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## ***Interactive comment on “Characterization of organic ambient aerosol during MIRAGE 2006 on three platforms” by S. Gilardoni et al.***

**Anonymous Referee #2**

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### **General Comments**

This paper describes aerosol measurements and compares the results in the urban area, at a boundary site and in the outflow. As such, it is a valuable contribution to the understanding of aerosol behavior and to the goals of the MILAGRO campaign. I recommend publication subject to comments below. In general, the goal is to identify aerosol processing and aging. To what extent is this realized? Could more be said in the discussion about this?

### **Specific Comments**

1. The discussion of meteorology and transport is somewhat scattered and vague. The research flights were designed to intersect the plume, so a separation of

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- north and south may not be the most meaningful (page 6627, line 28). Between (Fast et al., 2007, Doran et al., 2007, Shaw et al., 2007, de Foy et al., 2008 and de Foy et al., 2009, Baumgardner et al., 2009, Tie et al., 2009) there is enough information to be more specific about the plume evolution.
2. pg 6629, line 26: This paragraph is rather vague, but touches on a hot-button issue. It would be good to link it with the results from Johnson et al., 2006 which you cite just before in a different context, and Moffet et al., 2008 both of whom saw V/Ni plumes. Furthermore, Rivera et al., 2009 and de Foy et al., 2007 discuss transport from this source. (Minor point: Fig. 5 could have the days more clearly labeled to be able to compare transport events with other results, the caption could mention that this is for SIMAT and a legend would make it easier for the reader).
  3. pg 6633, line 21: It seems that this is a crucial point, that should be given more rigorous treatment. You are saying that there is extra oxidation in the urban plume, which is an important finding. The following paragraph, about the C130 measurements, seems to be in disagreement. There it says that the OM/OC ratio is nearly as high as at SIMAT - but wouldn't you expect it to be higher if the plume is more oxidized? This should also be discussed in relation to Fig. 3, where COOH fraction is lower in the C130 measurements than the others. The discussion follows by saying that the data are in agreement with meteorological analysis, but could you not go an extra step to see when the C130 was downwind, and how far downwind it was? On this point, what about data from when the C130 intersected the urban plume further away from the MCMA? This whole discussion could then be related to other work about urban plume oxidation (eg. Tie et al., 2007 and others) and be placed on a solid footing.
  4. As with the previous comment, this section could / should be much more forceful. Is the difference between the ratio (0.10 vs. 0.11) statistically significant? The

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C130 did intercept the plume further away, would this be more comparable to the Maine and Nova Scotia situation? Carbon monoxide is a useful tracer for mobile sources, with episodic impacts from other combustion processes, this paper could be more specific about what the message is (pg 6635, line 1-3). Do the VOC studies provide any relevant information on the discussion of the carbonyl and aliphatic groups? What about the CCN activity? This is proposed at the beginning, but not returned too in light of the measurements.

5. Expanding and/or refining the discussion along the research questions that you outline will lead to a clearer and stronger conclusion, for example the last sentence (pg 6636, line 8-11) could be expanded. pg 6635, line 16: to what extent does this suggest regional pollution or a well-mixed air mass in the basin?

### Minor Comments

1. pg 6620, line 23: You may like to add Grutter et al., 2008 and de Foy et al., 2007 with regard to the volcano emissions to include existing work on MILAGRO and MCMA-2003.
2. pg 6622, line 13: Please clarify: it seems you have 12-h at Altzomoni and 8-h at SIMAT. Is the 24-h in addition to these at both sites? line 15: "This time period ..." would clarify the justification for 11 - 6 sampling. The current wording suggests that you modified the sampling according to boundary layer height?
3. In the revised version, the references to Rappenglueck et al., 2007, should be updated with Baumgardner et al., ACP-MILAGRO special issue. The discussion could then be enhanced about transport to/from Altzomoni, at the moment it is rather vague (eg. pg 6622 line 2).
4. pg 6621, line 21: Please check the coordinates of SIMAT (Velasco et al., 2009).

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- pg 6628, line 7: Please add La Merced to Fig 1b. There was a sonic anemometer at SIMAT, why not use this data instead?
5. MIRAGE is not defined.
  6. Fig. 7: It would be better to show a zoom of the basin and surroundings, and to identify features of relevance to this paper. See for example Crounse et al., 2009.
  7. pg 6627, line 25: It should be clarified in the text that one volcano is extinct while the other is passively degassing. Note that the extinct volcano seemed to have more fires on its slope (Crounse et al., 2009).
  8. Section 3.4: the first sentence contains a superfluous “and” but should be reworded anyway.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 6617, 2009.

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