

Responses to referee #2

Anonymous Referee #2

Received and published: 11 January 2010

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.

This comment is similar to Dr. Cooper's main comment regarding the direct evidences for the occurrences of Pyro-convections that uplifted the fire generated pollutants, e.g., CO and aerosol, to the mid-upper troposphere. The sparse data in Aura or CloudSat indicates that these satellite observations did not overpass the locations at the moments that the pyro-convection occurred. Thanks to Dr. Cooper and this referee who pointed out the geostationary satellite observations. We examined Vis and IR images taken by the Japanese MTSAT-1R. We referenced discussions on the analyses by Dr. Andrew Tupper of Australia Bureau of Meteorology and others posted on the Yahoo pyrocb group on the occurrences of pyro-convection events during Nov-Dec 2006 Australia fires.

The HYSPLIT back trajectories were run starting at the locations and times of large TES CO observations (> 120 ppb and 80 ppb) for a given day. There are indeed not many of them. However, these trajectories are directly associated with the enhanced CO observations. We think that readers can easily draw the conclusions without showing another plot with more trajectory lines (forward or backward). The first conclusion is that the air parcels originated in the SE Australia fire area moved (south) westward as shown in Fig 6. The 2nd conclusion that we pointed out is that majority of the air parcels in the upper troposphere were traced back to the fire regions but those in the lower troposphere were traced back in a larger area to the south of the fire regions in a longer time. We feel that adding more air parcels here is not very necessary in providing more support to the discussions. The GEOS-Chem results presented in section 4 provided CO simulations with the similar metrological fields. These are more direct results compared to the trajectory or dispersion plots.

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.

The detailed analysis (with colored footprints of OMI, MLS and TES with aerosol or CO concentrations) example is Fig 5. We tried similar ‘footprint’ plots for Fig 1 & 2 for TES and MLS and find they hardly make sense due to large gaps in the spatial coverage. We decided to use the ‘interpolated’ images for these two figures for illustrations of our discussions on the CO enhancement location comparisons between TES and MLS. We marked locations of actual data points on the image maps and noted in the figure captions. Fig 5 and the detailed analyses describe the actual locations of the CO profiles from three instruments as requested by this reviewer.

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 their 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 A1 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.

Thanks for pointing out that there are more tracer data available for investigating plume transport events such as from Australian fires. We need indeed to mention other satellite observations; especially that MOPITT and AIRS are both providing CO retrievals at better spatial coverage than TES or MLS. The authors have been working with and are very familiar with Aura data, and one of the authors, Dr. J. Jiang, has experiences using CloudSat and CALIPSO data. OMI, TES and MLS are all on a same satellite platform (Aura). It is one of the goals of this paper to present nearly co-located and simultaneous TES and MLS measurements for the CO plumes, for the first time. Since the pollutant plumes in the troposphere vary strongly with time and location, other instruments on different satellite orbits may capture their enhancements at different locations and times from those of Aura. The inclusions of the other observations may add more evidences in tracking the plume. They however, won't change the conclusions. In section 2, we added discussions of the MOPITT CO observations during the Australia fire events and

mentioned, as this reviewer suggested, AIRS CO coverage in the mid-troposphere. We examined MOPITT CO daily maps from the MOPITT website and noticed some enhanced CO plumes over approximately the same general regions as TES and MLS observations captured by MOPITT observations. Ozone is not a good tracer of fire events and its distributions are complicated by chemical processes along the way and the stratospheric source.

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.

We agree with the comments that this reviewer made here. Therefore, this section has been shortened accordingly.

More detailed comments:

Page 23667, line 14: CO is not regulated everywhere on the planet.
This error has been amended in the manuscript.

Page 23668, line 8: Are lightning started fires termed accidental? Or are these natural fires?
This section has been deleted to make the manuscript more concise and relevant to Australian fires.

Page 23668, lines 19-20: Comparable appears twice in the same sentence.
This section has been deleted to make the manuscript more concise and relevant to Australian fires.

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.

As we explained above (in responding to comment 2), we feel that the ‘footprint plots’ with large data gaps do not illustrate the CO enhancement well. Such ‘dot’ plots are provided on the TES website along with the interpolated images. We added the reference (links at http://tes.jpl.nasa.gov/visualization/SCIENCE_PLOTS/TES_L3_Daily.htm) to the TES website and explanations in section 2, paragraph 2. We also added an explanation in the Fig 1 caption as this reviewer suggested.

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?

Yes, in order to produce the pseudo ‘retrieved’ CO profile viewing MLS CO profiles through TES observing system, we need the full CO profile from MLS. MLS team does provide full tropospheric CO profiles with the climatology in levels below 215 hPa. Since TES peak sensitivity is at 400-250hPa, the ‘retrieved value’ of MLS CO at 215 hPa through TES observing system is indeed influenced by the climatology at 400-250hPa. This CO value also contains the information from the truth at 215 hPa. We have emphasized that TES reported CO profile at ~25 levels in the troposphere are not independent from each other (illustrated by TES averaging kernels in Fig 3).

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.

We added discussions of MOPITT and AIRS CO and reported the enhanced CO plumes seen from the MOPITT CO browse images. Thanks to this reviewer for pointing out the missing discussions of Fig 5 on TES/OMI. We added the discussions of TES/MLS/OMI data for latitudes 60S-70S.

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

We appreciate reviewer #2’s suggestion. We changed the sentence to: ‘Since satellite data presented here are sparse in time and location...’.

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?

The MODIS fire counts are shown in Figure 6. It is hard though, to show them overlaid with tracer images in other figures. We added statements indicating the fires are reported and observed in Victoria, Australia by the NASA satellite in descriptions of Fig 1 in the text. In figure 6, the 8-day fire count locations are to mark the approximate locations of intense fires over the region of interest. The trajectories are shown in the figure for the course of a few days. We showed MODIS fire counts for the entire region in Fig 6.

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.

We removed the word ‘respectively.’

Page 23674, line 6: “marke”?

We changed ‘marke’ to ‘marked’ as suggested by reviewer #2.

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.

We agree with this comment. Our analyses are based on the reasoning that the much enhanced mid-upper tropospheric CO observed downwind from the known fire region are most certainly from the fire sources. Fast mixing usually occurs in the boundary layer. We therefore run the trajectories in the upper and lower troposphere to show the differences. The trajectories in the lower troposphere not only took longer time to get back to the longitudes of the fire sources but also to the south of the sources.

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 CloudSat 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.

Figure 7 shows the backward trajectory starting from a TES observation location with high CO and the CloudSat Ice-water contents during the hours of 0 to -24 hr (magenta only for the trajectory). We have examined the plots for -24 to -48, etc. No intersections between the trajectory and the CloudSat orbits were able to be identified. The discussions and the figure caption are probably confusing. We modified both. The modification also referenced comments from other reviewers.

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?

The actual scaling was from 2000 to 2003 (not 2002) and this has been corrected in the text. The scale factor used was developed by van Donkelaar et al. [2008, Atmos. Chem. Phys., 8, 2999–3014], which scales an inventory forward in time by using the distribution from the base year and updated total values for certain countries or regions as available. Proper scaling is not trivial and is dependent on available data for the calculation described. At the time of the original model run, van Donkelaar et al. had only produced scale factors up to the year 2003, although the current version of the model has scale factors up to 2005, but not beyond.

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

We appreciate reviewer #2's suggestion. Therefore, a reference to Fig 2 was added to the manuscript.

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.

Yes, the paragraph discusses the comparisons between GEOS-Chem model simulations of CO (Fig 8) and TES/MLS observations in Fig 2. We re-wrote most of the paragraph in stating the differences between model simulations and satellite observations at lower and mid-upper troposphere respectively.

Page 23676, lines 14-16: This sentence is very weak and does not present a convincing argument for pyroCb. Surely the authors can find better references to support such as Kahn et al. 2009 where they found a larger percentage of fire plumes above the boundary layer.

The Kahn et al. 2008 reference has been added accordingly to reinforce our point in the latter part of the sentence.

Page 23676, lines 16-18: This sentence is also very weak. 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.

In this section the amended manuscript concisely addresses the importance (and the current implementation and adaptation of Freitas et al. 2007's 1-D plume-rise model into GEOS-Chem).

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

We thank reviewer #2 for catching this error; it has been amended accordingly.

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

Although we appreciate reviewer #2's suggestion, we feel that the sentence reads well in its current state.

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

We appreciate reviewer #2 catching this error. The amendment was made and incorporated accordingly in the revised manuscript.

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?

We deleted this section in the paragraph to make section 4 of the manuscript more concise. An active sounding instrument, like Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observation (CALIOP), gives the best results for determining the altitude of an aerosol plume. OMI O₂-O₂ pressures of adjacent clouds provide constraints on the lower-limit of the plume's altitude, but this depends on the atmospheric conditions, especially the optical thickness of the plume. In general, the O₂-O₂ pressure of the plume is most indicative for situations with high Aerosol Absorbing Index (AAI, *i.e.*, ≥ 5) and no clouds. Radiative transfer simulations show that the O₂-O₂ algorithm always underestimates the altitude of the plume. The O₂-O₂ algorithm used in Dirksen et al. (2009) was designed and optimized for cloud retrieval, as illustrated by the employed scattering model (a Lambertian reflector with albedo 0.8). Dirksen et al. (2009) hypothesize that by using a scattering model, which is adjusted to the scattering and absorption properties (*i.e.*, optical thickness, single-scattering albedo) of a smoke aerosol layer sufficiently high optical thickness, will produce better O₂-O₂ retrievals of the pressure of smoke aerosol plumes. An improvement in this matter could potentially cause better congruence between MLS CO and OMI O₂-O₂.

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.

This sentence has been deleted from the manuscript to make this section more concise.

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

We appreciate reviewer #2 catching this error. We amended the manuscript accordingly – we deleted the word 'enhanced.'

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.

As suggested by reviewer #2, we amended our manuscript accordingly.