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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A review of biomass burning emissions, part II: Intensive physical properties of biomass burning particles" *by* J. S. Reid et al.

J. S. Reid et al.

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Response to interactive comment for acpd-2004-0078:

I appreciate the reviewer's comments and thorough review. These certainly could have been generated after a very thorough read, and I have updated the paper substantially in response. I am particularly grateful for instances where the reviewer has pointed out overlooked references. My response to these comments are listed below.

On the DMA versus the OPC of smoke particle research: The reviewer is of coarse correct, stating that if we are going to favor the DMA over OPCs for some smoke work we should note some of the more significant issues with DMA type systems. They also state that part of the discrepancy that we find between OPCs and DMAs may be due to a CNC memory artifact discussed by Collins (2002). This is certainly true

for the recently developed fast scan systems. However, the only papers cited in this paper with DMA data are those associated with the University of Washington aircraft that used a slow scan off of a sampling bag, and hence the issues discussed in Collins do not apply. Certainly the statement in the paper "If available and properly employed, differential mobility analyzer data is probably of the most value." in the paper holds and includes all papers such as Collins. In the confines of this paper, we are not in a position to outline all sampling corrections for each variation of an instrument, but the point is well taken.

On Modeling Studies. I agree with the reviewer completely. Originally the Trentmann and Jost papers were to be discussed in more detail in Part 1 of this series, but I have added further discussion to this manuscript.

On sampling strategies: As we point out in the manuscript, there have been a few attempts at Lagrangian studies. These are notoriously difficult to implement. Similarly, monitoring a fires lifetime would require significant effort and many flight hours. Such a study would also be prone to sampling bias. The question posed by the reviewer is a good one: Based on the insight from this review, what would be good strategy for future measurements. In the context of the modeling work discussed above, we agree with the reviewer that coupled model/measurement strategy is a strategy, as we now say in the manuscript. However, the reviewer states this as an "either/or" to receptor modeling, which we don't entirely agree with. When one considers the huge ensemble of quite variable fires to a handful of semi-Lagrangian cases, the issue of scale bias immediately comes into play. Hence, both are necessary programs. Best bet, a combined plume/receptor analysis program.

Title, on the definition of "intensive." While the reviewer gives one definition of intensive, our usage is borrowed from physics, where "Intensive" is defined as a quantity that applies to any subdivision of the set. By the scientific vernacular, "Intensive physical properties" or "Intensive variables" describe the inherent proprieties of the aerosol particles, such as size or chemical mass fraction. Its converse, or "extensive variables" are

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those that describe the entire magnitude of a set, such as the mass or number concentration, vertical distribution, and total mass flux. The use of the word “Microphysical” properties in the title is inappropriate because these include optical parameters such as mass scattering efficiency and asymmetry parameter. As these are discussed in another paper, we make the distinction between “intensive physical” and “intensive optical properties.”

5156, II 7-9: Why were the results of Dubovik et al., (2002) and Eck et al., (2003) teh only inversion studies included? The current situation is a result of us splitting the original paper into Parts II and III, as well as an overall reduction in paper length (at the request of earlier reviewers). Because we did not want to repeat ourselves, we saved the bulk of the discussion of these inversions for our companion paper on smoke particle optical properties. In the context of Part II, we are emphasizing consistency in data sets. As we show in Part III, the milieu of isolated inversions studies (each with their own methods on typically isolated smoke events with sparse validation) is extremely difficult to interpret. In the context of Part II, we feel they would confuse the issue.

Misc. language: Corrected.

Table 2-4: Corrected.

Table 5. The table clearly states “mass percentages.”

Table 6: Corrected.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 5135, 2004.

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Interactive Comment