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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evolution of anthropogenic pollution at the top of the regional mixed layer in the central Mexico plateau" *by* D. Baumgardner et al.

D. Baumgardner et al.

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The authors would like to express their appreciation for the careful critique submitted by the reviewers of our paper and the many helpful comments and suggestions that were made to improve and clarify our presentation. Our responses are given below in the order that the reviews were received; however, given that all three reviewers shared a common concern, we will address this first.

The original intent of the study, as described in the original paper, was to describe the properties of the regional mixed layer as they related to the likely origin of the air. A case study approach was taken, similar to that used to evaluate aircraft measurements, in order to study specific characteristics that could be attributed to the history of the air

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masses. We selected cases where the air mass trajectories could be indisputably separated into three general directions, east, southeast and southwest over 24 hour periods. That being said, rather than try to convince the reviewers of the soundness of our reasoning and because of the strong sentiment expressed against this case study approach, especially that of Reviewer 1, we have instead reanalyzed the measurements finding 14 days where neither local fires or clouds and precipitation would likely alter the characteristics of the boundary layer. We have been able to use a combination of 700 mb and 650 mb meteorological fields to separate the data into three classes, i.e. air masses coming from the east, SW and WNW in order to demonstrate convincingly that the properties of the mixed layer are sensitive to the sources of the air within the region.

Secondly, all the reviewers thought that our conclusions regarding organic material coming from biomass or wood burning was stated too strongly with insufficient data to support this. We agree and have changed the text to show the importance of SOA in correspondence with the new analysis and conclusions.

As additional papers relevant to our study were published after we had submitted our manuscript, we now have augmented the presentation with reference to some of the results from these newer submissions. Finally, as suggested by several of the reviewers, we have changed the title to more succinctly reflect the content of the manuscript. The new title is <>Physical and Chemical Properties of the Mixed Layer in the Region of the Central Mexico Megapolis <>

Note: Reviewers’ comments are sometimes paraphrased or shortened, Our responses are in <>

Response to Reviewer 3

<>We would like to thank the reviewer for the thoughtful comments and suggestions that were made. We have tried to address each of the questions and concerns and implemented those suggestions that were still relevant after reanalyzing the data and

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revising the text. <>

Two main points are called out in the abstract and conclusions: 1) Mexico City is not the only source of pollutants in the area, and 2) biomass and wood burning are the main sources of organic particulate matter. There are serious problems with both of these assertions. While I don’t disagree with the 1st point and agree that the site certainly experienced polluted air masses from the non-Mexico City locations, it would certainly help to have shown data from air masses originating from Mexico City for comparison. If no such air masses were observed or if the origin of suspected air masses is uncertain, this should be stated. Why are only 3 days of data used? It is fine to focus on select days, but it would help if the reader were given a feel for the rest of the dataset. It seems a reach to state that <>This mixture rapidly erases the signature of a unique Mexico City <>plume<>; and suggests that the environmental impact of this region should be considered as one that stems from a large area source rather than a single megacity.<> To make the above claim it would seem necessary to consider data that actually looked at the Mexico City plume, likely from various distances until the distinct <>Mexico City signature<> was no longer evident. The above statement is not well supported by 3 days of data in which the air originated from other locations. Consideration of emission inventories would also be helpful, as would more information on Puebla and Cuernevaca (e.g., population, etc) <>This has now been addressed by evaluating 14 days, two of which had air coming from Mexico City.<> The 2nd argument (about biomass and wood burning) is based on a comparison of the observed CO/OM ratios with literature values for diesel, biomass, and wood burning. This analysis appears flawed for the following reasons: a) it ignores the fact that the CO/OM ratios from urban areas, diesel exhaust and from wood burning decrease with photochemical aging (Kleinman et al, 2008, Greishop et al, 2008, Robinson et al, 2007, deCarlo et al, 2008). This was already partially described in this manuscript, and yet ignored in this part of the analysis! <>This argument has been removed.<> b) It ignores gasoline exhaust; which is the major source of CO in most urban areas and has a much higher CO/OM ratio than diesel exhaust. <>This whole section has been re-

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structured and biomass burning is no longer emphasized as a major contribution to the organic mass.<> The earlier discussion of OM and OM/CO, especially as compared to other locations (Salcedo et al, Kleinman et al, etc), is a strong point of this paper (along w/ figure 15), as is the description of the meteorology. There is other data presented in the paper (e.g., FTIR analysis of the filter samples) that is extremely interesting but only briefly discussed. For example, what could be the cause of the differences in the organic composition shown in figure 11? <>There are now more intercomparisons between the Altzomoni results and those from the ground sites and airborne platforms plus the FTIR results have a more succinct explanation for the observed differences.<> This manuscript would benefit greatly from more in depth discussion of the data presented in figures 11, 13, 14, and 15. Figure 16 and the accompanying discussion should be removed from this paper (as described earlier). <><>Done<><> Figure 17 and the accompanying text (BC and CO) could be removed too. <>Done<> More of the time series data (beyond the 3 days examined) would certainly be welcome and very appropriate for ACP. <>Done<> Specific comments: Abstract: <>highest concentrations were from the east<>. This should be clarified in the abstract; is this from Mexico City or not? Explicitly stating that Altzomoni is southeast of Mexico City would be helpful. <>Done<> Is this statement about high concentrations from the east only based on the 3 days evaluated or the entire dataset? <>Changed to reflect 14 days of analysis.<> Background section: I found the background interesting, but it does not prepare the reader for the rest of the manuscript well. It does not set the framework for the discussion of the relative importance of pollution sources (Mexico City vs non-Mexicocity), nor does it discuss the relative importance of the various organic PM sources (anthropogenic, wood/biomass burning, etc) - a topic for which there is not currently a consensus among researchers. The revision, which will hopefully focus on the more concrete findings of the study, should have a background/intro section that places the findings in context. <>Done<>. The whole introduction has been rewritten to reflect the reviewer’s comments. Fig 1, though interesting, is not necessary and could be described solely in the text. <>Done<> table 1: does the

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accuracy column refer to a 1 sigma or 2 sigma uncertainty? <>One sigma. Clarified in the table<> How is OM measured? Is this organic aerosol (OA) from the AMS? This needs to be explicity definied in the experimental section, since OM is an ambiguous term - does it include gases and PM? <>Organic PM<> or <>Organic aerosol<> are preferred terms. <>We no longer discuss OM but POM, a term our AMS expert, James Allan prefers.<> More information should be provided regarding the operation of the AMS - how were the 2 calibrations performed? Did they agree? Collection efficiency?. <>Done<> pg 3279: that O3 is lower in Mexico City compared to a hill side site is likely also due to the proximity and amount of NO emissions in Mexico City. <>Agreed<> The discussion of OM/CO (and similar ratios) are a strong point of the paper. These ratios should be calculated after subtracting the background OM and CO values; it is not clear if this was <>Done<>. These calculations need to be better defined. <>Done<> pg 3281: line 13: <>The mass concentrations of OM and inorganic compounds do not show a significant decrease in concentration as related to the possible origins of the air masses.<> - meaning is unclear. <>Removed<> pg 3281, lines 22 - 25: the comparison of the concentations of O3 and PM between Mexico City and Altzomoni is interesting, but the analysis fizzles with: <>From this weconclude that O3 and particles at the Altzomoni site are not only coming from the primary emissions, but there are additional sources that offset the dilution as the RMLgrows.<> It is well known that O3 is only formed as a secondary pollutant. It is also well established that particulate matter has both primary and secondary sources. These facts should form the starting point (rather than the conclusions) for a discussion of the observations. <>Rewritten<> pg 3282: the conversion factor of 1.8 to convert from OM to Organic Carbon is based onprevious results (Takegawa et al 2005) that might not be applicable to the air massesobserved at Altzomoni. A discussion of whether this factor is applicable is needed. In fact, Takegawa et al (2005) quotes studies that have shown OM/OC ratios in the range 1.2 to 2.1. <>No longer a part of the analysis<> The recent ACPD publication by deGouw et al (Atmos. Chem. Phys.Discuss., 8, 21265 - 21312, 2008), which was not yet published when this manuscript was submitted may contain useful

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data (though this would not be necessary if this section is removed). <>This section was removed<> Technical corrections: O3 is in ppb, CO in ppm; why not use ppb for both?. <>Done<> Abstract, line 12: <>25OE10E-3<>; needs units (both in abstract and later in the body). Shouldn’t this be 25 x 10E3? (without the minus sign) <>Abstract has been rewritten to reflect new analysis.<> Abstract line 15: Insert the word <>aerosol<> as indicated: <>The mass concentration of OM in the RML was greater than 70% of the total aerosol mass, regardless of the air mass origin.<> Rewriting as <>OM accounted for more than 70% of the total aerosol mass<> would be better still. <>Abstract has been rewritten to reflect new analysis<> Abstract, line 12: <>maximum CO at <> should instead read <> maximum CO concentration<> or <>maximum [CO]<> <>Corrected throughout the paper.<> figure 3 caption has typos: it appears that Mexico City and Cuernavaca are northwest and southwest of Mexico City (not northeast and southwest as written in the caption) <>Corrected.<> pg 3271, line 7: should read <>west of Puebla<> (not east) <>Corrected.<> fig 2 shows very interesting data. It would be easier to examine if there weren’t separate axes for the 2 locations. Just use one common axis. <>Corrected.<> pg 3280: line 29: need units for OM/CO <>Corrected.<> pg 3274: best to avoid contractions in technical writing (<>does not<> instead of <>doesn’t<>) <>Corrected.<>

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 3265, 2009.