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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Applications of Lagrangian dispersion modeling to the analysis of changes in the specific absorption of elemental carbon" *by* J. C. Doran et al.

J. C. Doran et al.

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Re: acpd-2007-0421

General comments

We were pleased that the referees found the material interesting and the presentation clear. Their comments were valuable; we have considered each one carefully and hope that we have been able to address all of them satisfactorily. Specific responses are given below.

We note that since the original submission of the manuscript to ACPD we have obtained some updated emissions inventory data that affect the simulated values of EC

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produced by biomass burning. As a result, quantities such as median values of specific absorption have changed slightly, and the new values are given in the revised manuscript. The analysis and conclusions are similar to those using the original inventories, however, so that the reviewers' comments and our responses are directly relevant to the current version.

Response to Anonymous Referee #1.

1. We were reluctant to put in too much information on the MILAGRO experiment's goals as a whole because we anticipate that one or more detailed articles will appear that will provide considerable detail, especially in light of the special issue of ACP that is being developed. However, we have added a few more sentences that place the work described in this paper in the context of the larger campaign's objectives.

2. We really didn’t know just what to expect concerning changes in absorption between T1 and T2. Previous studies of the rate at which soot becomes coated have given different "time constants", ranging from a few hours to a day or more. In the former case, measurable absorption changes between T1 and T2 would be expected; in the latter they would be hard to detect. We have added material to the text explaining this.

3. We have compared our recent results to related ones obtained by Barnard et al. in the MCMA-2003 experiment. We are unaware of similar efforts to measure absorption in the IMADA campaign.

4. We have added indicators of the 25th and 75th percentile values in both Figures 8 and 10, as suggested. Results of statistical significance tests of the information presented in Table 2 are given in the manuscript text.

5. We have added shading to indicate 25th and 75th percentile values of the particle ages at the T1 and T2 sites.

6. The purpose of the figure showing the wind comparisons is to demonstrate that

ACPD

7, S8460–S8464, 2008

the results without assimilation are generally quite good, and that the inclusion of the data to be assimilated produces only modest changes in most instances so the model is not "shocked" by the inclusion. In view of the scarcity of upper air observations we really couldn’t separate the data into assimilation and verification sets as the reviewer (and we) would have preferred.

7. Minor comments: a) We have fixed the run-on sentence. b) The "30-m" has been fixed. c) The spelling of Volkamer has been corrected. d) While diurnal is the more commonly used term, diel is actually more precise. Diel refers to a 24-h period; diurnal is often used to refer to the same period but can also mean occurring in the daytime. e) The caption has been fixed.

Response to D. Baumgardner

1. We have added a figure showing time series of predicted and measured CO (and EC) at T1. The simulation captures much of the temporal variation of the CO, although the magnitudes are underestimated by about 25%. This suggests that the transport and dispersion in the model are handled reasonably well. The simulated EC time variation is also reasonable but the underestimation is greater.

We are unable to compare EC-CO relationships at T0, T1, and T2. CO was not successfully measured at T2. CO data were available at T0 but the only EC data we have available from T0 were obtained from an aethelometer and we are not comfortable comparing our PAS values of EC with aethelometer-derived values without a direct comparison of the two instruments. As an alternative, we have added another figure that gives a CO-EC scatter plot for two categories. The first category is for periods when the computed MCMA fraction of the EC at T1 was greater than the median value of 0.55 and the second was for periods when the MCMA fraction was less than the median. There is no obvious difference in the behavior for the two categories, nor was there when we looked at periods corresponding to 25th and 75th percentile values of MCMA fraction. We speculate that this can be attributed to the variation of the EC-CO

ACPD
7, S8460–S8464, 2008

relationship from different parts of the city and different biomass sources, all of which would affect T1 more than at T0 or other locations in the city heavily influenced by local sources. A discussion has been added in the text.

2. We appreciate the suggestion that has been made regarding a possible explanation of the difference between our values of specific absorption and those found earlier by Baumgardner et al. (2002). We have included that information in the revised text and have acknowledged the source.

3. We have included the additional paper in our literature survey in the Introduction section of our paper.

Response to Anonymous Referee #3.

1. We have added an additional figure and more discussion of the uncertainties in the emissions inventories. We note in particular that the emissions inventories available to us appear to underestimate the amount of EC in the region, and this no doubt contributes to the poorer performance of the model at T2 and T1 already mentioned in the text.

2. The reported value of 9 mˆ2/g is at T1, as noted in the text. We further note that this is a value extrapolated from 870 nm to 550 nm. It is only when we compare our values of specific absorption to those obtained by other investigators do we do such an extrapolation. In contrast, the values depicted in the figures and in Table 2 are the measured ones at a wavelength of 870 nm. We have added an additional sentence in the introduction calling attention to the fact that our 870 nm values need to be extrapolated before they should be compared with other values. When this is borne in mind, the difficulty of reconciling our results in Figure 8 (which is Figure 10 in the revised manuscript) and Table 2 with the extrapolated (at 550 nm) value of 9 mˆ2/g goes away.

While the original Figure 7 in our previous work had a scale error, the temporal trends

ACPD

7, S8460–S8464, 2008

were basically correct. Thus, rather than adding an additional figure showing the day-to-day variation of αABS, we have added 25th and 75th percentile bars in Figure 8 that indicate the range of values found in this quantity at T1.

3. One of the reviewers (Baumgardner) has suggested that the difference in reported values is more likely to be due to a difference in locations rather than measurement techniques, and we have now incorporated his comments in the revised manuscript. We have also added additional material citing previous comparisons of measurement methods. As now explained in the text, a relevant study (Slowik et al. 2007) indicates that the use of a PAS and a Single Particle Soot Photometer, the instrument used by Baumgardner et al. (2007), would not be expected to produce a large bias. Unfortunately, we have not found similar comparison studies that include the column-averaged values of αABS reported by Barnard et al.

4. Table 2 is for T1 only, and this is now mentioned explicitly in the caption.

5. The lower values of αABS shown in Figure 8 (new Figure 10) are for 870 nm, as explained in item 2 above. The value of 7.5 mˆ2/g noted by the reviewer is for 550 nm.

6. Bars indicating 25th and 75th percentile values have been added to both Figure 8 and 10 (new Figures 10 and 12, respectively).

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ACPD

7, S8460–S8464, 2008