

Interactive comment on “Smoke injection heights from fires in North America: analysis of 5 years of satellite observations” by M. Val Martin et al.

Anonymous Referee #2

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Review of Val Martin et al., 2009

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

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have some minor comments that should be considered by the authors before publication in ACP.

Specific Comments:

- page 20516: line 1: I suggest to specify the years that are considered in the ‘multi-year record’ already in the abstract.
- Page 20516, line 23: add ‘m’ between ‘1800’ and ‘thickness’.
- 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.
- Page 20519, line 25f: You mention that no plumes were digitized in 2003. To justify the omission of 2003 in the your 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.
- Page 20520, line 2: Please add ‘of the US’ after ‘in the lower 48 states’.
- 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.
- 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.
- 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.

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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.

- 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.

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

- 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.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 20515, 2009.