Response to Reviewer 2 for Manuscript acp-2009-513

Title: Smoke injection heights from fires in North America: Analysis of 5 years of satellite observations

Date: January 14, 2010

We thank the anonymous reviewer for her/his thorough evaluation and constructive recommendations for improving this manuscript. Her/his comments (*in italics*) and our responses are listed below.

Comments:

The manuscript by Val Martin et al. presents an analysis of the injection heights from fires in North America based on 5 years of satellite observations. Recent publications have pointed to the sensitivity of model results to the injection heights of fire emissions, which is still a major uncertainty and one of the limiting factors in our understanding of the impact of fire emission on the atmosphere. In this respect, the manuscript provides novel, important information on a very relevant topic and is well suited for publication in ACP. The manuscript is in general well written. Sometimes the terminology and the distinction between smoke plumes and smoke clouds seems to be intermingled. The data analysis and interpretation of the results is comprehensive, sound and clear. I only have some minor comments that should be considered by the authors before publication in ACP.

We appreciate the favorable comments on the paper.

Specific Comments:

- *Page 20516: line 1: I suggest to specify the years that are considered in the 'multiyear record' already in the abstract.*

  Changed as suggested.

- *Page 20516, line 23: add 'm' between '1800' and 'thickness'.*

  Added as indicated.

- *Page 20517, line 22: Only three of the four fires used in Lavoue et al. 2000 were 'experimental fires', the fourth one was a wildfire. Please modify the sentence accordingly.*

  Modified as suggested.

- *Page 20519, line 25f: You mention that no plumes were digitized in 2003. To justify the omission of 2003 in the analysis you state that the 'goal of this work was to develop a 5-year climatology, and 2003 had a much less active fire season'. However, from a*

climatological perspective, the less active fire seasons should also be considered to derive a climatology. Please comment and potentially revise the argumentation.

We agree with the reviewer that less active fire seasons should be included in the analysis, and these fire seasons were in fact included. We should have been clearer about this. To pick the years other than 2004, we examined burned area statistics for Canada, Alaska, and the contiguous U.S.; 2004 was digitized first because of the interest in the record fires in Alaska/the Yukon.

High fire years in one of the regions are usually a low fire year in another. Thus, by picking high fire years in Alaska (2004 and 2005), Canada (2002 and 2004), and the contiguous U.S. (2006, 2007), we included very low fire years in Alaska (2006, 2007) and the contiguous U.S. (2004), near average fire years in the contiguous U.S. (2002, 2005) and relatively low fire years in Canada (2005 and 2007). Year 2003, which we omitted, was also a low fire year in Alaska (similar to 2007 and higher than 2006), in Canada it was similar to 2005, but not as low as 2007, and in the contiguous U.S., 2003 had over twice the area burned in 2004. We have broadened this discussion to include the information about low fire years, and the text in Section 2.1 reads now:

> We chose the years of 2002 and 2004-2007 since they were high fire years (in terms of area burned) in Alaska (2004 and 2005), Canada (2002 and 2004), and the contiguous US (2006 and 2007). These years included very low fire years in Alaska (2006 and 2007) and the contiguous US (2004), relatively low fire years in Canada (2005 and 2007), and near average fire years in the contiguous US (2002 and 2005).

- *Page 20520, line 2: Please add 'of the US' after 'in the lower 48 states'.*

  We replaced the term "lower 48 states" with "contiguous US" to be consistent throughout the manuscript.

- *Page 20525, line 1: In my opinion, the terminology of 'assimilated meteorological observations' to describe the results from the GEOS model is misleading, because it suggests that they are mainly based on observations. In my view, a more accurate description of the data would be 'We used the results from the data assimilation scheme from the GEOS model: : : '. Please consider changing the wording.*

  Rephrased as suggested. On a side note, the purpose of assimilation is to use a GCM to assimilate meteorological observations, a standard procedure at GMAO and ECMWF.

- *Page 20525, formula 1: The index of S on the left side of the equation is weird (i + 1- i), just using 'i' (or 'j' to distinguish this index from the index of the model layers) as index seems appropriate to me.*

We modified Equation 1 to make this definition clearer. The equation and text read now:

$$S_j = \frac{\theta_{i+1} - \theta_i}{z_{i+1} - z_i},$$

where S_j is the stability at the mid-point of the model levels (*i* and *i+1*), and θ and z are the potential temperature and altitude respectively at the model levels.

- *Figure 5: You state that the thin black lines represent the central 95 % of the data for each season, i.e., 5 % of the data lay outside of the range indicated by these lines. However, for fall 2002, it seems that there are no plume heights larger than the range given by the thin black line, so it seems that in this case the range indicated by the thin line has a different meaning. Please comment.*

  We thank the reviewer for this comment. The reviewer is correct to note that the thin black lines should represent the central 95% of the data for each season. We reviewed the IDL routine that was used to calculate the percentiles and found a bug. This bug only affected the percentiles above the 50th percentile calculated for small datasets, such as Fall 2002. We fixed the bug and updated all the figures in the paper that reported percentiles, i.e., Figures 5, 6, 9, 12, 13 and 15; changes in the results are indeed negligible.

- *Page 20528, line 9: The maximum plume height in Figure 5 is more than 7000 m in 2006, and not 5000 m as stated in the manuscript with reference to Figure 6. However, I guess that you are referring to smoke plumes and do not include smoke clouds here, but this is not explicitly stated here. Please be more specific on smoke plumes vs smoke clouds and maybe also refer to the maximum smoke cloud height of 7000 m as shown in Figure 5.*

  The reviewer is correct pointing that we report a maximum plume height of 5000 m based on the smoke plume dataset shown in Figure 6. To make the distinction between smoke plume and cloud heights clearer, we added the following clarifications in the text:

  Section 3
  Figure 5 presents an overview of the five years of MISR smoke plume and cloud heights.

  Section 3.1
  Figure 6 displays the distribution of median heights above the terrain for smoke plumes in eight biomes for the five years of the study […]

- *Figure 6: Please indicate in the figure caption whether smoke clouds are considered in this figure.*

  Added as indicated. The caption reads now:

  > Distribution of the median height above the terrain for smoke plumes in each biome [..]

- *Page 20531, line 24ff: Can you give an estimate of the minimum aerosol optical depth of a smoke cloud to be detected by the software operator? I guess it does depend on numerous factors including the underlying surface, but maybe one can give a rough estimate.*

  The reviewer is correct in noting that sensitivity to aerosol optical depth of smoke clouds depends on numerous factors, including the underlying surface. The key to retrieving stereo-heights from any features relies on seeing contrasts that can be tracked across multiple view angles. This contrast depends on the spatially varying albedo, optical depth, and possibly shadowing of the smoke cloud itself, along with brightness differences between the (non-uniform) smoke and the surface.

  AOD values for the smoke clouds located over land are typically 0.5 (median), with a minimum value of 0.06. However, MISR AOD retrievals in the MINX database are obtained from the MISR Standard aerosol product retrieval regions, which are 17.6 km, about the same scale as many smoke clouds and plumes. Also, MISR cannot obtain a total column optical depth unless multiple cameras can see through the atmosphere to the ground; this precludes aerosol optical depth (but certainly not aerosol height) retrievals over the thickest parts of many smoke plumes [Kahn et al., JGR 2007]. As a result, the aerosol retrievals tend to represent less optically dense smoke near a cloud, and not the cloud core itself that was identified and digitized for plume height by the software operator. We chose not to include the minimum aerosol optical depth of the smoke clouds in the manuscript since a more detailed study needs to be performed before any AOD value can be discussed.