

Interactive comment on “Microwave Limb Sounder observations of biomass-burning products from the Australian bush fires of February 2009” by H. C. Pumphrey et al.

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Pumphrey et al. report on measurements made by the Aura Microwave Limb Sounder that clearly indicate the injection of biomass burning gases deeply into the stratosphere in the wake of a historic bush fire blowup in Victoria Australia in February 2009. The MLS data are both remarkable and robust; the nature of the reported anomaly is unprecedented in the satellite data record. This is indeed a perfect subject for an ACP paper. The authors have done an excellent job of characterizing the MLS measurements and a very good job of writing a coherent paper. As such, this effort deserves to be published in ACP.

However, there are issues with this report that need to be addressed before publication. My assessment of these issues is that they are substantial, but not major. In other words, the main

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points made by the authors are conceded. But, there are details that are not sufficiently presented for the manuscript to be published in its current form. I will list these, with suggestions, and follow with minor/technical criticisms.

My convention below consists of an abbreviation for manuscript page number ("P") and Line number ("L") followed by the criticism.

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**Substantial Issues**

P3, L8. The claim that this is the first such event in the Aura MLS record might be disputable based on comments made elsewhere in this review. In particular, there is some evidence in Figure 11 that there is a stratospheric signal of biomass burning gases in December 2006. The authors are encouraged to examine this period more closely to determine if another stratospheric injection event was detected.

P4, L6. ? “...obvious problems...”? This statement almost begs to be amplified upon. The reader would benefit from a few words of clarification here.

Fig 2 discussion. It appears that as time goes on, the number of UTLS pressure surfaces with anomalous CO is reduced. Some discussion of this is warranted.

P5, L20. For the “unaffected points”, it is not obvious where in the world the largest CO values are and thus how representative these data points are for the time series. Some additional clarification is needed.

P5, L23. This is actually a very interesting and provocative finding (that there were no systematic changes in H<sub>2</sub>O, O<sub>3</sub>, etc. in the stratospheric biomass burning plume). I'd suggest the authors consider a statement about the data quality of those constituents to give the reader additional content with which to contemplate the chemistry of these unusual stratospheric plumes.

P6, L12. This statement needs a reference. I don't think it can be assumed that the reader community knows what is and what is not a biomass burning emission. Perhaps referring to a review paper on biomass burning emissions, and noting that ClO is not listed, would be something for the authors to consider.

Fig 7. My interpretation of the top panel is that there was a plume enhancement at all fre-

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quencies between 8000 and 15000 Mhz except for a narrow range centered on the frequency nearest Band 2. I.e. there is positive delta-Tb everywhere but there. Doesn't that suggest that plume constituents are having an effect wherever the delta Tb is non-zero? I suggest more clarification as to how to interpret the full frequency span that is shown in the top panel.

P7, L12. Earlier in the paper it is mentioned that there was no effect on water but here it states that the plume was "considerably wetter." A better characterization of the effect of the plume on water vapor is needed.

P9, L24. Winker et al. (2010) is cited regarding CALIPSO observations of the Black Saturday plume but that paper has no such discussion. Also, the acronyms for the instruments called out here should probably be expanded to show the full name, since they are not already defined.

Figure 11. These two plots are much too cluttered for the viewer to understand. Seven different lines, often occupying the same space, are nearly impossible to distinguish, even with the different colors and styles used. I'd suggest breaking each panel into 2, one for the 3 highest pressure surfaces (which are primarily all upper tropospheric) and the other for 100, 68, and 46 hPa. I'd ask the authors to consider dropping 31 hPa because it has almost no variability. If it is to be kept, perhaps make it a gray shade to distinguish it from all the others. One reason to offer the suggestion for an upper tropospheric and lower stratospheric breakout is that it would give the reader an opportunity to see how the tropical biomass burning, which (according to the text) is seasonal and extends only to the UT, compares with the episodic stratospheric pollution. The other reason is to put a sharper focus on the authors' claim that Black Saturday was a unique event. I see some hints in the figure that a smaller, but real, stratospheric perturbation took place in late 2006. Indeed there was another noteworthy pyroconvective scenario in Australia in November and December 2006.

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Minor/Technical Issues

P2, end of Section 2.1. The finding that the plume is ascending should be mentioned in the abstract.

P4, L14. A 1-sentence paragraph starts this section. It is not indented and looks awkward.

P4, L25. Mention that Fig 2 shows 6 UTLS pressure surfaces, to refine the description.

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Fig 5. The legend in the bottom panel hides an important part of the plotted data.

Fig 7. What determines the frequency resolution of the measurements in top panel? The continuous line and wiggles suggest a high frequency sampling.

Fig 7. Measurements and calculations are reported on altitude surfaces, not pressure... inconsistent with other figs and discussion. Also, the spacing is smaller than the quoted $\sim .7\text{ km}$ resolution. Please explain.

Fig 7. What is meant by "Intermediate"

Several figures. The "/" character is used to separate a label from units. This can be confused with a division symbol. Suggestion: use parentheses or brackets around units.

P7, L12. To what is the reference "latter effect?"

Fig 8. The altitude grid in the top panel is inconsistent with the lower panels.

Fig 9 and10. Add "UTC" to qualify the times stated.

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