Atmos. Chem. Phys. Discuss., 4, S2276–S2281, 2004
www.atmos-chem-phys.org/acpd/4/S2276/

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.

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Received and published: 1 November 2004

The authors give a very comprehensive summary of the state of knowledge on microphysical properties of particles generated by biomass burning. The review encompasses grass and forest fuel types in both tropical and temperate environments and covers particle formation, size distribution, morphology, chemistry, condensation nucleus properties, and aging processes, each with a discussion of uncertainties based on the measurement techniques used. It also summarizes the discussion by giving emission factors and an outlook on future research. There are only a few points which I would like to comment on.

When discussing data on particle size distributions, the authors favor measurements done with a Differential Mobility Analyzer (DMA) over those done with Optical Particle

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Counters (OPCs). They justify this preference by stating that OPC measurements of large particle concentrations are hampered by coincidence issues and that OPC data interpretation requires an assumption on the particle refractive index (in fact also on particle morphology). While this assessment is certainly true, DMA measurements have their own systematic uncertainties. The flows through a DMA have to be monitored accurately to prevent systematic shifts of the size distribution. Inversion of DMA data has to take into account particle diffusion and multiply charged particles. Most importantly, when operating a DMA in scanning mode, the memory effect of the Condensation Particle Counter (CPC) commonly used in a DMA setup has to be corrected for. Otherwise, this effect leads to artificial broadening of the features in the particle size distribution, especially for fast scanning speeds which are often used on aircraft platforms. Since this effect has been addressed only recently [Collins et al.(2002)], many of the data sets quoted in this article might be influenced by this artificial broadening. In fact, the systematic difference between OPC and DMA in the width of the accumulation mode of the observed biomass burning aerosols might be due to this artifact. To conclude, since both methods have their systematic uncertainties, the authors might want to simply state these uncertainties and weigh data obtained with OPCs and DMAs equally in their analysis.

I strongly agree with the authors that models, especially mesoscale gas and particle phase models, should be used to interpret the available data. The first attempts in this direction [Trentmann et al.(2003), Jost et al.(2003)] should be quoted and briefly discussed. Models could also be used to narrow down the complexity of the problem by providing suitable parameterizations, e.g. as a function of fuel type, burning stage, and size of the fire.

The authors state that the evolution of the emissions of a single fire over the fire's life span is not well known. The same seems to be true for quantifying the relative strength of the aging processes involved. It might be worthwhile to elaborate briefly how this insight could be used to improve measurement strategies for future experiments. For

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example, rather than surveying a large area affected by fire plumes, the emissions from a single fire could be monitored over the whole lifetime of the fire, as well as the evolution of the particles in the plume by following it in a quasi-Lagrangian way. This strategy could make use of transport models whose predictive qualities improved considerably over the past years.

Although the article is very readable, the language lacks precision in a few places which are named below.

Title: The term *intensive* as used in the title is not explained in the article. The term originally refers to a property of a statistical ensemble that does not scale with the number of elements in the ensemble. With respect to physical aerosol properties, this could refer to microphysical as well as optical aerosol properties. To avoid confusion, the authors might want to consider replacing the term *intensive physical* in the title with *microphysical*.

p. 5138, l. 9: The phrase *as commonly known* should be replaced by a reference.

p. 5142, ll. 18 – 25: This passage on the relative value of particle size distribution data obtained by DMA and OPC should be rephrased in the light of the discussion above.

p. 5149, ll. 13 – 15: Rather than just stating so, it should be said why the increase of the organic carbon mass fraction with increase of smoldering combustion is logical.

p. 5153, l. 13: The expression *biomass burning sized particle* is rather diffuse and should be rendered more precisely.

p. 5154, ll. 19 – 20: The expression *for smoke particles in the size range of smoke ...* is tautological and should be rephrased.

p. 5154, l. 25: *Prupacher* should be *Pruppacher*.

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p. 5156, ll. 7 – 9: Why are only the results of Dubovik et al. (2002) and Eck et al. (2003) included?

p. 5158, ll. 23 – 24: Do the stated hygroscopic growth factors refer to the particle diameter or to the particle scattering coefficient?

p. 5160: At the end of the first paragraph, it is stated that coagulation is the significant growth mechanism for local hazes. At the beginning of the next paragraph, it is said that gas-to-particle conversion can be as important as coagulation. Obviously, these statements refer to different regimes and time scales. When discussing the relative significance of the aging processes, the authors should always state clearly which regime and time scale they are refering to.

p. 5161: The second paragraph on this page should be rephrased in the light of the above discussion on systematic uncertainties of OPC and DMA measurements.

p. 5161, ll. 16 – 17: The quotation Reid et al. (1998a) should probably refer to Reid et al. (1998b).

p. 5163, l. 26: *... shrink in mass substantially due out-gassing ...* should read *... shrink in mass substantially due to out-gassing ...*.

p. 5167, l. 19: *it well known that* should be *it is well known that*.

p. 5167, l. 22: See discussion of term *intensive* for title.

p. 5172, ll. 28 – 29: *... , one manuscript may report, say, ion chromatography ...* seems to be a little colloquial.

p. 5175: The symbol r_{eff} used on this page should be defined somewhere in the article.

p. 5177, l. 7: *Boren* should be *Bohren*.

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p. 5183, l. 3: *Kuhler* should be *Köhler*.

Table 1: The abbreviations *Presc.* and *Temp.* are not used in the table. There are no bibliography entries corresponding to Martins et al. (1997) and Hobbs et al. (1997).

Table 2: The reference to Mazeurek et al. (1991) probably contains a typo. The expression $< 8 >$ should be explained.

Table 3: There are no bibliography entries corresponding to Ferek et al. (1997) (3 times) and Zarate et al. (2000). The abbreviation *N/A* or *na* should be used consistently. A unit should be stated for the numbers given in the table. The line corresponding to *Andreae et al. (1998)* does not contain data. It should be filled with data or removed. The abbreviation *bdl* should be explained.

Table 4: The abbreviation *IMP* explained in the caption is not used in the table. The references to Reid et al. (1998) (4 times) are ambiguous. The data of [Fiebig et al.(2003)] and [Formenti et al.(2002)] should be included in this table and also discussed in the text.

Table 5: A unit should be stated for the numbers given in the table. The typesetting of this table should be checked since data and corresponding references are not in the same lines.

Table 6: The abbreviation *MCE* should be explained. The reference to Andreas et al. (1991) probably contains a typo. There are no bibliography entries corresponding to Ward et al. (1991) (2 times) and Hobbs et al. (1997).

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References

[Collins et al.(2002)] Collins, D. R., Flagan, R. C., Seinfeld, J. H., 2002. Improved inversion of scanning DMA data. Aerosol Sci. Technol. 36 (1), 1 – 9.

[Fiebig et al.(2003)] Fiebig, M., Stohl, A., Wendisch, M., Eckhardt, S., Petzold, A., June 2003. Dependence of solar radiative forcing of forest fire aerosol on aging and state of mixture. Atmos. Chem. Phys. 3, 881 – 891.

[Formenti et al.(2002)] Formenti, P., Reiner, T., Sprung, D., Andreae, M. O., Wendisch, M., Wex, H., Kindred, D., Dewey, K., Kent, J., Tzortziou, M., Vasaras, A., Zerefos, C., November 2002. STAAARTE-MED 1998 summer airborne measurements over the Aegean Sea: 1. aerosol particles and trace gases. J. Geophys. Res. 107 (D21), DOI: 10.1029/2001JD001337.

[Jost et al.(2003)] Jost, C., Trentmann, J., Sprung, D., Andreae, M. O., McQuaid, J. B., Barjat, H., March 2003. Trace gas chemistry in a young biomass burning plume over Namibia: Observations and model simulations. J. Geophys. Res. 108 (D13), 8482.

[Trentmann et al.(2003)] Trentmann, J., Andreae, M. O., Graf, H.-F., January 2003. Chemical processes in a young biomass-burning plume. J. Geophys. Res. 108 (D22), 4705.

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