

This review is by Owen Cooper, co-editor for ACPD/ACP and the editor of this manuscript. I have not yet had the benefit of reading the reviews of the two anonymous referees and their opinions will have a major influence on my decision as to whether the paper will be published in ACP. My current opinion is that the analysis shows some interesting capabilities of the satellites to detect biomass burning outflow from particular biomass burning events, but the paper requires a major revision before it can be considered further. Please find below my list of concerns and suggestions for improving the manuscript.

Sincerely,
Owen Cooper

Thanks Dr Cooper and the two referees for the careful reviews of our paper. We have addressed all the concerned points in this and other responses, and have revised the manuscript and modified some figures to reflect the comments.

Major concerns:

1) One major problem with the paper that requires additional analysis is the assumption that the aerosol and CO features are due to smoke from pyro-convection, even though the satellites did not cross the fire region at the presumed time of pyro-convection. I read the Dirksen et al. paper to find their evidence for pyro-convection and was very surprised to see that their paper offered no firm evidence, they just assumed that pyro-convection occurred. While it appears likely that pyro-convection occurred you need to provide firm evidence. There must have been a lot written about these fires by the scientific community in Australia and you should scan the literature (try the Australian Met.Service) for evidence. A more direct way would be to look at the Japanese geostationary satellite IR images for the fire time period. Pyro-convection should show up as cold cloud tops with brightness temperatures typical of the upper troposphere directly above the fires. NCDC has archived IR images above the Japanese/Australian hemisphere for your time period every 3-hours at 10 km resolution:

<http://www.ncdc.noaa.gov/oa/rsad/isccpb1/index.php> Or if the temporal and spatial resolution of this dataset is not adequate you can try contacting the Australian Met. Service to see if they have the data.

The Dec 2006 Australia fires drew a lot of attention in the media and in the atmospheric science community. A good example is the discussions posted on the PyroCb Yahoo group website, <http://tech.groups.yahoo.com/group/pyrocb/>, where scientists from Australia/US had informally discussed local conditions and satellite data analyses (OMI, MTSAT, CALIPSO etc). The group collectively identified three pyro-convection events, Nov. 29th, Dec. 10th and Dec. 14th, two of which occurred in Victoria and Aura data are shown in this paper. However, except the Dirksen et al. paper that described OMI data, we are not able to find other formally published papers describing the events. Thanks for your suggestions to check the Japanese geostationary satellite IR images. We got access to the Japanese MTSAT-J1 IR or Vis images at NOAA or the Australia Bureau of Metrology websites. The IR images show cloud top temperatures in contrast to the warmer surface temperatures. Unlike images around Nov 29 in NE Australia, we do not see a distinguished 'cold cloud top' spot over Victoria in Dec 14 due to cloud coverage in a large area. We added the discussions on these IR images and summarized/referenced PyroCb group discussions. We also added discussions / references on the evidences of 'cold front' passage

occurrence during the Dec 14th PyroCb event described on the Australian Met Service website (thanks for the suggestion) and in Dirksen et al paper.

2) The Introduction needs a lot of work to improve the style but more importantly to improve the accuracy and relevance of the references. In the first paragraph the discussion switches back and forth between the importance of CO for air quality and for climate change. Please organize the discussion into two paragraphs, one on air quality and one on climate change. [The introduction of the manuscript has been amended accordingly.](#)

page 23667 line 14

When you say that CO emissions are regulated worldwide it gives the impression that all nations are regulating CO which isn't accurate. Instead say the emissions are regulated by many nations worldwide.

[This sentence has been amended accordingly.](#)

page 23667 line 15-16

You discuss the impact of CO on climate change through its reaction with methane and ozone but provide no references. Please see the many recent papers on this topic by A. Fiore (NOAA) and J. West (UNC-Chapel Hill).

[This sentence was amended with references from West et al. 2009a and 2009b.](#)

page 23667 lines 16-17

In addition to Logan (1981) you need a more recent reference that discusses the tropospheric CO budget. Thompson et al. 1994 only look at the impact of convection on CO above the central US and is not an appropriate reference for a global CO budget.

[This sentence was amended with reference from Duncan et al. 2007.](#)

page 23667 line 25-26

You mention the global decrease of CO from 1990 to 2000, but none of the referenced papers address global CO over this full 10-year period (most only discuss just the early 1990s while Parrish et al only discusses US emissions). Please find recent references to support your claim.

[This sentence was amended with reference from Duncan et al. 2007.](#)

page 23667 line 27-28

You discuss the link between climate change and increase in biomass burning but as far as I can tell the link has not yet been shown, at least by the papers referenced. What does IPCC 2007 say? For example Wotawa et al 2001 state in the first sentence of the abstract that they found no clear trend in the 1990s. Van der Werf et al. 2007 show no trend in global biomass burning from 1997-2004. Wouldn't Yurganov 2008 be a better reference than Yurganov 2004? And what about Van der Werf et al 2008? If you want to keep this discussion in the conclusions please list in your response to my comments the

papers that show a link between increased biomass burning and climate change along with a brief summary of their conclusions.

This sentence was amended with appropriate references.

While it is appropriate for you to discuss the general importance of biomass burning on global atmospheric chemistry and climate, your paper focuses just on Australian fires so the discussion of Northern hemisphere, European and Indonesian biomass burning is not relevant. Please delete the text from page 23668 line 3 to page 23669 line 6.

This section was deleted all-together.

3) Please clearly state the primary goal of this analysis at the beginning of the last paragraph of the Introduction. It would be helpful if you began with the sentence: "The primary goal of this study is to demonstrate....." then state whether it is to

- show that GEOS-Chem needs a pyro-convection scheme
- or to demonstrate that TES and MLS are capable of detecting relatively small features like the outflow from pyro-convection.
- or to argue that greater spatial coverage by satellites is required

Then go on to state how your study differs from that of Dirksen et al (2009) who use satellite retrievals to study the same event.

Thanks for the specific suggestions on describing the goals of this paper. The primary goals of this paper are indeed to introduce TES/MLS CO observations that Dirksen et al had not included, to show the limitations of the GEOS-Chem model without a pyro-convection scheme, and the needs for vertical, spatial and temporal resolutions in tracer retrievals in future satellite observations. We have modified the paragraph to closely follow the suggested structure.

4) I'm not clear on the use of the GEOS-Chem plume-rise model. If it is still under development and has not yet been fully implemented, how do you know that it shows that the aerosols reached the upper troposphere?

Adapting and incorporating a 1-D plume-rise model [Freitas et al., 2007] into GEOS-Chem should simulate PryoCb events. The process of adapting and incorporating the 1-D plume-rise model into GEOS-Chem is presently taking place. Dirksen et al. 2009 stated that they conducted preliminary results with an adapted version of the 1-D plume-rise model [Freitas et al., 2007], where their results suggest that aerosols emitted from large fires on Dec. 14th, 2006 reach the UT (~300 hPa); Dirksen et al. 2009 do not give a reason why their results suggest that aerosols reach the UT. Yet, given that Freitas et al. 2007 embeds a subgrid plume model in a 3-D CTM and obtained good results for CO distributions in regions close to the source, via parameterization of vegetation type (to estimate the heat flux and information on the fire size to calculate the buoyancy flux), the Dirksen et al. 2009 preliminary investigation, using the adapted 1-D plume-rise model, is likely parameterized in a similar fashion as Freitas et al. 2007.

5) An important conclusion of your study is that the current spatial and temporal coverage of the satellites is inadequate for fully tracking these types of events. You say the next generation of satellites needs better coverage, but if your paper is going to have any concrete influence on the design of these satellites you need to give a specific recommendation of what the minimum spatial and temporal coverage should be.

Since the next generation satellites for tropospheric chemistry have already been considered by US (NRC Decadal Survey 2007) and European/Asian countries, we modified the conclusion as "The new generation of satellite instruments for tropospheric chemistry, e.g., on a geo-stationary platform (ref), should provide improved vertical resolution of tracer profiles, better spatial and temporal coverage, and opportunities to capture the convection moments via their IR or Vis/UV spectra in the small pixel-footprints defined for urban air pollution events". We do not specify the specific numbers for the spatial and temporal resolution assuming they'll be driven by the broad goals of monitoring the air pollution events for air quality forecasts.

Minor comments:

If no explanation is given for a comment, please insert the suggested text into the appropriate place in the manuscript

page 23670 line 8 presumably caused pyro-convection
The 'word' presumably has been included in this sentence.

page 23670 line 12 upper troposphere
This sentence has been amended accordingly.

page 23671 line 22 It would be helpful to place these high CO mixing ratios in the context of other studies that have measured biomass burning CO in the upper troposphere. This will give the reader an idea of whether the event is typical or anomalous. A good comparison would be:

Extreme CO concentrations in the upper troposphere over northeast Asia in June 2003 from the in situ MOZAIC aircraft data

Author(s): Nedelec P, Thouret V, Brioude J, et al.

Source: GEOPHYSICAL RESEARCH LETTERS Volume: 32 Issue: 14 Article Number: L14807 Published: JUL 20 2005

Thanks for pointing out the reference paper. We've added this reference and quoted 300-800 ppb CO at 8 km measured by the MOZAIC aircrafts in spring/summer 2003 over boreal fire regions around Lake Baikal.

Figure 1

Please increase the line width of the continent outlines.

We amended the manuscript accordingly.

Figure 2

The panels are still too small and the maps outlines are difficult to see. Please enlarge each panel by 50% and make the map outlines in gray.

The figure currently fits the whole page, but we changed the outlines to gray to make them more visible.

Figure 5

Please increase the font of the latitude and longitude labels and of the colorbar labels. Also most of the retrievals, especially the low values are very faint and hard to see.

Instead of making the colors fade out to white, please use a color scale, like a rainbow in which all mixing ratios show up clearly even when they are low.

Done. We tried rainbow color scales and found they do not show CO enhancements well when comparing to OMI AI. We adjusted the min/max values for MLS and TES CO plots. Now they illustrate the CO enhancements in our discussions much better.