trajectory.

Page 23675, line 23: Why are Australian EDGAR emissions only scaled to 2002 from 2000? Why aren't they scaled to the observed year of 2006?

Page 23676, line 1: A comment referring back to Figure 2 is required for the first sentence to make sense.

Page 23676, lines 1-13: This paragraph does not make sense. Does it speak to comparisons between Figure 2 and Figure 8? If so, this needs to be clarified. Given the broad vertical sensitivity of TES, I do not think it is fair to say the transport occurred at 681 and 215 hPa, rather, the retrievals indicate transport at both these levels. The changes in the shapes of the mean averaging kernels is more fair to use to describe the difference in the TES CO observations with time. The entire discussion from lines 5-13 seems completely out of place, is superfluous to this study, and should be deleted.

Page 23676, lines 14-16: This sentence is very weak and does not present a con-

vincing argument for pyroCb. Surely the authors can find better references for support such as Kahn et al. 2009 where they found a larger percentage of fire plumes above the boundary layer.

Page 23676, lines 16-18: This sentence is also very week. The Fromm references are on target, but the authors could greatly strengthen their case for this being a pyroCb event and the importance of better modeling of such events.

Page 23677, line 9: Something is missing in this sentence, ”. . .and where it actually been observed.”

Page 23677, line 14: The end of the sentence should read, ” . . .height as in Dirksen et al. (2009).”

Page 23677, line 22: Reference should be to Hyer et al. 2007.

Page 23678, line 1: If the OMI O2:O2 height of the plume is 380 hPa, why is MLS seeing so much CO at 215 hPa above the smoke plume?

Page 23678, lines 3-5: This sentence does not well describe plume self-lofting after injection into the upper troposphere as discussed in the Stammes reference.

Page 23678, line 7: “enhanced chemical tracer”? The placement of “enhanced” seems misplaced.

Page 23678, line 18: The phrase, “The TES enhanced CO retrievals”, implies to me a change from the standard TES retrieval algorithm where the authors really mean the retrievals of CO enhancements. This is a subtle difference, but it could be confusing to some readers.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23665, 2009.